STUDIES IN ETHICAL RISK-TAKING

by

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Abstract

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Rettig's Behavior Prediction Scale, a sixteen-item measure of predictive ethical risk-taking was used to explore two issues relating to the viability of a combined Value/Relevant Arguments interpretation of choice shifts on hypothetical life dilemma problems. The Value/Relevant Arguments explantion of shift behavior along the cautious to risky dimension emphasizes that intensive consideration of item details, as would occur during group discussion, would render salient a set of widely held values which would then determine the dominant direction (towards riskier or more cautious solutions) of the ensuing discussion. It has been demonstrated in support of this interpretation that both the direction and magnitude of shift on a given choice dilemma item can be predicted on the basis of the proportion of pro-risky to pro-cautious arguments generated during group discussion.

The Value/Relevant Arguments interpretation has yet to receive strong confirmation, however, when applied to studies in which arguments are generated and considered by individual subjects outside of the group context. Studies bearing on this issue have been pursued in relation to the Familiarization Hypothesis, which consistent with the Relevant Arguments approach, holds that choice shifts occur as a consequence of further familiarity with item contents - through further private study or through discussion of item-relevant details - and independently of any intrinsic group dynamic.

The first objective of the present study was to systematically explore those variables which, based on an intensive review of the Familiarization literature, seemed to be related to the occurrence or nonoccurrence of choice shifts among subjects privately generating and considering their own arguments. The literature review suggested that procedural complications, most notably with respect to establishing an appropriate task orientation in privately responding subjects, may have interfered with objective attempts to assess the effects of familiarization. The following variables were selected for systematic study: Timing of Remeasurement in relation to familiarization (Interpolated vs. Postponed); Salience of Pretest (Available vs. Unavailable during familiarization and remeasurement) and Familiarization (thought sampling task either Related or Unrelated to BPS items).

A second issue relating to the viability of a Value/Relevant Arguments interpretation concerned the role of ethical values in producing the well-documented shift to gain emphasis on the BPS. If the salience of relevant values (here, rightness vs. wrongness) underlies the shift to gain emphasis (i.e., heightened discrimination between high and low values of RVgn, one of four built-in scale components), then under those experimental conditions associated with increased emphasis on RVgn in predictive behavior, there should be a parallel tendency to judge the behavior under high RVgn as less wrong than under low RVgn.

Accordingly two separate 2x2x2 factorial experiments, with independent variables as specified above, and differing with respect to the dependent variable (Behavior Prediction vs. Ethical Judgment) were run.

The data analysis of Behavior Predictions showed that none of the critical comparisons for RVgn reached statistical significance. However the trends suggested by inspection of mean values indicated that the Timing and Salience manipulations operated in the expected direction, with Postponed Remeasurement producing more change than Interpolated; Low Pretest Salience showing more change than High Salience; and Postponed, Low Salience combination producing greater pre-post change than Interpolated, High Salience. Contrary to expectation, these trends were similar for subjects who completed a filler task (Unrelated Familiarization) prior to remeasurement. Additionally, the data offer little justification for assuming that Behavior Predictions and Ethical Judgments are related kinds of decisions, at least when made by nongroup-exposed subjects.

The possibility that procedural details in and of themselves may have accounted for earlier found "familiarization effects" is considered, as are areas for productive further inquiry.

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For over a decade the topic of hypothetical risk-taking has generated an impressive amount of experimentation (c.f. research reviews by Burnstein, 1969; Cartwright, 1971; Clark, 1971; Dion, Baron and Miller, 1970; Myers and Lamm, 1976; Pruitt, 1971a, 1971b; and Vinokur, 1971) and critical comment (c.f. Brown, 1974; Cartwright, 1973; and Smith, 1972). Of particular interest is the characteristic pattern of choice shifts along the cautious-to-risky dimension following group discussion of hypothetical dilemmas. Two kinds of life dilemma scales have been used in these studies: the Choice Dilemma Questionnaire and the Behavior Prediction Scale. The Choice Dilemma Questionnaire (CDQ) consists of twelve items, each depicting a person in a hypothetical dilemma over which of two possible courses of action to take. In each item, one course of action is both more attractive and of lower probability of attainment than the alternative course of action. For example, item 1 (Kogan and Wallach, 1967) describes the plight of Mr. A, an electrical engineer and family man who must decide whether to stay with his present job which offers a modest salary but considerable security and old-age benefits, or to take a job at a higher salary with a smaller, less stable company, which offers less certain security but possibility of advancement if the company survives its competition. The subject is instructed to imagine himself in the place of the individual described in each dilemma, and to indicate on a list of odds choices (1 in 10, 3 in 10, 5 in 10, 7 in 10, 9 in 10 or not under any odds) the lowest probability of success he would require before advising that the less certain but more attractive course of action be attempted.

The Behavior Prediction Scale (BPS) focuses on hypothetical risk-taking situations where ethical concerns are engaged. The scale consists of 16 items sharing the common theme of a bank teller in conflict over whether to take some money from the bank which employs him. The situational context of the contemplated behavior changes from item to item. Specifically, high and low values of four theoretical components of "predictive ethical risk-taking" are systematically varied in all possible combinations. These components are:

1. Reinforcement values of gain (RVgn): high - the money is needed for a crucial medical operation; low - the money is needed by other people.

2. Expectancy of gain (Egn): high - the medical operation was guaranteed to cure the illness, the money obtainable would help many people; low - the success of the operation was not guaranteed, the money obtainable would help only very few people.

3. Negative reinforcement value of censure (RVcs): high - the theft would result in expulsion from the bank and a charge of criminal conduct; low - the theft would be settled in private with the bank president.

4. Expectancy of censure (Ecs): high - the theft would be detected; low - the theft would go unnoticed (Rettig,1966, p. 631).

The subject is instructed to read through each item and to predict on a 7-point scale (from "definitely no" to "definitely yes") whether or not the bank teller will take the money. Although the standard instructions accompanying the scale tell subjects not to be concerned with how wrong it would be to take the money in formulating their predictions, post-experimental interviews show that subjects are in fact concerned with the ethical implications of the behavior. Additionally, attempts to explain characteristic findings (e.g., Rettig and Rawson, 1967) implicitly assume that awareness of the unethical nature of the behavior is a critical factor in the decision-making process.

For both the CDQ and BPS, overall scores are computed as a means of measuring hypothetical risk-taking. Additionally, for the BPS, the relative influence of each of the four built-in scale components can be assessed, in order to provide further insight into the critical situational factors underlying predictive behavior. The contribution of each component can be assessed as follows:

> The structure of the BPS permits the use of matched pairs of items to represent a given component. Items are paired postexperimentally on the basis of the difference in the two levels of the remaining components. Thus, eight pairs of matched items are obtained for one component. Similarly, each of the remaining components is also represented by eight pairs of rematched items. Hence, the four components are represented by four different sets of item pairs, with each set containing eight pairs of items.
>
> An S's score on a component is obtained by summing the difference in his judgments across the eight post-experimentally obtained pairs of items representing that component. It can be shown mathematically that the four component difference scores ($\bar{d}$ scores) are obtained from the original 16 items in an orthogonal (and linearly independent) fashion. (Rettig,1972,p.221.)

It is interesting to note from an historical perspective that both the CDQ and BPS were originally used to study the risk-taking behavior of individual subjects. Kogan and Wallach (1964) developed the CDQ to study the cognitive processes and personality dynamics underlying decision-making. Rettig and his colleagues (e.g., Rettig and Rawson, 1963) were initially seeking to study the determinants of unethical conduct among individuals.

The direction of subsequent research was markedly changed following reports by Marquis (1962) and Brown (1965) of Stoner's (1961) findings of individual to group differences in overall scores on the CDQ. Stoner ran six subjects at a time, having them first respond to the CDQ items privately. The subjects were then brought together for group discussion and instructed

to reach a unanimous decision on each item. The consensus-derived odds choices, when summarized in terms of overall scores, were shown to be significantly lower- and hence riskier-- than the privately arrived at decisions of the same subjects. The "risky-shift phenomenon" quickly became the focal point of subsequent research using the CDQ. At about the same time, Rettig and his colleagues (e.g., Rettig,1966) demonstrated with a between-subjects design individual to group differences using the BPS, which were interpreted as indicating greater riskiness on the part of group-exposed subjects. Hence, the "risky-shift" became the focus of subsequent research with this scale as well. The theoretical controversy which thus ensued can best be clarified by treating each of the scales individually. In the discussion to follow, therefore, findings and theorizing relevant to the CDQ will be treated first, followed by a more detailed accounting of issues relevant to the BPS. The discussion will then proceed to a full elaboration of the present study.

From Stoner's work onward researchers using the CDQ have employed a standard paradigm in which subjects respond individually to a pretest, then are formed into small groups to discuss each item, and finally select individually the preferred odds choice for each dilemma. The discussion-to consensus requirement used by Stoner and other early researchers was dropped following Wallach and Kogan's (1965) demonstration that consensus is not a necessary precondition for the occurrence of choice shifts.

As previously noted, researchers initially focussed on overall scores in analyzing group vs. individual differences. Since overall scores were shown to be reliably lower and hence riskier following

discussion, many researchers conceptualized the phenomenon in terms of group-induced shifts to risk and so sought to explain "why groups make riskier decisions than individuals". This proved to be a too hastily drawn generalization when greater attention was subsequently given to the contribution of individual items to the overall scores. It was then shown that each item contributes differently to the overall score, with most items shifting in the risky direction, but still others reliably shifting in the cautious direction (c.f. Cartwright, 1971). It was thus seen that the so-called "risky-shift" phenomenon would be more appropriately construed in terms of "choice shifts" along the risky-to-cautious dimension (e.g.,Cartwright,1973;Pruitt,1971 a).

A satisfactory explanation of the above effects would seemingly have to account for two kinds of behavior: first, the differential odds choice behavior of nongroup-exposed vs. group-exposed subjects; and second, the differential "behavior" of individual items - i.e., that certain items characteristically produce shifts to risk, while certain others produced shifts to caution.

With respect to individual vs. group differences in odds choice behavior, most explanations have centered on either the dynamics of the group interaction, or the item-specific informational content of the group discussion. The former family of explanations would emphasize the role of a specific group-based process, such as diffusion of responsibility (Kogan and Wallach, Kogan, and Burt,1967), or social comparison (Brown,1965; Jellison and Riskind, 1970) in producing choice shifts. The latter set of explanations would focus on the arguments raised pro and con the various choice alternatives during the course of the discussion (Burnstein et. al.,1971; Burnstein,Vinokur and Trope,1973; Teger and Pruitt,1967).

A third set of hypotheses relates more specifically to item behavioral differences and could be seen as complementing either of the above. These concern the role of "widely held values" in mediating choice shifts. That is, whether one endorses a dynamics-of-interaction interpretation, or exchange of item-content-relevant information as being critical to explaining choice shifts, a mechanism is still needed to explain why specific items "behave" so predictably. Value theory would hold that what happens, either as a consequence of interaction or discussion is that widely held values are rendered salient and that these in turn mediate choice shifts. One kind of value theory (e.g., Brown,1965) would emphasize the "cultural value" of risk or caution inherent in each CDQ item. The odds choice selected by a subject reflects his or her attempt to express the cultural value. According to Brown (1965), when the subject first completes the CDQ individually, he or she endorses a position perceived to be slightly more risky or cautious than would be chosen by peers - in accord with the cultural value. When the subjects then come together as a group, some may learn that his or her choices were not so risky or cautious as initially believed. Accordingly, the subject revises his or her initial position in the direction of greater risk on those items embodying the cultural value towards risk, or towards greater caution on those items embodying the cultural value towards caution. The principal evidence in support of Brown's formulation stems from the so-called "self-other" studies in which subjects complete the CDQ items twice, first responding for themselves, and next responding as they perceive others like them would answer (c.f. Myers,1973). This procedure typically yields more extreme odds choices (in the direction of greater risk or greater caution)

for the "self" condition than for the "other" condition. These findings are seen as consistent with Brown's formulation in that subjects are shown to see themselves as manifesting the relevant cultural value to a greater degree than their peers. However, it has subsequently become apparent that self-other studies have a serious flaw. That is that reliable order effects have been demonstrated, such that the findings discussed above occur only when the subject responds for "self" first and "other" second. If the order is reversed, so too is the observed effect (McCauley, Kogan and Teger, 1971;Myers,1974). Moreover, implicit in Brown's formulation is the assumption that choice shifts occur as a consequence of social comparison. However, in studies where group members are permitted to exchange information about their odds choices, but no actual discussion of item contents is permitted, the characteristic shift effect is shown to be attenuated (e.g.,Clark and Willems,1969; Teger and Pruitt, 1967; Burnstein,Vinokur and Pichevin,1974).

