Attributions, Affect, and Distributive Justice: Toward an Explanation of Allocation Preferences

by

Peter Danial Villanova

Thesis submitted to the Faculty of the
Virginia Polytechnic Institute and State University
in partial fulfillment of the requirements for the degree of
Master of Science
in
Psychology

APPROVED:

C. M. Peterson, Chairman

J. A. Sgro

S. J. Zaccaro

September, 1984
Blacksburg, Virginia

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(ABSTRACT)

This study explored the convergence between attributional egotism and felt injustice, and the consequences of these two constructs on subsequent reward allocations. Drawing from the work of Crosby (1984), Heider (1958), and Snyder, Stephan and Rosenfield (1978), it was contended that individuals who felt that they are victims of injustice use causal inferences similar to those characteristically employed for ego defense. This hypothesis was evaluated in an experimental paradigm which induced feelings of injustice and attributional egotism.

Undergraduate participants (N=153) were provided with one of three descriptions of an experimental task. They were told that the task was used to assess the cognitive abilities of high school sophomores (a low prestige referent), college juniors (a high prestige referent), or

merely that it was a standardized achievement test (control). Following this expectancy manipulation, subjects were given implicit success or failure feedback by being given an easy or difficult 30-minute task. When the 30-minutes had elapsed, subjects were asked to imagine that they were to provide feedback in the form of rewards to hypothetical students who had completed the same test the subject had just taken. Following the reward allocation exercise, subjects provided causal accounts for their performance and responded to a number of measures designed to assess their perceptions of the test and self-affect.

Analyses revealed that the manipulations were successful, but that the attributional egotism and deprivation induction were of modest magnitude. Allocation policies across the six different conditions varied, in some cases supporting predictions, in others not. These findings support the contention that attributional defensiveness and deprivation are similar, but surprisingly show that enhancement biases and relative gratification can operate simultaneously. Discussion centered on the explanation of these findings and the implications for future research in this area.

Acknowledgements

This is an achievement long waited for. There are many people who have played prominent roles along the path. Each name that follows in this acknowledgement represents individuals who have aided, abetted, and otherwise given shelter to my efforts and dreams. For many of these people thanks would be a trite remonstration; they deserve far more than what I compose on these pages. Please, don't be too disappointed by my attempt to thank you by this means.

To my parents whom I love so very dearly, thank you, and though it may be one of the countless "thank you's" you have heard from me, this one is special. Thank you for helping dreams come true. Thank you for keeping the sun shining on my parade, and yes I know...its your parade as well.

BZ, thank you baby. With your support and patience you have helped me immeasurably. You have been and are ...beautiful.

To Merle, who had the patience, trust, and compassion to share with me when on several occassions all good things seemed to have a shadow cast upon them. Thanks for lifting those shadows.

I also wish to express thanks to Chris Crosby and Jim

Austin. The friendship we share is very important to me. This became even more apparent as this project progressed. Thanks for being a friend.

My committee deserves special thanks for believing in the ideas of a student, for encouraging ideas, and for helping bring them about. I would like to offer Dr. Christopher Peterson special thanks for the role he played in directing this Master's Thesis. It has been a distinct pleasure for me to have you for a mentor. Your brilliant intellect and astute character has been in evidence throughout this project. To Dr. Stephen Zaccaro I extend thanks for encouragement and support that went far beyond what is normally expected for a member of a committee. You have always been free to lend an ear. Finally, I would like to thank Dr. Joseph Sgro for his support and contribution to my thesis. In all, I am fortunate to have people of such virtue comprise my committee.

Others too have helped in many a way. Dr. H. John Bernardin provided helpful advice and has been a motivating force throughout this endeavor. Dr. Philip Bobko was helpful in advising on statistical matters. That should be apparent in several sections of the thesis.

Finally, I would like to dedicate this work to the memory of two dear friends, Peter and John.

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Attributions, Affect, and Distributive Justice: Toward an Explanation of Allocation Preferences

Recent efforts in distributive justice research have been aimed at developing a theory of allocation preferences (Leventhal, Karuza, & Fry, 1980). Such a theory must acknowledge the breadth and complexity of motives that underlie such preferences. The present study was devised as a modest effort in this direction. Several constructs that may prove useful in explaining allocation preferences are examined in the following pages.

When allocation judgments are made, the allocator assesses individual attributes and decides what reward assignment is appropriate. Justice, egocentric, or social motives may guide the decision scheme employed by an allocator (Mikula, 1980). It is now widely recognized that allocators employ different decision rules (such as equity, equality, and need) to suit a given situation or satisfy a particular motive (Deutsch, 1975; Leventhal, 1976a; Sampson, 1975) and that other biases (egocentric, politeness) also operate to influence subsequent judgments. Social scientists have just recently begun to grasp the psychological

forces that act on these decisions. This study was intended as an extension of recent efforts by focusing on the influence attributional bias and relative deprivation have on allocation decisions.

Allocation Preferences

As noted by Leventhal et al. (1980), "Allocation preferences are attitudinal responses to specific features of an allocation situation" (p. 175). As such, the study of allocation preferences must deal with the allocator's aquisition of attitudes as well as the consequences of holding such an attitude (as might be reflected in an allocation decision). These preferences can be manifested in either or both the procedural systems of the allocation decision and the distribution actually generated (Deutsch, 1975; Leventhal, 1976; Nozick, 1974; Rawls, 1971).

Attitudes may dispose an individual to favor one allocation procedure over others. Some of the components of the allocation procedure that could be subject to attitudinal variation are: the extent of information sought and used by the individual, the structure of the allocation decision-making process, the establishment of appeal procedures, and the

flexibility of mechanisms of the allocation process to change. The components of procedural preferences may shape the distribution of rewards but may not necessarily result in the intended distribution. In many cases, the individual may devote his efforts at ensuring a desired distribution without attending to the means by which such a distribution is achieved. In other cases, the procedure may be an established routine and due to the lack of comparative systems, the individual invests little effort in assessing the features of the current system. In the latter case, procedural components may have resulted in a particular distribution, whereas in the former, the procedural aspects are ignored by the individual. In either case, the individual is less concerned with allocation procedures than with something else (achieving an equitable solution, finishing the task). Procedural preferences, then, may or may not result in preferred distributions.

Distributional preferences reflect attitudes of what the consequential distribution should be. They embody a direct concern with the end-product of allocation decisions. However, these distributions are causes as well as effects; they have cognitive and behavioral implications for the recipient of the

reward. These implications may affect the allocator as well. For example, given a situation in which an employee feels underpaid and exploited, he might feel resentment toward the employer and sabotage some aspect of the organization. Thus, the consequences of an inequitable distribution bode ill for recipient and allocator alike.