An alternate version of Value Theory that has proved more viable (c.f. Pruitt and Cosentini,1975) was presented by Stoner (1968). Stoner's formulation acknowledges that there may be general cultural values toward risk and caution, but emphasizes item-specific values which become salient during the decision-making process. These item-specific values may thus deal with dimensions other than risk or caution, such as the value of earning a lot of money, or the value of personal happiness, which would be assumed to be widely held. Such item-specific values would guide the selection of a riskier or more cautious alternative, depending upon which would best serve the salient value. The principal evidence for this interpretation stems from a rather ambitious study by Stoner (1968) himself.

To study the influence of widely held values on risk preferences, Stoner prepared a 12-item Life Dilemmas questionnaire consisting of six "risky" and six "cautious" items. Four of the risky items were taken from the CDQ, while two of the cautious items had previously been developed by Nordhoy (1962), and so were known empirically to produce the appropriate shift following group discussion. The remaining six items were especially created for the study. The selection of items was explained as follows:

> The formulation of new life-situation items and the choice of old ones was designed to provide items which engage widely held values favoring one course of action over the other. Thus, an attempt was made to juxtapose values on each item in such a way that most of the subjects would consider one of the engaged values to be more important than the other one associated with the problem (Stoner,1968,p.449).

A "value-ranking instrument" was prepared as a means of independently assessing the relative importance of the differing values associated with each the risky and cautious alternative for a given item. For example, one of the risky items taken from Wallach and Kogan concerns a college senior who must choose for his Ph.D. studies between a high prestige university where the program might be too rigorous for him to succeed, and a less prestigious institution where he is certain to complete his degree. The value-relevant phrases drawn from this item were: "avoiding the risk of failure in your career" and "obtaining the best training available for your particular career". A list of 18 such phrases was prepared for subject ranking.

The study required three separate contacts with subjects over a five to six-week period. On first contact, subjects completed the value ranking instrument, unaware that this procedure was related to the rest

of the study. Two weeks later, subjects completed the Life Dilemma questionnaire twice, using the "self-other" procedure discussed above. This latter procedure was used as a second indicator of value orientations, following Brown's previous work. Three weeks later the subjects were brought together for group discussion of the items.

On the basis of responses to the value-ranking instrument, each of the items was classified as risky or cautious. More specifically, an item was classified as "risky" if most subjects ranked the value associated with the risky alternative as more important than the value associated with the cautious alternative; and if most subjects ranked the value associated with the cautious alternative as more important than the value associated with the risky alternative, the item was classified as "cautious". The thus-drived clasification of risky and cautious items was shown to conform well with the original classification of items. Additionally, subjects made initially riskier decisions for the risk items than for the cautious items. Moreover, when the data was broken down still further it was shown that for the risky items, those subjects who preferred the value associated with the risky alternative made initially riskier odds choices than those who did not show a preference; and for the cautious items, those who preferred the value associated with the cautious alternative chose higher (more cautious) odds than those who did not show a preference.

Comparison of the self-other responses showed that most subjects made riskier decisions for themselves on the risky items and more cautious decisions for themselves on the cautious items than when they were estimating the responses of others. Thus, both measures relating values to risk preferences supported Stoner's contention that subjects

record such preferences on the basis of widely held values engaged by the items.

Group discussion produced significant risky shifts for all six risky items and cautious shifts for four of the cautious items, two of which were significant. In accounting for group-induced choice shifts, Stoner considered both the social comparison explanation of Brown, as well as the relevant-arguments explanation of Nordhoy, whom he quotes as follows:

> In the group, the impact of values which are commonly accepted in the culture to which the subjects belong will be reinforced. The members will express opinions, and also accept arguments more readily which are concordant with these accepted values.(Stoner,1968, p.444).

Over the past five years, there has been a growing tendency to favor a combined Value/Relevant Arguments explanation of choice shifts. Recent evidence appears to lend strong support to the assumption that intensive consideration of item details, as would occur during group discussion, renders salient a set of commonly held values which would then determine the dominant direction (towards riskier or more cautious solutions) of the ensuing discussion.

It has been shown in a number of recent studies that both the direction and magnitude of the shift on a given item can be predicted on the basis of the proportion of pro-risky to pro-caution arguments generated. For example, Silverthorne(1971) found that there was a substantial proportion of arguments in the direction of ultimate shift for both "risky" and "cautious" items (12.5 to 1 for risky items, and 6 to 1 for cautious items). Myers and Bishop (1971) found, on the average, a 3 to 1 proportion of arguments favoring the direction of

shift on an item. Moreover these researchers found that across all discussions, there was a positive correlation between the proportion of arguments favoring the dominant direction and the magnitude of shift in that direction. In a similar vein, Ebbesen and Bowers (1974) found a correlation of .83 between average shift and discussion. Additionally, Morgan and Aram (1975) found a significant relationship between average shift on an item and the proportion of argumentation in the direction of the shift for both risky and cautious items. Each of the foregoing was based on content analyses of "Naturally" occurring group discussions.

Further, at least two studies (Ebbesen and Bowers, 1974; Vinokur and Burnstein,1974) have shown that when individual subjects are exposed to arguments previously generated in group discussion, the tend to shift their pretest choices in the direction advocated by the arguments presented. When these findings are considered in conjunction with those position that choice shifts follow the balance of arguments generated and considered. Moreover, since the same items characteristically produce a preponderance of either risky or cautious argumentation in study after study, the above findings are also consistent with that version of Value Theory which holds that each item evokes a set of widely held values that serve to influence the direction of discussion on that item.

While combined Value/Relevant Arguments Theory has thus received ample support when applied to studies of group-based argumentation, it has yet to receive strong confirmation when applied to studies in which

arguments are generated and considered outside of the group context by individual subjects. Studies bearing on this issue have been pursued in relation to the Familiarization Hypothesis, which consistent with the relevant arguments approach, holds that choice shifts occur as a consequence of further familiarity with item contents - through further private study or through discussion of item-relevant details - and independently of any intrinsic group dynamic. The inconclusive supportive evidence for the Familiarization Hypothesis is interpreted by some to mean that group exposure is not only a sufficient, but a necessary precondition for the occurrence of choice shifts. A review of the relevant literature can be seen as supporting an alternate interpretation, however: namely that procedural complications have interfered with objective attempts to test for a familiarization effect among nongroup-exposed subjects. A critical comparison between those studies which demonstrate an effect and those which do not reveal systematic procedural differences having to do with instructional set induced in subjects, timing of the posttest in relation to the familiarization task, and salience of the pretest during familiarization and postmeasurement. Such differences may well be responsible for the discrepant findings, bearing as they do on the kinds of inferences a subject might draw about the "true" purpose of the experiment and the "meaning" of his responses.

It has been acknowledged for some time (e.g.,Rosensweig,1933), at least at the level of rhetoric, that experiments with human subjects are hardly analogous to experiments with chemicals in a test tube. As Silverman and Shulman (1970) note:

> We are coming to full awareness that the analogy holds only with the profound qualification that we are inevitably working with an unclean test tube, contaminated by all the needs, anxieties, self-deceptions and intentions of someone who is aware that his behavior is being scrutinized as part of a psychological experiment (p.98).

The point is reiterated here because of its seeming relevance to the Familiarization research. For while the specific Familiarization task is similar from study to study, generally entailing the noting of arguments pro and con the choice alternatives, the method of introducing subjects to the task and of inducing the critical selection of odds choices on pretest and posttest differ. The differing demand characteristics (Orne, 1962) which may in this way be created, as well as the degree to which subject evaluation apprehension (Rosenberg,1969) may be alternately minimized or heightened would seem to be potentially important confounding factors in assessing the effectiveness of familiarization for generating choice shifts.

When, for example, the subject is exposed to the hypothetical dilemmas for the first time, it is quite conceivable that he is not entirely certain from the instructions about the purpose of the experiment. Will his responses be used to tell the experimenter something about the questionnaire items, or will they be used to reveal something about him as a person? The greater the lack of clarity the subject experiences as to the true purpose of the experiment, the more likely it would seem that his reactions to the stimulus items would reflect his concern about self-presentation (such as appearing sensible, or reasonable, or perhaps ethical) rather than what he regards to be the appropriate "rational" response based on item contents.

Exposure to group discussion seems to orient subjects to the essential item details, as Ebbesen and Bowers (1974) have noted:

> Group discussions provide the opportunity for an individual to observe both his own and other people's behavior in response to a particular set of stimuli. Accordingly, in discussions of risk-taking situations, the more often other people in the group argue that the high-risk alternative is valuable, the more likely it is that the subject sees these arguments as being caused by a property of the choice dilemma, and therefore, the more likely he should be to assign a high value to the risky alternative (Kelley,1967).

Inasmuch as nongroup-exposed subjects do not have the opportunity to use arguments generated by others to infer the existence of item properties which direct the argumentation, they must rely on their own reactions to the stimulus items. Such subjects would thus be particularly sensitive to experimental details which would either foster an appropriate task orientation or reinforce the initial self-presentation bias.

The literature directly concerned with the choice behavior of nongroup-exposed subjects will now be reviewed. It will be seen that the following assumption can be supported: that choice shifts can and do occur in the absence of group discussion, but only when the experimental task facilitates both further study of item details, and a change in the subject's orientation from self-conscious concern to true task engagement.

The Familiarization Hypothesis was first advanced by Bateson (1966) who observed that in the research to that date no control condition had been included in which "...individuals working alone in any sense duplicated the discussion task undertaken by the group (p.120)".

Bateson argued that without appropriate controls it would not be possible to separate group effects from those arising out of further study.

In the original Bateson experiment, subjects responded to a pretest consisting of five items from the Choice Dilemma Questionnaire which characteristically produce a shift to risk, and were assigned to either a Group (2- or 3-member) Discussion-to-Consensus; an "Alone" condition; or a Control condition. In the Group condition, the standard Wallach and Kogan paradigm was replicated. In the Alone condition, subjects were asked to pretend they were consultants to the central person in each Choice Dilemma, and in this role to prepare briefs of all points pro and con each course of action. They were further directed to spend five minutes on each item. After 30 minutes, subjects filled in the CDQ for a second time. They were explicitly told at this point that this was not a memory test and that they should reconsider each situation before responding. It is significant to note that the postmeasure was thus separated from the actual familiarization exercise. Control subjects prepared briefs on nonrelated issues for the 30 minutes intervening between pre- and posttests. Bateson also administered a "recollection test" to all subjects within eight days after the experiment.

The principal finding was that both Group and Alone subjects produced significant shifts to risk while Control subjects did not. The magnitude of the shift was about the same for each of the experimental conditions. Additionally, Group and Alone subjects showed significantly greater recall of details of the CDQ items on the recollection test than did Control subjects.

A replication and extension of Bateson's findings was soon reported by Flanders and Thistelethwaite (1967). In this study an attempt was made to extend Bateson's findings by including a "Familiarization plus Discussion" condition to permit a comparison between the relative influences of group and nongroup familiarization on decisions. Following the standard Wallach and Kogan (1965) procedure, subjects were given a pretest on all twelve CDQ items. They were then assigned to one of three treatment groups: Familiarization, Discussion, or Familiarization-plus-Discussion. A control group took the pretest, a 24-item extraversion measure (included for all subjects as part of the pretest) and then went directly to the posttest.

The familiarization instructions differed from those of Bateson, who, it will be recalled, asked subjects to assume the role of consultant and thus prepare a brief of all pro and con arguments. In the Flanders and Thistlethwaite familiarization procedure subjects were instructed to prepare for a group discussion. They were run three at a time (with partitions between them) and were led to believe that following their private restudy of the CDQ items to clarify all options open to the central person they would discuss these with the other two individuals. After twenty-five minutes of private consideration, during which they were encouraged, though not required, to make notes or underlinings to aid them in the subsequent discussion the posttest was introduced as a means of assessing their present decisions before proceeding to the group discussion. Of course, in this condition no discussion actually took place. In the Discussion-to-Consensus condition subjects took a posttest following all discussion. In the Familiarization plus Discussion condition the posttest also followed the discussion.

Within-group shift scores were significant for all three experimental conditions, and did not differ significantly from one another. In the Familiarization plus Discussion condition, therefore, discussion did not lead to any further shift over that achieved by familiarization alone. Changes in within-group variability of risk-taking scores showed that there were significant reductions in those conditions which included group discussion. These findings are interpreted to mean that there were in fact "...group-influence processes...at work in the groups which discussed the dilemmas (p.96)," but that these apparently did not mediate the choice shift effects.

The Bateson and Flanders and Thistlethwaite familiarization treatments are similar in several critical ways. It would be of value to consider these similarities prior to discussing the unsuccessful familiarization studies. Both successful treatments present a clearly defined and believable task to subjects. Each begins with instructions which define the subject's role in the experiment. The familiarization task logically follows from these role assignments. In each study postmeasurement follows the familiarization task: i.e., subjects are unaware while preparing their arguments that their pretest responses are to be reassessed. Moreover, when postmeasurement is introduced, subjects are offered an explanation which serves to further reduce the salience of their pretest responses. Each of these common features: clear role-defining instructions, separation of postmeasurement from the familiarization task, and reduced salience of the pretest at time of postmeasurement, would serve to make subjects appropriately task-engaged. Under such favorable conditions for minimizing evaluation apprehension, it would be expected that familiarization would lead,

on a cognitive level, to greater comprehension of item contents (which Bateson did in fact demonstrate) and on an affective level, to reduction of uncertainty or greater felt commitment, either of which is associated with greater extremity (i.e.,movement away from the psychological midpoint of the scale) in selection of odds choices (c.f. Dion and Miller,1971; Moscovici and Zavalloni,1969; and Myers and Lamm,1976).

In the unsuccessful replications, by contrast, there appears to be far less concern in the procedural details with the kinds of inferences the subject might make concerning his role in the experiment. In these studies, the familiarization task is preceded by little or no explanation of its purpose, and/or subjects are informed at the outset that their pretest responses will be reassessed following familiarization with each item.

For example, Teger et al. (1970) report five separate unsuccessful replications. In actual fact, not one of these replicated the critical aspects of either Bateson's or Flanders and Thistlethwaite's procedures. For example, the first two studies by Teger et al. were intended to replicate Bateson's procedure, yet differed in several critical ways. Whereas Bateson's experiment took place in the laboratory and used volunteer subjects, these were run during regular class meetings. Moreover, while Bateson's instructions induced subjects to assume the role of consultant and in this capacity to prepare pro and con arguments, the Teger et al. subjects were instructed to "write down all the points for and against the possible decisions (p.347)" - without any further explanation or justification.

Similar irregularities appear in the third through fifth "replications". The third stydy, for example, was intended to replicate Flanders and Thistlethwaite, yet took place during a regular class meeting. Moreover the published account of the experiment does not specify what explanation, if any, was offered to subjects for the familiarization task. (Even if subjects were told that they were preparing for a group discussion, it would be difficult to make such an anticipated discussion credible, given the time constraints of a regular classroom meeting.) The fourth study was also an intended replication of Flanders and Thistlethwaite and was run in the laboratory. The researchers attempted to control for "E-bias" by telling half of their experimenters to expect a conservative shift in overall scores, and half to expect a risky shift. In fact, the subjects under both experimenters produced a nearly significant cautious shift. Although the investigators do not attempt to explain these findings, these would appear to raise important questions about subjects' perception of their task.

Teger et al.'s fifth study was intended to systematically examine the effects of anticipated discussion on subject's private risk preferences, as in the Flanders and Thistlethwaite procedure. Yet, whereas Flanders and Thistlethwaite separated the familiarization treatment from the retesting of odds choices, Teger et al. had subjects fill in their choices following consideration of individual items. It would thus appear that contrary to their claims, Teger et. al. hardly permitted the "fairest possible test (p.347)" of the familiarization hypothesis. Indeed, Dion and Miller (1971) who have extensively reviewed the choice shift literature (e.g.,Dion,Baron and Miller,1970) concede that the "designs (of Teger et al.) may not have been optimal (p.525)".

In their study, Dion and Miller (1971) sought to clarify the "Psychological processes underlying risky-shifts (p.525)" by comparing the joint and separate effects of familiarization and discussion in their two possible sequences of occurrence. They sought to determine whether, in producing their effects, familiarization and discussion operate similarly or orthogonally. Following the standard pretest on six CDQ items which typically show a risky shift, subjects were assigned to one of four treatments: Discussion-to-Consensus plus Familiarization(D+F); Familiarization plus Discussion-to-Consensus (F+D); Private Study; and Serious Reconsideration. The Familiarization part of the first two conditions weas intended to replicate Flanders and Thistlethwaite, although specific instructions to subjects are not included in the published report. The report does indicate that subjects were explicitly instructed to write pro and con arguments, although Flanders and Thistlethwaite only encouraged subjects to make notes or underlinings to aid them in the subsequent discussion. The Private Study condition allowed the same amount of time per item as the previous treatments except that the instructions "...explicitly forbade listing arguments or making any marks in the booklet (p.529)." Subjects in the Serious Reconsideration condition were allowed 1.5 minutes per item to "seriously reconsider" their initial choices.

To summarize the principal findings of Dion and Miller (1971): in the F+D condition, Discussion led to a significant increase in risk-taking over Familiarization; while in the D+F condition, no increment occurred when Familiarization followed Discussion. On the <u>first</u> postmeasure (i.e.,for the two joint conditions, a post-measure followed each separate procedure) Discussion (D+F) produced an

overall risky-shift, while Familiarization (F+D) did not. Curiously, while Familiarization led to an overall mean difference score of only .98, the Private Study condition led to an overall mean difference of 1.49. The so-called Serious Reconsideration condition led to a nearly significant cautious shift, which the authors attribute to the time pressure on those subjects. They consider an alternate explanation as well; namely that when subjects are asked to "seriously reconsider" their choices, they might perceive their initial choices as incorrect.

Dion and Miller, then, failed to replicate two critical findings of Flanders and Thistlethwaite: that of a significant within-group shift following Familiarization; and no further shift over Familiarization alone, following Discussion. They conclude that:

> ...prior support for a familiarization explanation stems, not from familiarization per se, but rather from heightened levels of involvement inadvertently generated in the familiarization procedure by some aspect of the experimental situations (Dion and Miller, 1971, p.532)

Unfortunately, their report does not indicate how their "experimental situations" were in fact different from the parallel treatments in Bateson and Flanders and Thistlethwaite, and so does not provide any substantial insight into how such "heightened levels of involvement" might be achieved.

Two kinds of familiarization treatments were included in a previously mentioned study by Ebbesen and Bowers (1974). Following a standard pretest on five CDQ items, subjects were told they would now review the items again in order to prepare arguments for and against the risky alternative to be used in "future investigations of decision making (p.320)". The subjects were also told at this point that they would be reevaluating their initial risky choices. (Note, therefore,

that while these investigators offer subjects a seemingly credible definition of their role in the experiment, they confuse that role by mentioning that their Pretest responses will be reassessed.) The investigators compared a so-called "Free Familiarization" condition in which subjects were to record the most persuasive arguments in favor of either choice alternative, to a "Forced Familiarization" condition in which they were requested to generate a specific portion of risky to cautious arguments under "high choice" conditions (the latter being modeled after the standard high-choice counterattitudinal advocacy condition in attitude change studies). Both conditions included interpolated measures: i.e., subjects would record their arguments for a specific item and then immediately re-rate their position on that item. A condition identical to the typical Wallach and Kogan group discussion-to-consensus condition was also included. Since Ebbesen and Bowers were particularly interested in the relationship between proportion of risky to conservative arguments generated and direction to shift, they performed content analyses of arguments generated for each item under each condition. In the discussion condition, where an overall risky-shift did occur, the average shift and proportion of prorisky arguments correlated .83, with 60% of the groups generating a greater proportion of risky arguments. In the Free Familiarization condition, a significant overall shift did not occur, and the overall proportion of risky arguments generated was only .53, suggesting that such subjects were producing a balanced set of pro-risky to pro-caution arguments. There were, as well, no significant shifts among subjects in the Forced Familiarization condition.

Ebbesen and Bowers support an Attribution Theory explanation of choice shifts. Central to their thinking is the expectation that choice shifts occur as a function of subjects observing each other generate a specific proportion of risky to cautious arguments (p.324). Among subjects working privately, choice shifts would occur as a consequence of the subject observing himself or herself generate a specific proportion of risky to cautious arguments. The fact that subjects in the Free Familiarization condition did not produce significant choice shifts leads them to an important consideration:

> by including premeasures that force the subject to concentrate on their initial risky positions in most risky-shift and familiarization studies, researchers may have insured that self-observation effects will not occur (p.325)

Alternately, by forewarning subjects that their initial positions would be reassessed following their generation of arguments, the experimenters in this case and elsewhere may have set up strong demand characteristics for pre-post response consistency. This in turn would prompt subjects to produce a 50-50 balance of arguments, regardless of the intrinsic "pull" of the item. (Note that while only 10% of the group discussions produced an equal number of risky and cautious arguments, 49% of the privately generated "discussions" produced this proportion; c.f. Ebbesen and Bowers,1974,p.320).

This discussion began with theobservation that the explanatory potential of a combined Value/Relevant Arguments Theory seemed to fall down in the case of nongroup-exposed subjects who generate and consider their own arguments. Possible differences with respect to task orientation between group and nongroup subjects which would account for

the discrepant findings were then considered. It was suggested that the group facilitates an appropriate task orientation in subjects, such that their more risky or cautious dilemma solutions are perceived to be a function of revealed dilemma properties. Subjects who undertake familiarization privately lack the opportunity for consensual validation of their reactions to specific items and so are more sensitive to instructional and procedural details in inferring their appropriate orientation to the task. To the extent that orienting instructions and subsequent procedure succeed in defining and validating the subject's role in the experiment, they facilitate an approach to items more like that of group subjects. They would thus come to respond in terms of revealed item properties. To the extent that instructional and procedural details are unclear or inconsistent, the more likely it seems that the subject would become uncertain and perhaps even suspicious of the "true" meaning of the experiment. His prime concern would then be with how he is "coming across" to the experimenter. This self-conscious orientation would then interfere with the subject's involvement with the item contents.

In comparing the successful and unsuccessful familiarization treatments, it was noted that three critical features of the successful studies were not replicated in the unsuccessful ones. Both successful familiarization treatments offered subjects a clear role definition. These role assignments appear to have set up the choice dilemmas as the focal point of experimental concern. Remeasurement was held off until all familiarization was completed. The posttest was then introduced in a way that both minimized the salience of the pretest and legitimized the measure in terms of avowed experimental concerns.

These combined features are missing in the unsuccessful replications. It is argued that these differences in instructional and procedural details may have in fact been responsible for the occurrence or non-occurrence of a familiarization effect. In order to alleviate further empirical and conceptual confusion bearing on the role of familiarization in producing choice shifts, an experiment is needed in which the joint and separate effects of the variables discussed above are systematically explored. This will be one of the main objectives of the present study, which will now be further detailed.

Design of the Present Study

The present research seeks to extend greater support to a combined Value/Relevant Arguments explanation of choice shifts by testing its applicability to response shifts on another measure of hypothetical risk-taking, the Behavior Prediction Scale. In particular, the concern will be in systematically exploring those variables which are assumed to control the predictive behavior of nongroup-exposed subjects.

As noted above (p.2), the sixteen-item BPS has been used for over a decade in studies of ethical risk-taking. The scale items describe the dilemma of a bank teller in conflict over stealing money from the bank which employs him. The situational context of the contemplated behavior changes from item to item. High and low values of each of four theoretical components of "predictive ethical risk-taking" (Reinforcement value of gain, RVgn; Expectancy of gain, Egn; Negative reinforcement value of censure, RVcs; and Expectancy of censure, Ecs) are systematically varied in all possible combinations.