Let us assume, that a job is performed by two different individuals: A and B. Individual A has found the job to be difficult, and to require substantial effort to perform. Individual B, on the other hand, has found the job to be easy, and to require only modest effort to perform. Could these two different perceptions of the same job later affect these individuals' allocation policy when deciding to evaluate others performing this job? There is current research indicating that individuals' causal attributions for others' job performance may result from their own success or failure on the job (Mitchell & Kalb, 1982). It could be that these differing perceptions lead to different reward strategies.

If we can equate allocation decisions with performance appraisal decisions, then yet another consequence of prior experience can be illustrated. Let's assume that individuals A and B both supervise

employees who perform the job they themselves previously performed. Individual A's memory of what the job entailed differ from those of individual B. Will their appraisal of a common incumbent's performance be different as a result of their different perceptions of the job?

The above paradigm is not the exception but the rule with regard to supervisors' performance appraisal of subordinates. In most cases, supervisors have at one time occupied positions currently filled by their subordinates. If we assume that this experience may later affect supervisors' evaluation of their subordinates' performance, then the current employment of inexperienced and detached raters in performance appraisal research seems misguided. The present study is intended as an initial exploration into these matters. It tries to explain allocation practices by invoking two explanatory constructs: attributional egotism (Snyder, Stephan, & Rosenfield, 1978) and egoistical relative deprivation (Crosby, 1976). An attempt is made to show how these constructs may converge to explain allocation decisions in an experimental paradigm. Generally, the question of interest to the present study is as follows: <u>What allocation policy does an individual decide is</u>

appropriate for other individuals who have succeeded or failed at the same task at which they perceive themselves as having succeeded or failed?

The present study seeks to assess the extent to which biased causal ascriptions (indicative of attributional egotism) and felt resentment and perceived bias (indicative of felt relative deprivation) influence subsequent judgment policies.

Allocation Paradigms

Research in the area of social justice has witnessed the emergence of three dominant experimental paradigms. In one paradigm, subjects work competitively or cooperatively (individually or in small groups), and are provided with information regarding each members contribution to the attainment of voluntarily or experimentally set objectives. Subjects are then asked to allocate rewards (money, credit points) proportionally to themselves and the other participant(s). These allocations are compared to the operational standards which define recognized justice policies (i.e., equity, equality, need) to determine subjects adherence to any one or more of them (Leventhal, 1976a).

In a second paradigm, one used to assess the consequences of distributive practices, subjects work on a task alone or in small groups. The experimenter usually distributes rewards in a predetermined manner designed to elicit subjects' perceptions of, or reactions to, the existent distribution rule. This paradigm is frequently used to test for the operation of psychological constructs such as perceived inequity or relative deprivation. These constructs are usually operationalized as feelings of resentment and anger in the case of inequity or relative deprivation, and gratitude or contentment in the case of relative gratification (for reviews see Cook, Crosby, & Hennigan, 1977; Walster, Berscheid, & Walster, 1976).

The third paradigm is used less frequently, and best represents the convergence of perspectives sought in the present study. This paradigm may employ individual or group participation, and has subjects allocate rewards to themselves, others, or both without necessarily invoking social comparison as a referent standard. This approach tends to focus on both the "historical and contemporaneous information" that contributing to the operation of an internal standard (Messick & Sentis, 1983, p.63). In many cases, research using this paradigm assumes that individuals

possess an internal standard of justice (Pritchard, 1969), aspiration level (Weiner & Kukla, 1970) or some other individual difference (Greenberg, 1979; Lerner, 1975; Stake, 1983) that explains allocation preferences. An added benefit of using the historical-internal standard perspective is that an individual's past justice history or acquired comparative standard (learned, but either personally preferred or socially mandated) can be used as an explanatory construct (Pritchard, 1969).

Personal accounts for events, established standards of acceptable or expected performance, and reactions to violations of these standards have an experiential basis; they are grounded in one's personal history. Thus, at the heart of every allocation, personal history and the success or failure to realize established expectancies exert a psychological force upon the allocator. Personal history and the violation of established expectancies may explain the acquisition of attitudes that are later manifested as allocation preferences. What remains to be illustrated is just how these forces come to bear upon the allocation decisions of individuals.

Attributional Egotism

In their 1980 review, Kelley and Michela offered a general model to characterize the research in the attribution field. Kelley and Michela divided the field into two broad categories: (a) attribution theories which explain how people infer cause for an event and (b) attributional theories which explain the consequences of causal ascriptions. Since this study concerns itself with the consequences of attributions, a brief overview of this area and discussion of the proposed convergence with felt injustice follows.

A well-documented phenomenon in attribution research is the tendency for individuals to provide causal ascriptions that portray themselves in a positive light (Greenwald, 1980; Harvey & Weary, 1984; Zuckerman, 1979). Individuals accept responsibility for a positive event, and refuse responsibility for a negative event. This phenomenon is usually attributed to the operation of self-serving or egotistic biases. These biases are believed to stem from the need to protect and the desire to enhance self-esteem (Weary-Bradley, 1978).

Miller and Ross (1975) were not convinced that the motivational interpretation could withstand challenges from rival hypotheses. Their review showed that

several studies used designs which casted doubt on the validity of authors' inferences. They argued that these results could be more parsimoniously explained by the operation of information-processing factors and cited several such interpretations: (a) individuals intend and expect to succeed more than fail and so are more likely to make internal attributions for success, (b) the covariation between response and a failure outcome may be discounted by individuals who have experienced a pattern of increasing success, and (c) individuals possess an expectancy-confirmation bias that discounts the occurence of events discrepant with established expectancies.

The Miller and Ross paper led to research aimed at resolving the issue (for reviews see Snyder et al., 1978; Tetlock & Levi, 1982). By 1979, evidence generally favored the motivational explanation. That year, Zuckerman stated:

> "When all of the above is considered together with data from those experiments specifically designed to rule out nonmotivational explanations (e.g., Miller 1976; Sicoly & Ross, 1977), the self-serving hypothesis receives strong support. However, at this point, it would be wrong to conclude that motivational factors are the sole determinant of self-serving attribution, or that the available evidence is firm proof of motivationally-based distortion. It may be contended, however, that insofar as the evidence reviewed is concerned, the

> motivational explanation is more effective than any of its informational alternatives (1979, p.276)."

Research on attributional egotism has recently been extended to the role of misattribution for arousal (Gollwitzer, Earle, & Stephan, 1982) and to its external validity (Peterson, 1980; Smith & Manard, 1980). Research currently suggests that attributional egotism may be an adaptive mechanism that facilitates personal adjustment (Storms & McCaul, 1976; Weary, 1981; Wortman, 1976). Also, recent evidence suggests that self-presentational biases may mediate egotism (Weary, 1982). The adaptive benefit of egotism is subject to social and environmental constraints.