Findings with the BPS have tied in with the hypothetical risk-taking literature in general in that specific response differences emerge between nongroup-exposed and group-exposed subjects, which have been interpreted as indicating greater riskiness on the part of subjects who participated in group discussion of the items. Specifically, it has been shown that nongroup-exposed subjects make their greatest discrimination with respect to the RVcs component, recording their highest or "riskiest" predictions when RVcs is low - i.e., when the teller believes that if he is caught, he can settle the matter privately with the

bank president. Following group discussion, on the other hand, subjects make their greatest discriminations along the RVgn component, recording their highest predictions of the behavior when RVgn is at its high value - when the bank teller considers taking the money to pay for a critical medical operation he needs. A comparison between group and individual predictive behavior shows that mean difference scores are about the same, and that predictions under low RVgn are likewise similar. Thus the shift to "gain emphasis" characterized by a dramatic increase in predictive behavior under high RVgn is the most consistent difference between nongroup and group subjects. A weaker ($p=.10$) but still relatively consistent finding has been that group subjects produced higher (and so "riskier") overall scores.

Rettig and his colleagues have rejected a cultural value interpretation of these effects, where the cultural value relates to risk and caution. They reason that since the behavior in question is unethical, the salient cultural value would be towards caution, and so towards lower, rather than higher predictions of the behavior's occurrence. The possible contribution of widely held values along dimensions other than risk vs. caution has not been considered (c.f. Rettig,1966,1972; Rettig and Turoff, 1967).

Rettig (e.g.,Rettig,1972) has favored a "locus of control" explanation of the observed effects. He notes that when individuals take part in a group interaction, they transfer some of their outcome control to the group. This transfer of control is accompanied by an expectation that more positive outcomes will occur. Hence a shift in orientation occurs, from costs to be incurred to rewards to be gained.

This would lead to greater risk-taking in a group context and increased discrimination along the RVgn component.

The locus of control interpretation seems to have two serious shortcomings however: first, while this explantion is based on the dynamics of group interaction, essential comparisons are typically made between group-exposed and nongroup-exposed subjects. Although the presence or absence of group exposure is the most conspicuous difference between these two sets of subjects, there may be other critical differences which must first be ruled out. For example, nongroup-exposed subjects may devote less time or serious attention to item details because of a lack of clarity concerning their role in the experiment. The Behavior Prediction Scale requires subjects to predict the likelihood that an unethical behavior will occur. Without a clear explanation of why this predictive behavior is sought, individuals might experience considerable evaluation apprehension. They may then formulate predictions on the basis of what these would reveal about themselves, rather than on the basis of discriminable item properties. A second shortcoming of the locus of control explanation is that it does not appear to fully explain the typical pattern of obtained choice shifts. If choice shifts are presumed to occur because subjects expect more positive outcomes, then there would be the expectation of a generalized tendency across all items for subjects to record riskier predictions. However, as noted, the increase in overall scores is only a marginally significant finding. The more robust finding is that of significantly higher levels of predictive behavior when RVgn is at its high level - when the bank teller considers stealing the money to pay for a critical medical operation he needs. It is this phenomenon that requires further elucidation.

An alternative to the locus of control explantion will now be offered, which is designed to accomplish two important objectives: a) provide a more satisfactory explanation of BPS findings; and b) lead to a closer theoretical integration between ethical risk-taking behavior and hypothetical risk-taking in general.

As previously discussed in the context of the Choice Dilemma Questionnaire, a combined Value/Relevant Arguments interpretation of choice shift effects holds that each dilemma item evokes a set of widely held values. These values serve to influence the direction of subsequent discussion or argumentation and so ultimately the kind (risky or cautious) of dilemma solution reached. When applied to the BPS, it would seem that the kind of cultural value most likely to be rendered salient would concern the rightness vs. wrongness of the contemplated behavior. Once the moral or ethical dimension of valuing is rendered salient, the contemplated behavior might appear more or less wrong, depending on its situational context. Under situational contexts where the behavior could be construed as less wrong in a moral sense, the preponderance of arguments would serve to make the behavior more acceptable or justifiable, leading to higher predictions of its likelihood of occurrence. Lower predictions would occur whenever the situation context does not render the behavior more justifiable in a moral sense. There is reason to believe that these kinds of contextual discriminations occur with respect to high and low values of RVgn, as Rettig and Turoff (1967) observed early on in relation to group behavior:

> The observed shift in RVgn took place only at its high level; that is, when the money is needed for a crucial medical operation. What is apparently happening during group discussion is that the needed medical operation is seen to provide a justifiable reason for taking money illegally. Such group-supported justification would qualitatively change the nature of the situation portrayed in the high-RVgn items from being unethical to being socially acceptable, at least in part (Rettig and Turoff,1967,p.180).

That the situational context of a behavior can influence moral evaluations of that behavior was an early finding of Piaget (1932) and has received recent attention among proponents of Attribution Theory. Kelley (1967), for example, has noted that while the nature of the behavior may iteself be overemphasized in formulating moral judgments, the perceived degree of personal control of the actor, as well as his motives are often significant considerations. These two contextual criteria may be summarized as follows:

1) Degree of personal control of the actor: the actor would be seen as more blameworthy if he freely chooses a morally questionable behavior over viable alternatives, than if he is a victim of circumstances over which he has little control (Kelley, 1971,p.24; Stevenson,1967,p.141).

2) Personal motivation of the actor: the actor would be seen as more blameworthy if he undertakes a morally questionable act for personal gain, than if he undertakes the act to preserve his present status or to avoid personal loss (Kelley,1971,p.23).

Applying these criteria, there would appear to be a strong basis for discriminating on moral grounds between the situations depicted under high and low levels of RVgn. Under high RVgn, the teller contemplates stealing the money in order to pay for a critical medical operation he needs. Here it would appear that the situation leading up to the behavior is one which is out of the personal control of the teller: a person is not ordinarily blamed for a health condition which

requires surgery. Moreover, the personal motivation of the teller would appear in this case to be loss avoidance - i.e., prevention of further deterioration of his health.

Under low RVgn, however, the teller considers stealing the money to help others who need it. His personal control seems rather high: an individual is ordinarily not expected to play philanthropist unless his personal fortune and personal inclination lead him to do so. Furthermore, the teller's motives may be suspect: perhaps he sees himself as a modern-day Robin Hood, stealing from the rich to give to the poor. He may stand to gain in terms of prestige or status as a consequence of his act. Under low RVgn, then, the teller can be viewed both as having more personal control over the situation which leads up to the contemplated act, and as being motivated more by the possibility of personal gain than by the need to avoid loss. In short, he is more blameworthy, and so the behavior he contemplates is more wrong in an ethical sense.

It remains to be demonstrated, however, that subjects who place the greatest emphasis on the RVgn component in formulating behavioral predictions actually do so on the basis of the differing ethical implications of high and low RVgn. This will be the second principal objective of the present study.

In summary, there are two main objectives of the present study. The first is to systematically study those variables assumed to be related to the occurrence or nonoccurrence of choice shifts among non-group-exposed subjects. In particular, it is expected that when instructional and procedural details foster an appropriate task orientation in such subjects, a shift to gain emphasis will be evident in their predictive behavior. The second objective is to test the

assumption that ethical values underlie the shift to gain emphasis on the BPS. This assumption is consistent with a combined Value/ Relevant Arguments interpretation of choice shifts. If the salience of relevant values (here, rightness vs. wrongness) underlies the shift to RVgn emphasis, then under those experimental conditions associated with increased emphasis on RVgn there should be a parallel tendency to judge the behavior under high RVgn as less wrong than under low RVgn.

The experimental design consists of two separate 2x2x2 factorials which will differ with respect to the dependent variable. For one factorial the dependent variable will be subjects' predictive behavior; for the second factorial the dependent measure will be subjects' ethical judgments. The independent variables and their specific levels can be described as follows:

FAMILIARIZATION

BPS item-related: Subjects will be asked to complete a thought sampling task in relation to each of the 16 BPS items.

BPS item-unrelated: Subjects will be asked to complete a thought sampling task for each of 16 "filler" items unrelated to the BPS.

TIMING OF REPEATED MEASUREMENT

Pure repeated measurement: Subjects will be administered the posttest following completion of familiarization task for all 16 BPS or filler items.

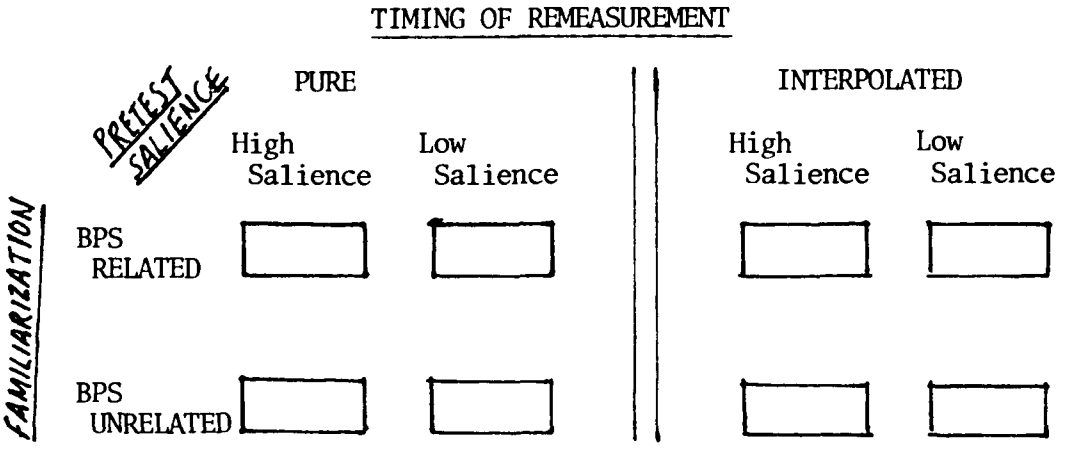
Interpolated repeated measurement: Subjects will reassess their pretest responses on an item-by-item basis. That is, they will complete the thought sampling task on a single BPS or filler item and then record their prediction or judgment for one BPS item before proceeding to the next familiarization task.

SALIENCE OF PRETEST

High salience: After completing the pretest, subjects will retain these responses for the duration of the experiment.

Low salience: The pretest will be collected immediately after completion.

The design just described could be depicted graphically as follows:



Note: Above represents each of two separate factorials with repeated measurement

Method

A total of 224 female subjects were recruited out of lower level psychology courses at Hunter College. At the time of recruitment they were told that subjects were needed for an experiment in social psychology which involved some writing and decision-making. They were told also that the experiment would take approximately 1 1/2 hours to complete. It was further explained that at the end of the experiment, subjects would be given a candy bar as a small token of the experimenter's appreciation. Subjects who filled out registration forms were later contacted by phone to arrange individual appointments.

I. Subjects recording behavior predictions. One entering the laboratory, the subject was given a 16-page BPS booklet in which items were presented one to a page, and a separate answer sheet. A cover sheet entitled "Behavior Prediction Scale" offered specific instructions for what comprised the pretest. These were identical to those instructions used by Rettig.

Following the pretest, half of the subjects had their answer sheets collected, while half were permitted to retain them, constituting the Salience manipulation. High and low salience subjects were then randomly assigned in equal numbers to one of the following conditions.

BPS-Related Familiarization/Pure Postmeasurement. Subjects in this condition retained their original BPS booklets. A new instruction sheet read as follows:

> Now that you have had a chance to briefly familiarize yourself with each of the stories, we would like you to do one more thing. We can now tell you that what you have been doing is helping us to select items to be used in another part of the study which involves group decision-making. We are trying to pick out items that will stimulate a good deal of discussion and debate. One way that we can determine this is to see what kinds of thoughts "pop into mind" when individual subjects consider these items.

> Accordingly, we want you to treat each item in the booklet as the beginning of a story which you are to complete. The stories, as you know, each portray a person in conflict about taking money that does not belong to him. As you turn to each page, read the opening lines carefully. Then complete the story by writing out your thoughts as they come into mind. You may want to indicate such things as what the person finally decides to do and how things turn out in the end. Although at times the opening lines of the stories may appear alike to you, each situation differs in some respect from every other one. So be sure to read each introduction carefully before you begin writing. Please use the space provided below each story to do your writing.

When the subject had completed all stories, he was given a final instruction sheet which read:

> We are deeply grateful for all the time and energy you have invested in the study. Before you leave, we would like you to take just a few minutes more to do one final thing. What we would like you to do, using the procedure described below, is to record a prediction for each of the situations described.

The remaining instructions reproduced the standard pretest directions.

BPS-Related Familiarization/Interpolated Postmeasurement. Subjects in this condition received a new instruction sheet which began with the same two paragraphs described above (beginning with "Now that..." and ending with "Please use...."). However, the instructions then continued as follows:

> One more thing! After you have completed a story, and before you turn to the next page, we would like you to take just a moment or two more to record a numerical prediction for the situation described.

The remaining instructions once again reproduced the standard pretest directions.

BPS-Unrelated Familiarization/Pure

Following the pretest, subjects in this condition had their BPS booklets collected, and were then provided with new booklets. As in the original booklet, one BPS item appeared at the top of each page in the new booklet. However, in the middle of each page a new item appeared. Each of the new items, 16 in all, described a "real-life" problem completely unrelated to the BPS. These items were prepared especially for this study. Each was five sentences in length, as is each BPS item and described such themes as a young man deciding on a college major, a young woman deciding whether to continue dating a man disliked by her parents, a middle class mother deciding whether to buy the baby doll her young son wants for his birthday, etc.

The accompanying instructions read as follows:

> Now that you have had a chance to briefly familiarize yourself with each of the stories, we would like you to do one more thing. We can now tell you that what you have been doing is helping us to select items to be used in another part of the study, which involves group decision-making. We are trying to pick out items that will stimulate a good deal of discussion and debate.
>
> On top of each of the following pages you will again see the items that you just responded to. In addition on the bottom of each page a new item appears.
>
> It is very important, in order to insure consistency in the study, that you carefully follow the directions below as you proceed from page to page.
>
> 1) First read the item on the top of the page.
>
> 2) Next read the paragraph on the bottom of the page. Please treat the paragraph as the beginning of a story which you will complete with your thoughts as they occur to you. You may want to indicate in your story such things as what the characters decide to do and how things turn out in the end.

> 3) When you have finished writing your story, please turn to the next page and repeat (1) and (2) above.

When the subject had completed all stories, he was given a final instruction sheet which was identical to the one provided for subjects in the BPS-Related/Pure condition described above.

BPS-Unrelated Familiarization/Interpolated Postmeasurement

As in the previous condition, subjects had their original BPS booklets collected following the pretest and were then given a new booklet as described above. The accompanying instructions were the same as those described in the previous condition, through specific directions 1) and 2). The instructions then continued as follows:

> 3) After you have written your story and before going on to the next page, please go back to the item on the top of the page and re-read it. Then take just a moment or two more to record a numerical prediction for the situation described. (This was followed with the standard pretest directions.)

Subjects were then instructed to repeat the described procedure on each subsequent page of the booklet.