Attribution and Affect

Authors have also investigated what affective consequences attributions may hold for an individual (Weary, 1980). Research in learned helplessness pioneered the discovery of causal relations between causal attributions and behavioral consequences (Peterson & Seligman, 1984), and extensive research has now been conducted to extend these findings to achievement contexts (McFarland & Ross, 1982; Weiner, Russell, & Lerman, 1978, 1979). Weiner at al. have

pursued a systematic program of research on attributions for achievement-related tasks. Their research showed that there are outcome-related and attribution-related affects. They suggest that general positive and negative affects are associated with success and failure respectively, independent of the causal account for the outcome. However, self-esteem implications stem from the attributions for these outcomes; ability attributions for a successful outcome elicit feelings of pride and competence, whereas ability attributions following failure elicit feelings of incompetence and inadequacy. Weiner et al. (1978) also found that with the exception of luck attributions, which yield reports of surprise and awe, other causal attributions for success and failure yield opposite reactions like those found for ability attributions. For example, attributions to effort following success elicit feelings of pride, whereas these same attributions following failure elicit feelings of shame.

Of particular importance for the present study, Weiner et al. (1978) found that given a successful outcome attributed to the intentional acts of others, subjects reported feeling grateful and appreciative. On the other hand, given a failure attributed similarly

subjects reported they felt revengeful and aggressive. These reactions parallel those postulated by Utne and Kidd (1980) for the attribution of injustices inflicted on the self and perpetrated by others. Their contention was that given attributions of intentional injustice, the individual would experience affect that was more negative and intense than if the injustice were perceived as unintended or beyond the personal control of the agent. They also make the point that attributions to the intentional actions of external agents are necessary for one to refer to an outcome as unjust; self attributions for an outcome imply that it was deserved or intended, thus inconsistent with the definition of injustice.

The attribution of responsibility (or blame) to others is congruent with the dimensional composition and purpose of defensive attributions (Weary, 1981). Forty years ago, Heider's insight led him to a similar conclusion:

> Scapegoat behavior often is not simply release of aggression but includes blaming others for changes which, if attributed to the person, would lower the self-esteem. Cantril says that one of the functions of scapegoat behavior is that "it exonerates the people themselves from any blame for their conditions" (1944, p.369).

Extending the similarity between perceived

personal injustice and defensive style, might there be a continuum of "defensive arousal", with the degree of defensiveness a result of the causal accounts that constitute it (Fiske, 1981)? As illustrated in the findings of Weiner et al. (1978), when an individual provides a causal ascription to fortuitous circumstances (luck), the associated affect is undifferentiated between success and failure outcomes. Such ascriptions, then, lack intensity or direction due to the absence of a specific causal locus. Failure attributions implicating the efforts of others engender feelings of resentment and anger directed at a specific causal locus - the task, the perpetrator. Sometimes these attributions are the result of correct inferences, and sometimes they are biased by the motivation to protect one's self-esteem. It could be that the greater the threat to self-esteem, the greater the need to construct "protective illusions" (Freud, 1917) that identify a specific causal locus which in turn contributes to the experience of enhanced affect.

Defensive attributional style and the perception of injustice both may elicit the projection of blame and engender hostile feelings.

Affective Consequences of Felt Injustice

The relative deprivation concept (Crosby, 1976) is similar to the notions of defensive attribution. When people feel relatively deprived, they attribute responsibility for their situation to external agents. Also, when relatively deprived, people perceive their situation as unjust and harbor feelings of resentment toward the object perceived as responsible for their current plight. Both reactions can be used by individuals to deter the negative consequences an undesired event may have for self-esteem. Relative gratification, however, is not congruent with self-enhancement with regard to the directionality of affect.

When one feels relatively gratified, affect is directed outward toward an external attitude object in acknowledgement of the assistance provided, or pleasure obtained from the prevailing circumstance. On the other hand, the enhancement hypothesis predicts that individuals will assume personal responsibility for a positive event and deny responsibility to others for bringing it about. In this case, the person is not grateful; the person alone is responsible for the good event and has no reason to be grateful. Thus, while they converge with regard to the affective state

experienced by the individual, they differ with regard to the perceived cause.

Previous conceptualizations of relative deprivation made it difficult to demonstrate its convergence with attributional egotism. For example, Crosby (1976, p.90) offered a model of egoistical relative deprivation that consisted of five necessary preconditions:

1. wanting some object or condition
2. seeing that Other possesses the wanted object or condition
3. feeling entitled to the object or condition
4. thinking it feasible to obtain the object or condition
5. denying personal responsibility for failure to possess the object or condition

Crosby felt that if any of the five preconditions were absent, then relative deprivation does not occur (p.91). However, since her 1976 paper, she has offered more modest requirements for the induction of relative deprivation. In 1984 she stated:

> ...the system proposed in 1976 makes strong demands: five preconditions are a lot (p.61). On the basis of (new evidence), I have proposed a new model of deprivation (Crosby, 1982). It features wanting and deserving as the two preconditions of resentment, grievance or deprivation (p.67, italics in original).

This new model specifies that relative deprivation is experienced when people perceive (1) "a discrepancy between actual outcomes and desired outcomes and (2) a

discrepancy between actual outcomes and the outcomes they deserve" (Crosby, 1984; p.68). This revision facilitates the inducement of relative deprivation in the laboratory, as well as the assessment of its convergence with other concepts.

The term "relative deprivation" has been used in two different ways. One use of the term is derived from the concept of reference groups as developed by Hyman (1942), Merton (1957), and Stouffer et al. (1949). This use focuses upon the discrepancy between the individual's attainments and the attainments of others whom he uses as comparative referents. The second usage is derived from Lewin's level of aspiration theory (Lewin, Dembo, Festinger, & Sears, 1944) and emphasizes the discrepancy between the individual's aspirations and his attainments. These two uses are not contradictory, and indeed, Lewinian discussions of the factors determining the level of aspiration explicitly acknowledge the role of groups and social referents in establishing personal standards.

Consonant with recent applications of the Lewinian usage (Deutsch, 1974,p.28), I will use the term "relative deprivation" to refer to the "perceived discrepancy between what a person obtains and what he

believes he is entitled to obtain in the distribution of the opportunities, conditions, and goods which affect his welfare". Apparent in Crosby's earlier list of necessary preconditions is that a social standard (i.e., comparison with Other) is necessary for the occurence of felt deprivation. In her 1984 paper, Crosby stated that social comparisons are no longer a necessary precondition. Rather, they may serve to augment the feeling of injustice. An individual has over the course of his/her personal history acquired a standard of acceptability with regard to expected, and deserved outcomes. This internal standard can also function as a source of comparative information that when discrepant with actual information may cue feelings of contentment or disappointment. As noted by Clark (1982,p.285):

> ...unexpected positive (or negative) events ought to be more likely to produce ongoing positive (or negative) feeling states than should expected positive (or negative) events.