II. Subjects recording ethical judgments. Each of the eight treatment combinations outlined above was replicated for subjects recording ethical judgments. In each parallel condition, the instructions differed only with respect to the behavior requested from subjects: i.e., these subjects were asked to judge "how right or wrong it would be for the person to take the money". The scale to be used as the basis for judgments appeared as follows:

```
                0----1----2----3----4----5----6
definitely                fifty-              definitely
  wrong                    fifty                right
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Hypotheses

1. Timing of Remeasurement

When the Familiarization task is Related, there will be greater pre-post increases in RVgn and Overall scores when remeasurement is postponed until all familiarization is completed, than when remeasurement occurs following familiarization with each item. That is, for Related Familiarization , Pure Remeasurement should lead to greater pre-to-post change than Interpolated Remeasurement. No appreciable difference in terms of Timing is expected under Unrelated Familiarization. Accordingly, the Analysis of Variance should show a significant Timing X Familiarization interaction.

Rationale. A review of the literature bearing on the Familiarization Hypothesis suggests that the familiarization task may elicit differing orientations from subjects, depending upon when remeasurement is introduced in relation to the task. When remeasurement is interpolated with familiarization, so that following familiarization with a given item the subject must record his second prediction for that item, it is more likely that aself-presentation concern is reinforced: the subject infers that the "true" purpose of the exerpiment concerns characteristics of subjects, rather than characteristics of the items (the stated purpose of the experiment). That is, the orienting instructions and procedural details may appear inconsistent and appropriate task engagement is prevented from occurring. Accordingly, for such subjects, the quality of familiarization would be weakened, and little change over pretest responding would be expected.

When remeasurement is effectively separated from familiarization, so that subjects enter the familiarization task without knowing that their numerical responses are to be reassessed, the task would appear consistent with the orienting instructions, and subjects' concern with self-presentation would be reduced. This in turn should enhance subjects' intensive consideration of item details. Moreover, given the "projective" nature of the selected familiarization task, it is assumed that subjects who experience minimal self-concern would be more likely to identify with the bank teller, and so attend to his motives for contemplating the act, leading to increased differential predictions in relation to RVgn.

It is assumed that the differential influence of Timing of Remeasurement is tied in with the opportunity for familiarization. In the absence of opportunity for familiarization, Timing of Remeasurement would not be a factor affecting predictive behavior. Hence, a Main Effect for Timing would not be expected, nor would differences among subjects participating in Unrelated Familiarization be expected.

2. Salience of Pretest

When the Familiarization task is related, there will be greater pre-to-post increases in RVgn and Overall Scores when pretest responses are unavailable during familiarization and remeasurement, than when these responses are available. That is, for Related Familiarization, Low Salience of pretest should lead to greater pre-post change than High Salience of pretest. Moreover, in the absence of opportunity for related familiarization (i.e., under Unrelated Familiarization), little difference would be expected in terms of pretest availability. Accordingly, the Analysis of Variance should show a significant Salience X Familiarization interaction.

Rationale. When subjects retain pretest responses throughout familiarization and remeasurement, the salience of these initial predictions will compete with the updating process which familiarization is designed to serve. Thus, at postmeasurement, the self-persuasion effects of familiarization may be inhibited by subjects' awareness of their initial responses. When subjects participate in Unrelated Familiarization, there is no reason to assume that their postfamiliarization reactions to the BPS items will differ from their initial reactions. Accordingly, availability vs. unavailability of the pretest would be expected to have little differential impact on such subjects. This line of reasoning leads to the expectation of no Main Effect for Salience, but a significant Salience X Familiarization interaction.

3. Timing x Salience

Among subjects participating in Related Familiarization, there should be greater pre-to-post change for RVgn and Overall Scores in the combined Uninterpolated, Low Salience treatment than in the combined Interpolated, High Salience treatment.

Rationale. As previously noted, Interpolated remeasurement is presumed to reinforce subjects' initial concern about self-presentation, precluding appropriate task engagement. For such subjects, availability of the pretest may further inhibit the updating process of familiarization. Such subjects may even use the familiarization task to further justify their initial reactions, rather than to modify them. This should lead to consistency in pre-post responding. Alternately, a review of the literature bearing on the Familiarization Hypothesis suggests that familiarization should have its greatest impact when remeasurement is postponed and pretest responses are unavailable. Thus, the combined

Pure, Nonsalient, Related Familiarization treatment would be expected to be the optimal one for choice shifts.

Subjects Recording Ethical Judgments

The specific hypotheses discussed above would be expected to hold for subjects recording judgments of the rightness-wrongness of the behavior in lieu of its likelihood of occurrence. A fundamental assumption here is that increased predictive behavior is tied in with underlying changes in moral judgments: the behavior is seen as more likely to occur when it is judged to be more justifiable in a moral sense. Thus the same treatment combinations which produce increased predictive behavior, would be seen to increase the tendency to judge the behavior as "less wrong" morally.

Results

I. Behavior Predictions

Hypothesis 1

Greater pre-post increases in RVgn and Overall Scores are expected under Related Familiarization when postmeasurement is held off until all familiarization is completed (i.e.,Pure) as compared to Interpolated. No differences are expected for Pure vs. Interpolated Remeasurement when familiarization is Unrelated.

Table 1 shows the Analysis of Variance summary for RVgn. It will be observed that the Timing X Familiarization interaction is not significant. Referring to Table 2 which shows mean difference values for the relevant treatment combinations, it can be seem that the Timing variable operated similarly under Related and Unrelated Familiarization: greater increases occurred when remeasurement was Pure as opposed to Interpolated. Neither of these simple main effects is significant ($F < 1$). The Main Effect for Timing, moreover, is not significant (Table 1).

Table 3 shows the Analysis of Variance summary for Overall Scores. Once again, the Timing X Familiarization interaction is not significant. Referring to Table 4, the mean difference values for the relevant treatment combinations, it can be seen that there is no pre-post change under Pure, and a slight pre-post decrease under Interpolated when Familiarization is Related ($F < 1$). For Unrelated Familiarization, Pure produced an increase of 1.75 while Interpolated produced no change ($F < 1$).

Hypothesis 2

Greater pre-post increases in RVgn and Overall Scores are predicted under Related Familiarization when Salience of Pretest is Low as compared

TABLE 1

Summary of Analysis of Variance of Pre-Post Differences for RVgn (BP)

SOURCE	df	MSS	F	p
MAIN:				
TIMING	1	28.000	0.745	n.s.
FAMILIARIZATION	1	54.321	1.445	n.s.
SALIENCE	1	89.286	2.375	0.122
TWO-WAY:				
TIMING X FAMIL.	1	8.036	0.214	n.s.
TIMING X SALIENCE	1	57.143	1.520	n.s.
FAMIL. X SALIENCE	1	0.036	0.001	n.s.
THREE-WAY				
TIMING X FAMIL X SALIENCE	1	38.894	1.034	n.s.
ERROR	104	37.599		

TABLE 2

Timing x Familiarization
Mean difference values for RVgn (BP)

		FAMILIARIZATION	
		Related	Unrelated
TIMING	Pure	2.9463	1.0357
	Interpol.	1.4285	0.5714

TABLE 3

Summary of Analysis of Variance of Pre-Post Differences for Overall Scores (BP)

SOURCE	df	MSS	F	p
MAIN:				
TIMING	1	26.036	0.499	n.s.
FAMILIARIZATION	1	24.143	0.463	n.s.
SALIENCE	1	2.286	0.044	n.s.
TWO-WAY:				
TIMING X FAMIL.	1	18.893	0.362	n.s.
TIMING X SALIENCE	1	30.036	0.575	n.s.
FAMIL. X SALIENCE	1	46.286	0.887	n.s.
THREE-WAY				
TIMING X FAMIL. X SALIENCE	1	34.321	0.658	n.s.
ERROR	104	52.199		

TABLE 4

Timing X Familiarization
Mean difference Values for Overall Scores (BP)

		FAMILIARIZATION	
		Related	Unrelated
TIMING	Pure	00.0000	1.7500
	Interpol.	- 0.1429	-0.0358

to High. No difference is anticipated for levels of Salience under Unrelated Familiarization

Referring again to Table 1, the Salience X Familiarization interaction proves to be nonsignificant for RVgn. Inspection of the relevant mean differences in Table 5 shows that for both Related and Unrelated Familiarization, greater pre-post increases in RVgn did in fact occur under Low Salience, but that in each case, the difference is nonsignificant ($F < 1$).

Table 3 shows the Salience X Familiarization interaction to be nonsignificant for Overall Scores. The relevant mean difference values in Table 6 show a small decrease under Low Salience and a small increase under High Salience for Related Familiarization ($F < 1$), and a small increase under High Salience with a somewhat larger increase under Low Salience for Unrelated Familiarization ($F < 1$).

Hypothesis 3

In Table 7, it can be seen that greater increase in RVgn occurred under Pure, Low Salient, Related Familiariza ion than under Interpolated, High Salient, Related, as expected. The difference falls short of significance, however ($F(1,104)=2.007, p > .10$). Under Unrelated Familiarization, the Pure, Low Salient combination likewise produced the greater increase ($F < 1$).

In Table 8, it is seen that under Related Familiarization, greater increase in Overall Scores occurred under Interpolated, High Salience than under Pure, Low Salience. The difference is not significant ($F < 1$). Under Unrelated Familiarization, greater increase occurred under Pure Low Salience, than under Interpolated, High Salience, although the difference is once again, not significant ($F(1,104)=1.040, p > .10$).

TABLE 5

Salience X Familiarization
Mean difference values for RVgn (BP)

		FAMILIARIZATION	
		Related	Unrelated
SALIENCE OF PRETEST	Low	3.0715	1.7143
	High	1.3215	- 0.1071

TABLE 6

Salience X Familiarization
Mean difference values for Overall Scores (BP)

		FAMILIARIZATION	
		Related	Unrelated
SALIENCE OF PRETEST	Low	-0.8571	1.3572
	High	0.7143	0.3572

TABLE 7

Salience X Timing X Familiarization
Mean difference values for RVgn (BP)

		FAMILIARIZATION			
		Related		Unrelated	
	TIMING:	Pure	Interpolated	Pure	Interpolated
SALIENCE OF PRETEST	Low	5.1420	1.0000	2.0710	1.3570
	High	0.7860	1.8580	0.0000	-.2150

TABLE 8

Salience X Timing X Familiarization
Mean difference values for Overall Scores (BP)

			FAMILIARIZATION			
			Related		Unrelated	
		TIMING:	Pure	Interpolated	Pure	Interpolated
SALIENCE OF PRETEST	Low		0.2857	-2.0000	2.2143	0.5000
	High		-0.2857	1.7143	1.2857	-0.5714

II. Ethical Judgments

Hypothesis 1:

Table 9, the Analysis of Variance summary for RVgn, reveals that the Timing X Familiarization interaction is nonsignificant. Inspection of the relevant mean values in Table 10 shows that under Related Familiarization, Interpolated shows greater pre-post increase than Pure ($F < 1$), while under Unrelated Familiarization, there is virtually no difference between the two.

The Analysis of Variance summary for Overall Scores in Table 11 shows the Timing X Familiarization interaction falling short of conventional levels of significance ($p=.13$). As can be seen in Table 12, this can be accounted for as follows: for Related Familiarization, higher mean scores are obtained under Interpolated than under Pure Remeasurement ($F(1,104)= 1.783, p > .10$), while for Unrelated Familiarization, the reverse is true ($F < 1$).

Hypothesis 2:

Referring again to Table 9, it is seen that the Salience X Familiarization interaction is not significant. In Table 13, which shows the relevant mean values, Low Salience of Pretest produced greater change than High Salience ($F < 1$) for Related Familiarization. Under Unrelated Familiarization, High Salience produced slightly greater change than Low Salience ($F < 1$).

With respect to Overall Scores, Table 11 shows the Salience X Familiarization interaction to be nonsignificant. Table 14 displays the relevant mean values. These values suggest that under Related Familiarization, there is slightly higher pre-post increase under Low, as compared to High Salience, while under Unrelated Familiarization, there is greater pre-post change under High, as opposed to Low Salience. Neither of these simple main effect is significant ($F < 1$ in each case).

Table 9

Summary of Analysis of Variance of Pre-Post differences for RVgn (EJ)

SOURCE	df	MSS	F	p
MAIN:				
TIMING	1	16.509	0.496	n.s.
FAMILIARIZATION	1	3.223	0.097	n.s.
SALIENCE	1	3.223	0.097	n.s.
TWO-WAY				
TIMING X FAMIL.	1	21.437	0.645	n.s.
TIMING X SALIENCE	1	58.580	1.761	n.s.
FAMIL. X SALIENCE	1	29.009	0.872	n.s.
THREE-WAY				
TIMING X FAMIL. X SALIENCE	1	77.223	2.322	0.126
ERROR	104	33.257		

Table 10

Timing X Familiarization
Mean difference values for RVgn (EJ)

		FAMILIARIZATION	
		Related	Unrelated
TIMING	Pure	0.8571	1.3929
	Interpol.	2.5000	1.2857

Table 11

Summary of Analysis of Variance of Pre-Post differences for Overall Scores (EJ)

SOURCE	df	MSS	F	p
MAIN:				
TIMING	1	110.009	1.361	n.s.
FAMILIARIZATION	1	3.223	0.040	n.s.
SALIENCE	1	42.509	0.526	n.s.
TWO-WAY				
TIMING X FAMIL.	1	182.580	2.259	0.132
TIMING X SALIENCE	1	102.223	1.265	n.s.
FAMIL X SALIENCE	1	118.080	1.461	n.s.
THREE-WAY				
TIMING X FAMIL. X SALIENCE	1	157.937	1.954	n.s.
ERROR	104	80.825		

Table 12

Timing X Familiarization
Mean difference values for Overall Scores
(EJ)

		FAMILIARIZATION	
		Related	Unrelated
TIMING	Pure	1.1429	3.3572
	Interpol.	5.6786	2.7857

Table 13

Salience X Familiarization
Mean difference values for RVgn (EJ)

		FAMILIARIZATION	
		Related	Unrelated
SALIENCE OF PRETEST	Low	2.3571	1.0000
	High	1.0000	1.6786

Table 14

Salience X Familiarization
Mean difference values for Overall Scores (EJ)

		FAMILIARIZATION	
		Related	Unrelated
SALIENCE OF PRETEST	Low	3.8214	1.4285
	High	3.0000	4.7143

Hypothesis 3:

Table 15 shows the relevant mean difference values for a comparison between the Pure, Low Salient vs. Interpolated, High Salient treatment combinations for RVgn. Contrary to expectation, there is virtually no difference between these treatments for either Related or Unrelated Familiarization.

Table 16 shows the relevant mean values for parallel comparisons with respect to Overall Scores. Under Related Familiarization, greater change occurred under Interpolated, High Salience than under Pure, Low Salience. The difference, however, is not significant ($F(1,104)=1.206$, $p > .10$). A similiar pattern is seen under Unrelated Familiarization, and is likewise nonsignificant ($F < 1$).

Table 15

Salience X Timing X Familiarization
Mean difference values for RVgn (EJ)

		FAMILIARIZATION			
		Related		Unrelated	
	TIMING:	Pure	Interpolated	Pure	Interpolated
SALIENCE OF PRETEST	Low	1.6430	3.0720	-0.5000	2.5000
	High	0.0710	1.9290	3.2860	0.0710

Table 16

Salience X Timing X Familiarization
Mean difference values for Overall Scores (EJ)

		FAMILIARIZATION			
		Related		Unrelated	
	TIMING:	Pure	Interpolated	Pure	Interpolated
SALIENCE OF PRETEST	Low	1.7860	5.8570	-0.4290	3.2850
	High	0.5000	5.5000	7.1430	2.2860

Discussion

I. Behavior Predictions

No appreciable increase in Total Scores was observed among the various treatment combinations, suggesting that overall level of predicting remained about the same from pretest to posttest. This finding in and of itself is not unusual since it has been noted that in previous studies with the Behavior Prediction Scale, such changes rarely reach conventional levels of significance even for group-exposed subjects.

With respect to RVgn, none of the critical comparisons reaches statistical significance. However, the trends suggested by inspection of mean values indicate that the Timing and Salience manipulations operated in the expected direction: with Pure remeasurement producing greater pre-post change than Interpolated; Low Pretest Salience showing more change than High Salience; and Pure Low Salient combination producing greater pre-post change than Interpolated High Salient.

There are two intriguing aspects of these trends: 1)that they are similar for both Related and Unrelated Familiarization, and 2)that they fall short of statistical significance. With respect to the first point, the data suggest the conclusion that subjects who had the opportunity for familiarization with BPS items did not differ appreciably in their predictive behavior from subjects who undertook an unrelated task prior to remeasurement. This finding is consistent with that of the majority of previous studies which failed to find evidence for a familiarization effect. In accounting for the two studies (Bateson, 1966; Flanders and Thistlethwaite,1967) which did find evidence of significant pre-post change following familiarization, Dion and Miller's (1971) assessment might prove helpful:

> ...prior support for a familiarization explantion stems, not from familiarization per se, but rather from heightened levels of involvement inadvertently generated in the familiarization procedure by some aspect of the experimental situations (p.532).