The basic assumption underlying this contention is that a distribution of expectancies for particular events is generated during a person's life, and these later serve as standards by which current information is evaluated for its suitability and desirability

(Miller & Ross, 1975). These expectancies also generate hypotheses for future outcomes and promote biased confirmation of these hypotheses (Snyder & Gangestad, 1981).

What seems to be a more plausible explanation of comparison processes is that people use an internal standard comprised of established expectancies as well as socially generated standards. For example, a college freshman is likely to possess personal expectations of doing well on an exam commonly used to test the academic proficiency of high school sophomores. This college freshman may also use socially engendered expectations or "oughts" (Heider, 1958) to assume that he/she would succeed on such an exam. Heider (1958) provides a discussion of the power of such "oughts". He refers to them as impersonal standards that indicate what should be done or experienced. They are independent of individual wishes or desires and stem from social values and expectations. As noted by Asch (1952, p.357; cited in Heider, p.219) ought is:

> Action that fits the requirements we judge to be appropriate or right; to fail to act appropriately we experience as violating a demand, or being unjust.

Returning to the example cited earlier, there is a

social "ought" or expectation that the college freshman do well on this exam. We can assume that his/her success would be "appropriate or right" and that failure would be a violation of the student's and our own expectancy and "unjust". How might this later affect allocations to (similar) other students who may have failed or succeeded on the test, tried hard or not, and who are high or low in ability? Do similarity biases then come into play? That is, do these intrapersonal processes then impact on interpersonal evaluations?

Let us assume now that the student, instead of expecting an exam suited for a lower status (educational) reference group, anticipates taking a test suited for college juniors (a higher status reference group). In this case, the student's expectancy to do well is more modest. Here, the personal expectancy engendered is lacking due to the absence of experience with a higher status exam. The social ought, on the other hand, is operative, and it represents a vector in the student's environment that reduces the student's expectancy of success. But the student finds that his/her performance far exceeded initial expectancies. As above, do referent cognitions and causal inferences then influence the student's

evaluation of others? Will certain person attributes be more highly regarded, attended to, and rewarded?

The present study uses such a paradigm to induce feelings of justice and injustice, success and failure. Students are provided with one of three descriptions of an experimental task. They are told that the task is used to assess the cognitive abilities of high school sophomores (low prestige reference group), college juniors (high prestige), or merely that it is a standardized achievement exam. Students indicate their expectancy and certainty of success on a pre-test questionnaire. They are then given a test that is either easy (implicit success) or difficult (implicit failure). Following the test, students are asked to imagine that they are responsible for providing feedback to other students who had just taken the same test. These hypothetical students vary in terms of four attributes (sex, ability, effort, outcome). After the assignment of rewards, students provide reports of their causal attributions for their performance on the test, perception of the test, experimenter bias, and self-affect. The following hypotheses are offered:

Hypothesis #1: Defensive causal attributions (i.e., attributions to luck and task difficulty) will be employed more by subjects in the low prestige-

difficult test condition than subjects in any other condition (other conditions should be approximately equivalent on this attribute).

Hypothesis #2: An enhancement bias will be more characteristic of attributions given by subjects in the high prestige-success condition. The latter subjects will make greater attributions to ability relative to subjects in other conditions. Again, other conditions should be similar in the extent to which they self-enhance.

Please note that this is an imprecise operationalization of attributional egotism. The present experimental design does not preclude the expectancy interpretation offered by Miller and Ross (1975). Rather, it capitalizes on the perceived discrepancy between one's actual and expected outcomes. Ideally, the prestige manipulation should follow the outcome manipulation (test difficulty). Such a strategy was used by Miller (1976) and makes for a stronger motivational interpretation of attributional biases. Since, in the present experiment, information describing the nature of the task precedes subjects' performance, the measure of attributional egotism will be imprecise.

Hypothesis #3: Relative deprivation

(characterized by feelings of anger and resentment directed at the test or experimenter) should only occur for those subjects perceiving they have failed on a test suited for a lower status reference group (i.e., high school sophomores). Relative deprivation, then, is predicted to covary with defensive externality. On the other hand, relative gratification would, according to the justice literature, be present among subjects in the high prestige-success condition. Note that the enhancement hypothesis predicts that subjects would provide internal causal ascriptions for their success on this test, whereas the gratification hypothesis predicts that subjects are "grateful" to the efforts of others and thus entails ascribing causality to external agents (luck, ease of the task).

Predictions for the role of biased causal ascriptions and feelings of deprivation and gratification on reward allocations are not so straightforward, nor is there available literature to specify exactly how these constructs should influence allocation policies.

Hypothesis #4: The magnitude of allocations (as represented by the average allocation) should be higher among subjects who are given implicit failure feedback. However, the magnitude should be even greater amongst

those who fail at a test suited for high school sophomores. Thus, a main effect for test difficulty is hypothesized; simple effects should show that this difference is even more pronounced among subjects in the low prestige condition. These subjects will have had great difficulty in completing the test, and they would share common beliefs with lower status students who must take the test. They will perceive the test as quite difficult and so elevate their allocations above those of other subjects.

A policy-capturing approach is used to assess the extent to which the change in value of each cue is used by subjects in their decisions. A multiple regression equation, using each of the four cues as predictors (sex, ability, effort, and outcome of the hypothetical student) and the composite allocation as the criterion, is generated for each subject. The standardized partial regression coefficient (Beta) calculated for each cue will represent the extent to which the cue can be used to paramorphicaly portray subjects' distribution preferences. Also, the squared standardized partial regression coefficient provides an index of the amount of variance in the criterion attributable to each cue. Summing these, a coefficient of determination is obtained that represents the extent

to which subjects' overall allocations are predictable (i.e., can be explained by the value of the cues).

Hypothesis #5: Given that subjects have just completed the test, their experience with it should remain salient during the allocation exercise. Subjects who feel deprived and who would have a defensive attributional style (low prestige-failure) may weigh the ability cue more heavily than those not having perceived themselves as having failed. Those subjects who perceive themselves as capable of success believe they are high in ability, but who perceive themselves as having failed "unjustly", may in turn reward students high in ability more, relative to subjects in other conditions. Since the weight of the ability cue in the judgment strategies of the other conditions should be equivalent, an interaction between prestige and test difficulty is predicted for the ability cue.

Hypothesis #6: Subjects who feel deprived (low prestige-difficult test) and who self-enhance (high prestige-easy test) are predicted to weight effort cues more than subjects in other conditions. Subjects who feel deprived and make defensive causal ascriptions are apt to reward one more for the exercise of an attribute over which they have control. These subjects would,

due to their unfavorable situation, wish to encourage the efforts of others to overcome the unfairness to which they were previously subjected. Effort cues would be weighted heavier as well by subjects who self-enhance. These subjects may attribute their own good fortune to their self-directed effort. In turn, they wish to reward others like themselves who overcame adversity by working hard. In this case a disordinal interaction between prestige and test difficulty is hypothesized. Self-enhancing and defensive subjects (as defined by their attributions) would weight effort cues equally, but greater than the remaining conditions.

Hypothesis #7: Outcome on the test would be weighted most heavily by subjects who perceive that they succeeded at a high prestige (easy) test. This a product of their tendency to self-enhance. These subjects of course succeeded and so might assume that the most meritorious attribute one can possess is success. Subjects in other conditions would weight outcome similarly but different from subjects in the high prestige-success condition. Again, an interaction is predicted between prestige and test difficulty.

Note that no predictions are offered with regard to the effects of gratification. As noted above, the

attributions stemming from gratification are contrary to those predicted by the self-enhancement hypothesis. The author's preference for the latter is based upon the large volume of literature supporting the occurence of such a bias (see Zuckerman, 1979).

Hypothesis #8: The final hypothesis regards the extent to which subjects will use the cues provided to base their judgments. Research in the judgment literature has led to a recently offered hypothesis regarding the influence of positive affect on subsequent decision schemes. Isen, Means, Patrick, and Nowicki (1982) hypothesized that positive affect may lead to a hurried and simplified judgment process. They argue that an individual in a positive affective state will find a single, minimally satisfying, or "satisficing" criterion (Simon, 1976) rather than attend to and use all the information available on which to base a decision ("optimizing"). Preliminary support for this hypothesis has been obtained by Means (1980), and the underlying process has been likened to the inclination of employing simplifying heuristics (Tversky & Kahneman, 1974).

Clark (1982) recently offered a compelling case for the simplifying effects of arousal on decision processes. She feels that the effects of arousal on

judgment are usually mediated through automatic as opposed to controlled processes (p. 283). This implies that individuals may seldom be aware of the effect that arousal may have on their decisions.

Given this line of reasoning, then, subjects who are either positively (high prestige-success) or negatively aroused (low prestige-failure) will use a simplifying heuristic in their judgments. This would result in policies indicating reliance on a small number of cues on which to base their judgments.

Method

Overview

Subjects were provided with one of three descriptions of an experimental task. They were told either that the task was used to assess the cognitive abilities of high school sophomores (low prestige), college juniors (high prestige), or merely that it was a standarized achievement test (control). Following this expectancy manipulation, subjects worked on a 30-minute problem-solving task that provided implicit success or failure feedback. When the 30-minutes had elapsed, subjects were asked to imagine they were to provide feedback in the form of rewards to hypothetical students who had completed the same test the subject had just taken. Four characteristics of the hypothetical students were orthogonally varied in the vignettes (gender, ability, effort, outcome). Subjects' causal attributions for their performance, and measures of test perception and self-affect followed.

Subjects

153 undergraduate students enrolled in introductory psychology classes at Virginia Polytechnic Institute and State University participated in the experiment for extra credit toward their final grade. These students were largely freshmen and sophomore students. However, each cell did have one student who was a junior in college. Since the experiment was conducted at the beginning of the academic year, and it was believed that the prestige manipulation would still be effective, their data were included in subsequent analyses.

A 3 X 2 ANOVA design was used with three levels of educational prestige for whom the test was allegedly suited (college junior, high school sophomore, control), and two levels of test difficulty (easy, difficult). Subjects were somewhat unequally distributed among the six cells of the design as follows: low prestige-easy (n=24), low prestige-difficult (n=27), high prestige -easy (n=29), high prestige-difficult (n=23), control-easy (n=25), and control-difficult (n=25). Approximately 25 subjects participated in each experimental session in which one level of prestige information was provided. Both test difficulty levels were present in each session by

randomly assigning the two test forms within each session.

Procedure

Approximately 25 subjects were run in each of six experimental sessions. Upon arrival to the experiment, subjects were asked to sit as far apart from one another as possible. They were asked to do this "since we are interested in obtaining a true measure of each individual's ability". The experimenter began each session by saying that since the subjects had consented to participating, he should perhaps provide them with some information about the test. In the low prestige (i.e., high school sophomore) conditions the experimenter described the test to subjects as follows:

> "The test booklet before you is called the Diagnostic Achievement Test. It was recently developed and is being used in a number of high schools to assess the cognitive abilities of high school sophomores. Scores on the DAT are used by guidance counselors and highschool students to plan course curriculums for the students' junior and senior years."

Subjects in the high prestige conditions (college junior) were told:

> "The test booklet before you is called the Diagnostic Achievement Test. It was recently developed and is being used in a

> number of colleges to assess the cognitive abilities of college juniors. Scores on the DAT are used by student advisors and college students to plan a course curriculum for the students' senior year".

In the conditions in which any prestige manipulation was absent (control) subjects were told:

> "The test booklet before you is called the Diagnostic Achievement Test. It is a recently developed standardized achievement test similar to those you may already have had experience with. Scores on the DAT can be used by teaching staffs and students to plan student course curriculums."

Subjects were then asked to fill out a 3-item questionnaire that tapped their perceived expectancy of success, certainty of attaining this level, and importance of doing well on the DAT. Each of these items were measured on 9-point scales. They will herein be referred to as the expectancy, certainty, and importance, measure respectively (these items are available in Table 1). When all subjects had completed the questionnaire, the experimenter gave the signal to begin working on the test and reminded the subjects that they had 30 minutes in which to complete it. As already mentioned, in each session, half the subjects worked on the easy test while the other half worked on the difficult test.

Subjects' test outcome perceptions were

manipulated by predetermined test difficulty. The easy test provided implicit success feedback to subjects and implicit failure feedback was provided by the difficult test. Test difficulty had been pre-tested on an independent sample of students prior to this experiment. From this pre-test it was found that on the difficult form (comprised of 45 items), students omitted an average of 10 items, whereas on the easy form (comprised of 33 items), students rarely omitted any items. Thus test length and time constraints were paired so that two tests of discriminable difficulty resulted. Also obtained through this pretesting was information regarding the average amount of time students required to complete the easy test (estimates for the difficult test were not obtained due to the time constraint). Most students completed all the items on the easy test by the time 25 minutes had passed, allowing them several extra minutes to check their answers. This extra information derived through pre-testing allowed the experimenter, once 25 minutes had elapsed, to make the following announcement:

> "If you have completed the exam and are satisfied with your answers, please place your answer sheet in the test booklet and close it."

This announcement was included in the experiment

so that subjects given the difficult test (who were for the most part not near completion of the test) would take note that several of their peers had already finished the test they assumed was identical to theirs, making the implicit failure feedback even more salient for them. On the other hand, subjects taking the easy test (who were for the most part finished with the test) would notice that several of their peers were still working feverishly to complete the test.