That is, the procedural details in and of themselves, may have accounted for the observed effects. In the present experiment, the absence of a difference between Related and Unrelated Familiarization would bring attention to the Main Effects of Timing and Salience. These, as noted, did not reach statistical significance, which leads to consideration of the second point raised above. The difficulty is the substantial within-group variance, relative to the treatment mean squares. Since subjects were randomly assigned to treatments, nonrandomness as a source of bias can most likely be ruled out. The Analysis of Variance performed on pretest overall scores was nonsignificant ($F < 1$), confirming that subjects in the various conditions did not differ with respect to initial risk-taking levels.

There is, however, the possibility that uncontrolled individual difference factors did affect the impact of the various treatments. For example, a closer look at the pretest behavior of subjects reveals that while pretest values between groups were comparable, with mean component values showing RVcs to be higher than RVgn (the expected finding in studies with the BPS), a substantial number of subjects in each treatment cell actually produced higher RVgn scores than RVcs scores. That is, initially, subjects within groups differed with respect to their emphasis on RVgn vs. RVcs. These differences may be important in assessing the impact of treatments which are presumed to affect subject orientation to task. Table 17, for example, compares the mean difference values with

respect to RVgn for all subjects, vs. initial censure emphasizers, vs. initial gain emphasizers, for the levels of each of the independent variables. In each case, it appears that initial gain emphasizers show less pre-post change than initial censure emphasizers. Since initial gain vs. censure orientation was not a factor used in treatment assignment, these differences were not considered in the data analysis and so may have contributed to the large within group variance.

Table 17

Mean pre-post difference values of RVgn for all subjects vs. initial censure emphasizers vs. initial gain emphasizers

VARIABLE	ALL SUBJECTS		INITIAL ORIENTATION			
			RVcs > RVgn		RVgn > RVcs	
	N	mean diff.	N	mean diff.	N	mean diff.
TIMING						
Pure	56	1.998	40	2.770	16	0.068
Interpolated	56	0.999	35	1.310	21	0.480
FAMILIARIZATION						
Related	56	2.196	38	2.520	18	1.510
Unrelated	56	0.803	37	1.650	19	-0.840
SALIENCE						
High	56	0.607	29	1.440	27	-0.287
Low	56	2.392	46	2.500	10	1.897

II. Ethical Judgments

As noted, Familiarization was seen to have some differential impact on overall level of ethical judgments, most notably in relation to the Timing variable. While there was no real difference between Pure and Interpolated remeasurement under Unrelated Familiarization, the difference under Related Familiarization approaches significance, with Interpolated apparently leading to greater pre-post increase in Overall Scores than Pure Remeasurement. That is, subjects who were remeasured on an item immediately following familiarization with that item showed greater pre-post change in their tendency to judge the behavior as "less wrong" than did subjects who were remeasured following familiarization with all items. This finding runs counter to Hypothesis 1. Hypotheses 2 and 3 were also not confirmed.

With respect to RVgn, Interpolated produced more change than Pure, under Related Familiarization, a trend opposite to that predicted by Hypothesis 1, but consistent with Overall Score changes noted above. Low Salience produced more change than High Salience under Related Familiarization, a trend which is consistent with Hypothesis 2. There is virtually no difference between the Pure Low Salient and Interpolated High Salient treatment combinations under Related Familiarization, which disconfirms Hypothesis 3.

These trends, when compared to those previously discussed for Behavior Predictions, provide little justification for assuming that Behavior Predictions and Ethical Judgments are related kinds of decisions. Rather, the evidence suggests that Behavior Predictions and Ethical Judgments are independent decisions, at least when made by subjects who are not exposed to group discussion.

It could be argued, however, that the question of whether changes in ethical judgments underlie changes in behavior predictions following

group discussion is still an open one. That is, in the present experiment, the predictive behavior of subjects participating in Related Familiarization did not differ from that of subjects participating in Unrelated Familiarization. Thus it could be argued that group exposure is a necessary precondition for relevant values to become salient.

This explantion for increased emphasis on RVgn should still be tested in an experiment involving group discussion of BPS items. In such an experiment it would seem wise to consider the initial predictive orientation of individual subjects as reflected in their differential emphasis on gain vs. censure. By considering this factor in assigning subjects to groups, it would be possible to test a Values/Relevant Arguments explanation against, for example, a Group Polarization explanation. The latter, which has recently received renewed attention (Myers and Lamm,1976), argues that choice shifts following group discussion reflect further enhancement by the group of the dominant direction of individual pre-discussion choices. Increased emphasis on RVgn following group exposure would thus be expected only in those groups where subjects are initially gain-oriented. In those groups where subjects are initially censure-oriented, the post-discussion change should be in the direction of increasing RVcs. Alternately, the view that a shift to RVgn occurs as a consequence of increased salience of underlying moral values, would predict increases in RVgn for both initially gain-oriented and initially censure-oriented groups.

III. Thought Samplings

While a formalized content analysis of thought samplings were felt to be beyond the scope of this project, an impressionistic overview would seem to be of some interest and value.

Thought samplings produced by subjects recording behavioral predictions as well as by those recording ethical judgments reveal the strong discriminating influence of the RVcs component. That is, in terms of relating story details and conclusions, subjects appeared to be particularly sensitive to whether the bank teller, if caught, would be expelled from the bank and subject to criminal prosecution (High RVcs); or would be able to settle the matter privately with the bank president (Low RVcs). This sensitivity to the RVcs component is of course consistent with all previous research on the predictive behavior of nongroup-exposed subjects responding to the BPS, and is seen also in the numerical responses of the subjects in the present study. (See Tables 18-20.) Additionally there is a tendency among subjects recording ethical judgments to rate the behavior as more wrong under High, as opposed to Low, RVcs. (See Tables 21-23.)

It is interesting to note that subjects recording behavioral predictions seemed less likely to invoke a moral value (i.e., "it would be wrong to take the money because that would amount to stealing") in their thought samplings than were subjects recording ethical judgments. It may be that moral values are not salient aspects of the decision-making process (at least outside of the group context) unless the task explicitly calls attention to the rightness-wrongness dimension. This point is consistent with the earlier observation of little relationship between numerical predictions and wrongness judgments.

Another trend observed among a minority of subjects is revealing, insofar as it suggests a potential drawback of using a projective-type task such as thought sampling. That trend was for some subjects to introduce new story elements apparently aimed at freeing the teller (and so the subject) from the dilemma of whether to take the money. For example,

Table 18

RVcs (BP) Component Values by Cell

Treatment Combination	Pretest RVcs	Posttest RVcs
Related Familiarization:		
Pure, Low Salience	-17.0714	-13.2143
Pure, High Salience	-14.0000	-14.7143
Interpolated, Low Sal.	-15.8571	-12.2857
Interpolated, High Sal.	-10.6429	-10.0714
Unrelated Familiarization:		
Pure, Low Salience	-12.2143	-14.2857
Pure, High Salience	-10.8571	-12.2857
Interpolated, Low Sal.	-14.2143	-12.0000
Interpolated, High Sal.	-11.6429	- 7.3571

Table 19

Summary of Analysis of Variance of Pre-Post Differences for RVcs (BP)

SOURCE	df	MSS	F	p
MAIN:				
TIMING	1	211.750	3.845	0.050
FAMILIARIZATION	1	32.142	0.584	ns
SALIENCE	1	41.286	0.750	ns
TWO WAY:				
TIMING X FAMIL	1	141.750	2.574	0.107
TIMING X SALIENCE	1	15.750	0.286	ns
FAMIL. X SALIENCE	1	185.143	3.362	0.066
THREE WAY:				
TIMING X FAMIL. X SALIENCE	1	0.036	0.001	ns
ERROR	104	55.066		

Table 20

Salience X Timing X Familiarization
Mean pre-post difference values
for RVcs (BP)

		FAMILIARIZATION			
		Related		Unrelated	
	TIMING:	Pure	Interpolated	Pure	Interpolated
SALIENCE OF PRETEST	Low	3.8571	3.5714	-2.0714	2.2143
	High	-0.7143	0.5714	-1.4286	4.2857

Table 21

RVcs (EJ) Component Values by Cell

Treatment Combination	Pretest RVcs	Posttest RVcs
Related Familiarization:		
Pure, Low Salience	-5.3571	-5.000
Pure, High Salience	-4.2143	-4.4286
Interpolated, Low Sal.	-5.2857	-4.5714
Interpolated, High Sal.	-2.7857	-1.5000
Unrelated Familiarization:		
Pure, Low Salience	-2.5714	-1.7143
Pure, High Salience	-3.9286	-5.6429
Interpolated, Low Sal.	-1.5000	-1.7857
Interpolated, High Sal.	-3.2857	-4.000

Table 22

Summary of Analysis of Variance of Pre-Post Differences for RVcs (EJ)

SOURCE	df	MSS	F	p
MAIN:				
TIMING	1	5.143	0.246	ns
FAMILIARIZATION	1	28.000	1.340	ns
SALIENCE	1	15.750	0.750	ns
TWO-WAY:				
TIMING X FAMIL	1	7.000	0.335	ns
TIMING X SALIENCE	1	18.893	0.904	ns
FAMIL. X SALIENCE	1	15.750	0.754	ns
THREE-WAY:				
TIMING X FAMIL. X SALIENCE	1	1.750	0.084	ns
ERROR	104	20.900		

Table 23

Salience X Timing X Familiarization
Mean pre-post difference values
for RVcs (EJ)

		FAMILIARIZATION			
		Related		Unrelated	
	TIMING:	Pure	Interpol.	Pure	Interpol.
SALIENCE OF PRETEST	Low	0.3571	0.7143	0.8571	-0.2857
	High	-0.2143	1.2857	-1.7143	0.7143

in one case, the teller inherits a substantial sum of money on the eve of the contemplated crime; in another case, he marries the bank president's daughter and so can readily secure the money he needs as a loan; and in still another case the teller's health spontaneously improves and he no longer requires the surgery. In each of these instances the stimulus situation is dramatically altered as the motive for considering the illegal act is extinguished.

In summary, the present experiment was designed to accomplish two objectives: to systematically study those variables assumed to be related to the occurrence or nonoccurrence of choice shifts among subjects undergoing private familiarization; and to test the assumption that ethical values underlie the shift to gain emphasis on the Behavior Prediction Scale. With regard to the first objective, the study is consistent with the majority of previous studies which failed to find evidence for a familiarization effect. The study confirms Dion and Miller's (1971) contention that factors other than familiarization per se were responsible for the positive findings of Bateson (1966) and Flanders and Thistlethwaite (1967) in that, with respect to RVgn, the Timing of Remeasurement and Salience of Pretest variables operated similarly for subjects who had the opportunity for Related Familiarization and those who did not.

With regard to the second objective, no evidence linking behavior predictions and ethical judgments is found in the present study. However, as noted, such a relationship may yet be found in studies providing group discussion of BPS items. Studies of this kind should first explore the dispositional differences among subjects which may underlie initial differential emphasis on RVgn vs. RVcs.

Further Considerations

As with any study, consideration must be given to those factors which bear on the extent to which findings may be generalized. Accordingly, we now deal with specific aspects of the present study which relate to this concern.

1. Measure of Hypothetical Risk-Taking. The present study has utilized the Behavior Prediction Scale as the measure of hypothetical risk-taking, while previous work bearing on the Familiarization Hypothesis has been based on the Choice Dilemma Questionnaire. This makes it necessary to consider the differences inherent in the two scales.

First, it should be noted that CDQ items are independent of each other thematically. That is to say that each item depicts a dilemma which is quite distinct, even permitting the use of only a subset of items in a number of studies. On the other hand, the 16 items of the BPS are interrelated thematically: the theme of a bank teller in conflict over taking money from the bank which employs him is common to all items. Only the situational context of the behavior is systematically varied from item to item. This fact, coupled with the consideration that built-in component scores provide a critical source of data, necessitate the use of the entire scale in each study. These points are of significance in considering a familiarization task appropriate to the BPS, which is further discussed under (2) below.

A second difference is that unlike the CDQ items, where the choice alternatives do not engage ethical concerns, the BPS dilemmas deal with the possibility of unethical conduct. It is therefore conceivable that the dynamics underlying decision-making differ for the two scales. On the other hand, however, previous studies (e.g.,Chapko,1972; Chapko and Solomon, 1974; Schulman,1972) as well as some major reviews of the risk-taking

literature (Kelley,1969; Kogan and Wallach,1967; Myers and Lamm,1975) do establish the precedent of attempting to integrate findings with both scales into a general scheme of decision-making.

2. Operationalizing Familiarization. The thematic similarities from item to item on the BPS as well as the necessity of employing all 16 items to achieve uniformity of scoring with previous studies, required that a different approach to the selection of an appropriate familiarization task be taken. In studies using the CDQ, as previously noted, investigators sought to accomplish familiarization by having subjects list arguments pro and con the choice alternatives (with some minor variations in a few studies). This would seem to be a feasible approach given the independence of theme from item to item.

On the BPS however, where each item is rather similar to the others, recording of relevant arguments seemed inappropriate. An alternate task was sought which would serve to make each item seem distinct from the others, as well as sustain subject interest. Thus it was decided to use a story-writing or "thought sampling" exercise in which each item was to be treated as the beginning of a story to be completed. The accompanying instructions encouraged subjects to consider what the teller decides to do and how things turn out, and so seriously think through the details of each item. It was further hoped that such a task would be sufficiently engaging to sustain subject interest through all 16 items, and additionally, that female college students, in thinking through the contemplated actions of a male bank teller would be more inclined to shift their attention towards item properties and away from concerns about self-presentation.

Thus, while the familiarization procedure outlined above was believed to be appropriate, given the distinctive features of the BPS, it does differ

markedly from procedures used in previous studies with the CDQ. Thus caution must be exerted in relating the findings of the present study to familiarization studies of the past.

3. Absence of formal check on manipulations. A critical interest in the present study has been with the effects of procedural differences on predictive and judgmental behavior. In particular the assumption was made that certain procedural details, such as low salience of pretest and postponement of remeasurement would facilitate a differing orientation in subjects than other kinds of procedural details, such as high pretest salience and interpolation of remeasurement with familiarization. More specifically, it was expected that the former set would lead to appropriate engagement in the task with greater attention to item properties than to self concern; while the latter details of procedure would serve to reinforce concerns about self-presentation or evaluation apprehension.

However, no direct check was included on whether these differences were in fact accomplished. Particularly in light of the lack of clear-cut findings, such additional information would be useful at this point. In the planning stages of this study, the inclusion of an explicit manipulations check was considered and rejected, in part because of the concern that such a check would serve only to heighten self-conscious concern among all subjects, and so wash out any actual success in creating differing orientations to the task. The decision was further influenced by the thoughtful comments of Dion et al. (1971) after they themselves criticized the absence of such checks by virtually all experimenters in the area of risk-taking:

> This is not to argue that manipulation checks are a panacea. They are often particularly susceptible to the image-making or face-saving whim of the subject. When administered before treatment they may haveeither a sensitizing effect or a commitment effect; when administered afterward, they may no longer reflect the psychological state that existed at the time of treatment (Dion et. al., 1971,p.315).