Once 30 minutes had elapsed, subjects were instructed to stop working, place all the test materials inside the test cover, and put this under their desks. Subjects were then given verbal and written instructions as to how the reward allocation form was to be completed. Briefly, they were told to imagine that they were providing feedback in the form of rewards (ranging from 1=very poor to 20=very good) to students who had just completed the same test they had taken. This reward allocation form was similar to that used by Weiner and Kukla (experiment 1, 1970). The allocation form consisted of 32 vignettes that reflected all possible permutations of the four attributes (gender, ability, effort, outcome) and their levels. Two levels (high-low, good deal-little, or 90% answered correctly-50% answered correctly) were

presented for three of the four student attributes (i.e., ability, effort, and outcome respectively).

Gender consisted of two levels, but each was repeated in the vignettes a second time by using prototypic male and female names (i.e., Sue-Kim, Bob-Jim). Two different randomized orders of presentation were used. Thus, there were (4 X 2 X 2 X 2) 32 allocation decisions for each subject. The following are two vignettes drawn from the allocation form:

> Bob is low in ability, put forth a good deal of effort on this test, and answered 50% of the questions correctly.
>
> Sue is high in ability, put forth little effort on this test, and answered 90% of the questions correctly.

By providing all possible permutations, each attribute was ensured an orthogonal relationship with any other (i.e., correlations among the stimulus cues were zero).

When all subjects had completed their allocations they proceeded to the next questionnaire consisting of perceived causal attribution items (ability, effort, task difficulty, luck), a social comparison item, and relative deprivation-self reported affect measures. Causal attributions and relative deprivation-affect were assessed on 5 and 7-point scales (item scale ranges are shown in Table 1) with approximately half of

the items reversed scored to attenuate subjects' acquisition of response-sets.

The social comparison item asked subjects to indicate how well they perceive they did relative to members of the student population for whom they were told the test was suited for. They did this by drawing a vertical line through a 100 mm. horizontal line with one the following anchors placed at each end:

"performed better than all (100%) other students who took this test"	"performed worse than all (0%) other students who took this test"

Also, the continuum was anchored by three percentage values (25%, 50%, 75%) to reduce scale value ambiguity.

The questionnaire used to assess the magnitude of relative deprivation -self perceived affect consisted of the items listed in Table 1. Each item is followed by its maximum scale value (in parentheses) and the abbreviated term used to identify it through the remainder of the text. Upon completion of this questionnaire, the experimenter solicited self-generated hypotheses regarding the purpose of the experiment from the subjects. It was apparent from these inquiries that subjects had no knowledge of the actual hypotheses being tested. The experimenter then debriefed the subjects as to the nature of the study.

Insert Table 1 about here

Results

For ease of presentation, all ANOVA summary tables, correlation matrices, and supplementary descriptive statistics are in Appendix A.

Overview of Results

Analyses performed to assess the strength of the manipulations revealed strong support for the non-equivalence of test difficulty and the effect of the prestige manipulations on subjects' expectancy of success. Subjects given information that portrayed the test as suited for a higher status referent had lower expectancies of success on the test. Also, when the test was portrayed as suited for a lower status referent, subjects had higher expectancies of succeeding.

Mixed support was obtained for the operation of attributional egotism and relative deprivation. Some measures indicated that the induction was successful, whereas others did not. Reduction of these measures to factor scores indicated that the induction appeared to be more successful than was revealed in individual analysis of these variables. Surprisingly, the causal attribution measures showed there was a tendency for

subjects in the high prestige-easy test condition to self-enhance, whereas the relative deprivation items seemed to indicate that these subjects were relatively gratified. Factor analysis of the attributional and relative deprivation items showed that deprivation and egotism converge, supporting the contention that causal locus is central to the definition of both constructs.

Subjects' allocation policies were collapsed within each condition and each regression coefficient was transformed to a Fisher z-score. Subjects in the high prestige-success condition weighted the outcome of hypothetical students more than other conditions with the exception of those subjects in the low prestige-failure conditions. This was the only allocation hypothesis that received support.

Manipulation Checks

A 3 X 2 unweighted means analysis of variance (ANOVA; Keppel, 1982) indicated that the prestige manipulation was effective in influencing subjects' expectancy of success on the forthcoming test ($F(2,134)=12.52, p<.001$). A t-test of the differences between the mean expectancy levels of the low prestige and high prestige conditions showed that subjects anticipating a test portrayed as suited for high school

students had significantly higher expectancies for success (mean=6.0) than those awaiting a test portrayed as suited for college juniors (mean=4.8; $t(101)=5.14, p<.001$). As shown in Figure 1, the three different test descriptions resulted in three discriminable levels of expectancy of success.

Insert Figure 1 and Table 2 about here

In order to check the test difficulty manipulation, an ANOVA was performed on three performance measures. As Table 2 indicates, subjects given a difficult test omitted significantly more questions ($F(1,151)=13.09, p=.0004$), and of those they attempted, a smaller proportion was answered correctly ($F(1,151)=18.22, p<.0001$). While subjects given the difficult test tended to answer slightly more questions correctly than those given the easy test, this difference was not significant ($F<1$). This anomoly could simply be due to the fact that the difficult test was comprised of more items. These results clearly support the non-equivalence of the two tests and the success of the perceived outcome manipulation.

A third manipulation check, social comparison, followed the test. Subjects were asked to compare

their perceived performance with that of other students for whom the test was designed (high school sophomores, college juniors, students like yourself). As shown in Figure 2, there were main effects for test prestige ($F(2,144)=8.77, p<.001$) and test difficulty ($F(1,144)=12.96, p<.001$) with no evidence of an interaction ($F<1$).

These data show that given information about a forthcoming test that was portrayed as suited for one of higher status, not only were subjects' expectancies of success more modest, but so too were their self-evaluations relative to members of the higher status reference group. On the other hand, when the test was portrayed as being suited for a lower status reference group, subjects' expectations for success and their self-evaluations relative to members of this reference group were inflated. Note that subjects given no prior information concerning a reference group for which the test was designed, yet given tests of different difficulty levels, have similar self-other evaluations. Thus, the manipulations were both effective in influencing subjects' initial expectancies, perceptions of test difficulty, and self-evaluations.

Insert Figure 2 about here

Causal Attributions

First, evidence for attributional bias (defensiveness, enhancement) was assessed via 3 X 2 unweighted means ANOVAs. Table 3 indicates that subjects' reported causal attributions showed small differences across conditions. Only in the case of ability attributions did differences across conditions reach significance. The main effect for test difficulty ($F(1,144)=4.32, p<.05$) shows that subjects who took the easy test made greater attributions to ability for their own performance (see Table 3). The magnitude of these differences was disappointing. Effort, luck, and task difficulty attributions did not differ across conditions. Thus, at best, weak support was found for the first two hypotheses.