4. Absence of "pure control" condition. It may be seen as regrettable that the present research did not include what might be called a "pure control" condition to more clearly determine the effects of familiarization vs. no familiarization on changes in predictive or judgmental behavior. As noted in the methods section, all subjects, whether undergoing related familiarization or not were exposed to the BPS items on three occasions: at pretest, prior to the familiarization or filler task, and finally at posttest. It could be argued that the pre-filler exposure to the BPS for control subjects may have in effect provided more opportunity for familiarization with the items than would be optimal in a design which explicitly attempts to assess the impact of familiarization. In weighing the findings of little perceptible difference between familiarization and control subjects, the possible eroding impact of familiarization may in part be attributed to this. The findings might have been more clear-cut if a control group had been included which received only the pretest and the posttest, with no intervening exposure to the BPS.

The above considerations must be weighed before attempting to generalize from the present research, and certainly prior to the planning of follow-up studies.

Appendices

Appendix I

Behavior Prediction Scale

This questionnaire presents 16 different situations, each situation portraying a person in conflict about taking money which does not belong to him. After you read through a description, and before you turn to the next page, we would like you to take a moment or two to record a numerical prediction for the situation described. What you are to predict in each case is whether the person will take the money.

In recording your prediction, use the following procedure:

1) You have been provided with an answer sheet on which to record your predictions. Please record the numbers corresponding to your predictions one to a line. Thus, your first prediction should be recorded on the line to the right of #1. Your final prediction should be recorded on the line to the right of #16.

2) In making your prediction, please select the appropriate number from this scale:

QUESTION: WILL HE TAKE THE MONEY?

ANSWER:

0----1----2----3----4----5----6

definitely no — fifty-fifty — definitely yes

Enter the 0 if you feel that the person will definitely not take the money. Enter the 6 if you feel that the person will definitely yes take the money. Enter the 3 if you feel that the chances are about equal that the person will or will not take the money. Use the in-between numbers for the varying degrees of certainty, 1 or 2 being more on the no side, 4 and 5 being more on the yes side.

REMEMBER! Your choice of the numbers is to indicate whether or not the person would take the money, not how right or wrong it would be to take the money. Althought the 16 situations may appear to you very much alike at times, each situation differs in some respect from every other situation.

ETHICAL JUDGMENT SCALE

This questionnaire presents 16 different situations, each situation portraying a person in conflict about taking money which does not belong to him. After you read through a description, and before you turn to the next page, we would like you to take a moment or two to record a judgment for the situation described.

What you are to judge in each case is how right or wrong it would be for the person to take the money. In recording your judgments, please use the following procedure:

1) You have been provided with an answer sheet on which to record your judgments. Please record the numbers corresponding to your judgments one to a line. Thus, the number corresponding to your first judgment should be recorded on the line to the right of #1. Your final judgment should be recorded on the line to the right of #16.

2) In making your judgment, please select the appropriate number from this scale:

QUESTION: How right or wrong would it be for him to take the money?

ANSWER:

0----1----2----3----4----5----6

definitely wrong | fifty-fifty | definitely right

Enter the 0 if you feel that it would definitely be wrong for the person to take the money. Enter the 6 if you feel it would definitely be right to take the money. Use the in-between numbers for varying degrees of right or wrong, 1 or 2 being more on the wrong side, 4 and 5 being more on the right side, and 3 being the neutral position.

Remember! Your choice of the numbers is to indicate how right or wrong it would be for the person to take the money. Although the 16 situations may appear to you very much alike at times, each situation differs in some respect from every other situation. Read the item through carefully, record your judgment, and proceed to the next page.

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1. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would be enough to help a great number of people. The employee was sure that his theft would go unnoticed by the bank. However, the employee knew that if he were caught, he would be expelled from the bank and charged with criminal conduct.

2. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon had guaranteed the employee that the operation would cure the illness. The employee was sure that his theft would go unnoticed by the bank. However, the employee knew that if he were caught, he would be expelled from the bank and charged with criminal conduct.

3. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would be enough to help a great number of people. The employee was sure that his theft would go unnoticed by the bank. Furthermore, the employee was convinced that if he were caught, he could settle the matter privately with the bank president.

4. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon could not give the employee any guarantee that the operation would cure the illness. The employee was sure that his theft would be detected sooner or later. However, the employee was convinced that if he was caught, he could settle the matter privately with the bank president.

5. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon could not give the employee any guarantee that the operation would cure the illness. The employee was sure that his theft would go unnoticed by the bank. Furthermore, the employee was convinced that if he were caught, he could settle the matter privately with the bank president.

6. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would be enough to help a great number of people. However, the employee was sure that his theft would be detected sooner or later. However, the employee was convinced that if he was caught, he could settle the matter privately with the bank president.

7. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon could not guarantee that the operation would cure the illness. The employee was sure that his theft would go unnoticed by the bank. However, the employee knew that if he were caught, he would be expelled from the bank and charged with criminal conduct.

8. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon had guaranteed the employee that the operation would cure his illness. However, the employee was sure that the theft would be detected sooner or later. Furthermore, the employee knew that if he was caught, he would be expelled from the bank and charged with criminal conduct.

9. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would only be enough to help a very few people. The employee was sure that his theft would go unnoticed by the bank. However, the employee knew that if he were caught, he would be expelled from the bank and charged with criminal conduct.

10. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would only be enough to help a very few people. However, the employee was convinced that his theft would be detected sooner or later. Furthermore, the employee knew that if he was caught, he would be expelled from the bank and charged with criminal conduct.

11. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon had guaranteed that the operation would cure his illness. The employee was sure that his theft would be detected sooner or later. However, the employee was convinced that if he was caught, he could settle the matter privately with the bank president.

12. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would only be enough to help a very few people. The employee was sure that his theft would go unnoticed by the bank. Furthermore, he was also certain that if he were caught, he could settle the matter privately with the bank president.

13. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would only be enough to help a very few people. The employee was sure that his theft would be detected by the bank sooner or later. However, the employee was convinced that if he was caught, he could settle the matter privately with the bank president.

14. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon could not give the employee any guarantee that the operation would cure the illness. The employee was sure that his theft would be detected sooner or later. Furthermore, the employee knew that if he was caught, he would be expelled from the bank and charged with criminal conduct.

15. A bank employee had to have some money to pay for a crucial medical operation he needed. The employee was thinking of stealing the money from the bank. The operating surgeon had guaranteed the employee that the operation would cure the illness. The employee was sure that his theft would go unnoticed by the bank. Furthermore, the employee was convinced that if he were caught, he could settle the matter privately with the bank president.

16. A bank employee had to have some money in order to lend it to other people who needed it. The employee was thinking of stealing the money from the bank. The money he could get would be enough to help a great number of people. However, the employee was sure that his theft would be detected sooner or later. Furthermore, the employee knew that if he was caught, he would be expelled from the bank and charged with criminal conduct.

Note: In actual study, these items were presented one to a page.

Appendix II

Filler Items Comprising Unrelated Familiarization

1. Ralph is a sophomore in college. He is trying to decide what to choose as his major subject. His father, a prominent physician, wants Ralph to major in chemistry. His mother, a concert pianist, wants Ralph to major in music. Ralph is to meet with his advisor tomorrow to declare his major.

2. Mary and John have been dating for over a year. Mary's parents don't like John because he is a heavy drinker. Mary thinks that she is in love with John and feels that with her support, he will overcome his drinking problem. Her parents urge her to date other young men before things have gone too far. In the mail, Mary receives an invitation from her brother's college roommate to come up for Homecoming Weekend.

3. Elaine and Harry have been married for 22 years and have two sons away at college. They have had a happy marriage for the most part. Recently however, Harry confessed to Elaine that he was seeing another woman, Adele. Elaine was very upset and threatened to move out if Harry did not stop seeing Adele. Harry feels that he needs more time to make up his mind.

4. Rachel is planning to have the family over for Thanksgiving Dinner. She has already invited her husband's parents to come. However, she can't decide whether to invite her husband's only sister, Clara, and Clara's husband. She is annoyed that Clara has never invited the family over to her house for dinner. Clara does not know that Rachel is angry with her, but is aware that her parents have already received their invitation.

5. After many attempts, Sally has finally stayed on a reducing diet. She has already lost 20 pounds and has to lose 20 more. She goes to a wedding reception where there are many good things to eat and drink. Sally was so busy shopping all day for an evening bag to match her gown that she hasn't had anything to eat. As she looks down the Smorgasbord table, a platter of lasagna catches her eye.

6. Mary has a chance to join one of the most desirable sororities on campus. Her best friend Ruth was rejected by the same sorority a year before. Mary has been lonesome at college which is far away from home, and she knows that the sorority will give her the opportunity to make some good friends. Yet she remembers how deeply hurt her friend Ruth felt when the sorority turned her down. The sorority has invited Mary to a rush party the following evening.

7. Eleanor has been trying on shoes in the same store for nearly an hour. The shoe salesman, though very busy, has been extremely kind and has brouught out all the styles and colors that Eleanor wanted. Finally, Eleanor finds the style she likes. The salesman brings out the shoes in her size and they fit fine. As the salesman wraps up the shoes, Eleanor notices that one shoe is a little darker than the other.

8. June had studied very hard in college and is about to graduate with highest honors. She has offers from several first-rate graduate schools in her field, physics. June's parents, though proud of her success, are disappointed that she has few friends and rarely goes out on dates. They fear that if she goes on for rigorous training, she will never marry. June acknowledges that her studies have allowed her little time for an active social life.

9. Freida and her husband Jack live in a typical middle-class community with their son Eric. Eric is a friendly little boy who gets along very well with the other children in the playground. Recently, Eric asked his mother to buy him a doll he saw on television. The doll, called "Baby Cuddles" comes with her own hair curlers and baby stroller. Tomorrow is Eric's birthday and he eagerly awaits his new toy.

10. Tom Eagle is an ambitious young reporter for the New York Post. He has not covered any major stories as yet but has performed competently enough during his first year on the paper. Another young reporter, Nancy Burns, recently joined the paper. One day, while Tom is covering a reception at City Hall, he learns that a major scandal is brewing in the District Attorney's Office. When he tells his editor about it, the editor sends Nancy out to cover the story.

11. Eight years ago Barbara left a lucrative publishing job to begin raising her family. Now that her children are old enough to attend school for the full day she is eager to go back to work. After months of looking, she finds a very exciting job with a large publishing firm. She is the first woman ever to be hired for such a high status position. On Monday morning, as Barbara dresses for her first day of work, her six-year-old son Bruce awakens with a fever of 103.

12. Bill and Susan have been married for three years. They are very happy together, although Bill's job requires that he travel a good deal. Susan's job as an advertising executive keeps her in the city while Bill is on the road. Susan loves her job but knows that if she stopped working she could join Bill on many of his business trips. Bill wants Susan to quit her job so that she will have more time for him.

13. Robert's success in law school has exceeded his own expectations. At the end of his first year he ranks in the top 5% of his class. On the other hand, Robert's fiance and classmate, Helen, has been doing miserably. At a meeting with the Dean, Helen is warned that unless her work improves she will be dropped from the program. She phones Robert and tells him she must talk to him right away.

14. Harriet has a wonderful job that enables her to manage living expenses while her husband Sam attends medical school. On a recent visit to her doctor, a small tumor is discovered on one of her ovaries. The doctor tells Harriet that if she ever hopes to have a family she must become pregnant as soon as possible. Harriet and Sam have always loved children and hoped someday to raise a large family. But right now Sam has three years of medical school left.

15. Jack is a senior at Colgate University and has been captain of the football team for three years. He has always looked forward to a great career in professional football. In the next-to-last game of the season, Jack has a bad fall and fractures his knee. The orthopedic surgeon handling his case tells Jack that while he will recover complete use of his knee, he could never hope to play professional football. Jack is extremely depressed because he has never considered another career outside of football.

16. Cathy and Richie have dated all through college. They hope to get married sometime in the future. For the present however, Cathy plans to continue her studies in New York while Richie intends to go to law school in Maryland. Just before graduation, Cathy learns that she is six weeks pregnant. She doesn't know what to do.

Note: In actual study, these items were presented one to a page.

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Autobiographical Statement

Doris Klein Hiatt grew up in Far Rockaway, Queens where she attended the local public schools: P.S. 42, J.H.S. 198, and Far Rockaway High School. Her parents, Hannah and Leonard Klein believed it was important for children to have many interests and areas of competence. With their support, as well as that of her older brother Charles, Doris Klein competed successfully in numerous local spelling bees and science fairs, developed a hobby of ventriloquism, and played baseball on an otherwise all-boys' team in the Police Athletic League. At the time of her high school graduation, she ranked third in a graduating class of 804 and had completed a term as president of student government.

She pursued her undergraduate studies at the College of Arts and Sciences, Cornell University. There she majored in Government, became one of the first women disc jockeys on the college-owned AM radio station and held several offices in student government. In her senior year she served as President of the Women's Self-Government Association and led a successful effort to abolish certain regulations concerning curfews, mandatory residence in college dormitories and contract dining, which discriminated against women students. It was during this year that she met her future husband, Mark Hiatt, in a History of the Civil War course.

After graduation, she spent two years as a career counselor and job development specialist, first at Pace University and then at Barnard College. There then followed a nearly seven-year association with the Department of Psychology at Hunter College, initially as Lecturer, part-time and finally as Instructor.

At this writing Doris Klein Hiatt lives in West Long Branch, New Jersey, with her husband Dr. Mark Hiatt, a neonatologist, and their four year-old son Brian Douglas. A second child is expected to be born in a month's time.