Insert Table 3 about here

Also analyzed were concomitant variables that may have been influential in minimizing the differences obtained with the attribution measures. Certainty of

attaining level of expected success correlated r=.35 ($p<.001$) with ability, and r=-.21 ($p<.01$) with luck attributions. Perceived task importance correlated r=.26 ($p=.001$) with effort attributions. Correlations between these two covariates and the remaining attributions did not exceed r=.17. To assess the appropriateness of using certainty and task importance as covariates, a homogeneity of slopes test was performed in each case. Since violations of this assumption result in more conservative estimates of the effect (i.e., power is lost; see Peckhman, 1970), the alpha level was set at $p=.20$ (Kirk, 1968). None of the proposed ANCOVA models met this criterion, and so, no covariate analyses were performed.

Test Perceptions

Data from the dependent measures of affect provide evidence of negative affect and feelings of bias for those subjects who perceived themselves failing on a test suited for high school sophomores. For clarity, discussion of these variables is divided into two categories: 1) variables related to subjects' perceptions of the test and 2) variables indicative of affective arousal.

Table 4 presents the means for the groups on each

of the test perception items. The 3 X 2 ANOVAs performed on each of these variables revealed a prestige X test difficulty interaction ($F(2,144)=1.78, p<.05$) for subjects' perceptions of the test as an accurate measure of ability. Analysis of simple effects for test difficulty at each level of the prestige factor showed that perceptions of the test as an accurate measure of ability varied as a function of perceived success or failure only for those subjects in the low prestige conditions ($F(1,144)=5.53, p<.05$). Subjects given implicit success feedback perceived the test as a more accurate measure of their ability than subjects given failure feedback. No differences were found the remaining test perception items.

Insert Tables 4 and 5 about here

Also, subjects told the test was for higher status students, and who then found it relatively easy, responded in a similar manner. If the need to self-enhance had been stronger, the responses for these latter subjects would have been opposite those of the former. While these results were not as robust as expected, they provide modest evidence indicating that subjects in the low prestige-difficult test condition

were relatively deprived, as indicated by their responses to these test perceptions measures. But also, subjects who were predicted to self-enhance made similar responses to some of these items (i.e., the extent to which the test should be used for making decisions; the extent to which it was an accurate measure of one's true ability). In sum, limited support was obtained for the third hypothesis.

Affective Measures

As above, a 3 X 2 ANOVA was applied to each of the affective measures. A pattern of results similar to the test perception items emerged. That is, responses tended toward the hypothesized direction, but in most cases did not reach significance (see Table 5). Due to extreme heteroscedasticity (Hartley Test yielding a ratio greater than 10 to 1, $p<.01$) the item tapping blame directed at the experimenter was not used in the analyses.

Subjects' feelings after the test differed due to test difficulty ($F(1,144)=17.06, p<.001$). Subjects given the difficult test, but especially those faced with a difficult test suited for a lower status group, reported feeling less positive. This also resulted in a marginally significant main effect for test prestige

($F(2,144)=2.29, p=.11$).

However, these main effects may be a statistical artifact of the type of analysis employed. For example, Bobko (1984) showed that a multi-factorial design can yield "unwanted" main effects when analyzed in a conventional manner (in this case a 3 X 2 ANOVA). That is, when the hypothesis of interest posits an ordinal interaction exists between two independent factors, the use of conventional analyses my obscure the theoretical interpretation of results.

To cite an example, let us turn to the variable at hand: subjects' reported affective state. In this case there are six groups, but only <u>one</u> is predicted to deviate from the remaining 5. The others are, according to hypothesis #3, predicted to be essentially equivalent. That is, the null hypothesis for differences among these five groups would not be rejected. The use of a 3 X 2 ANOVA in this case yields a main effect for test difficulty, and simple effects reveal that only subjects in the low prestige condition vary as a function of test difficulty on this variable. However, no conclusive statement can be made regarding the postulated relations with these analyses. On the other hand, Bobko (1984) proposes that to adequately test for an ordinal interaction (given it is predicted

by theory) one must determine whether the five conditions that are predicted to be equivalent do indeed demonstrate equivalence (i.e., no significant differences). One manner of doing so is to perform an overall one-way ANOVA on the (5) conditions predicted to be the same. If the null hypothesis is not rejected, the second step is to pool the equivalent conditions and test the difference between this pooled estimate with that of the condition predicted to deviate from the rest (Bobko, 1984). If the null hypothesis is rejected (i.e., the conditions are not equal), then the appropriate test would be the conventional 3 x 2 ANOVA to discover where, if any, the differences lie.

This above procedure is followed here. The earlier 3 X 2 ANOVA was mentioned so that the difference between the two methods could be illustrated better. First, the conditions (excluding the low prestige -difficult test condition) predicted to be identical were subject to a one-way ANOVA. This test showed that the difference among these conditions were marginally significant ($\underline{F}(4,119)=1.96, \underline{p}=.10$) but not of sufficient magnitude to warrant abandoning further analyses. Second, these marginally different conditions were then pooled and tested in another one-

way ANOVA against the mean of the low prestige-difficult test condition. The t-test indicated that the predicted difference was highly significant ($t(38.3)=-4.14, p<.001$). It can be concluded that strong support was obtained for the hypothesis that the condition predicted to be relatively deprived was indeed in a less positive mood.

Factor Score Analysis

The data were further explored by reducing them. Initial reduction was performed by subjecting all the pre-test, affective items, causal attribution, and perceived test quality items with the exception of test appropriateness (since this item was omitted in the control conditions) to a principal-components factor analysis with varimax rotation (see Table 6). Six factors emerged in the solution. They were interpreted as follows: 1) defensive externality, 2) expectancy, 3) test quality, 4) motivation, 5) experimenter bias and, 6) task difficulty attributions.

The expectancy and task difficulty attribution factors did not reduce data sufficiently to merit further discussion; the expectancy factor was comprised

of variables that had already served as manipulation checks, whereas the task difficulty factor was defined by only one variable (task difficulty attributions) having a loading of acceptable magnitude. The experimenter bias factor yielded extreme heteroscedasticity across groups. For these reasons they were not subject to further analyses. As for the remaining three factors, a factor score coefficient matrix was generated along with subjects' factor scores.

Note the character of variables and their loadings on factor 1 (the defensive externality factor). This factor is comprised of both attributional defensiveness and relative deprivation items. Their loadings on this factor are also consonant with hypothesis 3, that attributional defensiveness and relative deprivation would converge.

Insert Tables 6 and 7 about here

Factor scores were then weighted with respect to the proportion of non-missing values present in each subjects' data set. Evidence for differences in factor scores between conditions was assessed with 3 X 2 unweighted means ANOVAs. The mean factor scores are

listed for each condition in Table 7. The 3 X 2 analysis conducted on scores for the negative externality factor resulted in a main effect for test difficulty ($F(1,144)=10.58, p=.001$). This effect can be attributed to the strong positive deviation from other scores by subjects in the low prestige -difficult test condition (see Figure 3). As above, the solution for analyzing ordinal interactions suggested by Bobko (1984) was applied to the data for this variable. The one-way ANOVA comprised of the five (excepting the low prestige-difficult test condition) conditions predicted to be equivalent showed that they indeed were the same ($F<1$). These groups were then pooled together and tested for any difference from the low prestige-difficult test condition. The t-test conducted showed that, once again, strong support was obtained for the hypothesized difference ($t(37.6)=3.81, p<.001$).

Thus, these subjects could be distinguished from the others on the basis of their scores on this defensive-externality factor. Their high score on this factor indicates that they made greater luck attributions and less attributions to ability, and attributed blame to the test for their performance. Also, they were more bitter toward the test and generally felt less pleasant. On the other hand,

subjects given the easy task, regardless of the prestige manipulation employed, attributed their performance to their ability, not luck, and felt more pleasant and less bitter towards the experimenter. This result lends further support to the idea that attributional defensiveness and relative deprivation converge.

Insert Figure 3 about here

As illustrated by the significant interaction in Figure 4 ($F(2,144)=3.55, p<.05$), the test was evaluated differently in each condition. Again, subjects who failed a test allegedly suited for a lower status group evaluated the test negatively. However, subjects given an easy test and told it was suited for a higher status group gave even more extreme negative evaluations. As shown in this figure, there was little difference between the two prestige control conditions. An analysis of the simple effects of test difficulty revealed that subjects in the high prestige condition differed as a function of perceived success or failure on the test. Surprisingly, subjects given the easy test evaluated the test significantly more negative than those given the difficult test

($F(1,144)=11.91, p<.001$).

Insert Figure 4 about here

The differences between the two test difficulty groups imbedded in the low prestige condition were marginally significant ($F(1,144)=3.73, p<.10$). Control groups did not differ ($F<1$). From these analyses then, the two prestige manipulations resulted in two outcome dependent opposite evaluations of test quality. The enhancement biases of subjects in the high prestige-easy test condition were not as pronounced as the defensive biases of subjects in the low prestige -difficult test condition.

Scores on the motivation factor did not differ significantly. For the most part, attributions to effort and perceived task importance, which comprise this factor, show no variance attributable to the manipulated factors.

These results considered together then, show that subjects' perceptions of the test, cognitive evaluations, and affective responses were different across conditions. While the results are not as robust as had been anticipated, it remains to be seen whether these differences matter for subsequent reward

allocations.

Reward Allocations

Mean allocations and averaged within-group judgment standard deviations are presented in Table 8. Once again, 3 X 2 ANOVAs were performed, showing no significant differences in mean allocations. However, the plotted group means in Figure 6 show that there is superficial support (statistically nonsignificant) for the hypothesis that subjects in the low prestige-difficult test condition tended to be more generous raters than their colleagues. Somewhat surprising is the discovery that average allocations differed among test difficulty levels in the low prestige condition, though this interaction was not significant. Also, the test difficulty factor did not result in any differences in the mean allocations of subjects in the remaining prestige conditions. For both the control and high prestige condition, there was no difference between test difficulty levels.

Insert Table 8 about here

To assess rater use of the four stimulus cues, Pearson correlations between each cue value and

allocation magnitude were computed. Since predictors were orthogonal, correlations were equivalent to standardized partial regression coefficients (i.e., Betas; Kerlinger & Pedhazur, 1973). Betas were then transformed to Fisher (1921,1970) z-scores. As noted by Fisher (1970), the transformation of correlation coefficients to z-scores results in a z distribution which "will in all ordinary circumstances be normal" (p.201). These z-values were then subjected to 3 X 2 unweighted means ANOVAs.

Examination of the extent to which subjects relied on the four ratee stimulus cues yielded some intriguing, yet unclear findings. For example, a near significant interaction between prestige and test difficulty in the extent to which subjects used gender of the ratee in their allocation decisions emerged ($F(2,147)=2.98, p<.10$). To investigate this further, a Pearson correlation was computed between rater sex and the Fisher z-score of the sex of ratee cue. These variables were found to be uncorrelated ($r=0$), and so sex was not further considered as a possible covariate. What may have been operative was a bias due to sex-role orientation, which since not assessed here would remain undetected.

There were no significant effects attributable to

the manipulations for ability Betas. This is in contrast to the hypothesis that subjects in the low prestige-difficult test condition would make greater use of ability information. Instead, group means for the ability Beta were largely indiscriminant with a tendency for subjects in the low prestige-difficult test condition to altogether omit this cue as an element of their allocation policy.

Effort and outcome Betas on the other hand, both show significant prestige X test difficulty interaction effects ($F(2,147)=3.70, p<.05$ and, $F(2,147)=4.10, p<.05$). Effort cues played a larger role in the judgment policy of subjects taking the easy test designed for a lower status reference group. To understand this better, simple effects for test difficulty were investigated. It was found that only subjects given a test portrayed for a lower status group differed in the use of effort cues as a function of test difficulty ($F(1,144)=8.47, p<.01$). This result, too, is contrary to what was expected. A plausible explanation might be that these subjects also made greater attributions to effort for their own performance. However, this was not the case at all. The ordinal position of the conditions as plotted in Figure 5 are just the reverse with regard to effort attributions. It is possible

also that this condition may have been comprised of a disproportionate number of subjects high in some individual difference attribute. For example, Stake (1983) showed that subjects high in Protestant Ethic endorsement tended to place greater weight on effort information in their allocations. If such is the case here, then individual differences seem to have over-riden the effect a brief 30-minute test coupled with status manipulations could muster.

Insert Figures 5 and 6 about here

The significant prestige X test difficulty interaction for outcome Betas provided partial support for my hypothesis (see Figure 6) but simultaneously provided contradictory evidence. As expected, subjects given an easy test suited for a high status group attributed more importance to outcome information on their judgments. This was also confirmed through simple effects analyses conducted across test difficulty levels. This analysis showed that subjects in the high prestige condition differed in their use of this cue as a function of test difficulty ($\underline{F}(1,144)=4.57, \underline{p}<.05$). However, this pattern was marginally significant in the opposite direction for

scores in the low prestige condition as well ($F(1,144)=3.07, p<.10$). This latter discovery is just the opposite of what was predicted. Perhaps in this case, there was a tendency for subjects experiencing either euphoria at having succeeded at the test or disappointment and anger for failing to have less discriminating policies. It appears that these subjects may have simply latched on to the most salient cue and based their allocations upon it alone, or at best minimally entertained others. As shown in Table 9, conditions were not equivalent in the extent to which subjects used the various cues. The allocation policies represented by these equations imply that subjects whose affect has been aroused positively or negatively, tend to utilize the diversity of cues to a lesser extent. This was further investigated by testing the difference in the percent of variance of each subjects' policy that could be predicted from the sum of the cues.

Insert Table 9 about here

A 3 X 2 ANOVA conducted on the coefficient of determination for subjects' judgment policies showed that subjects' overall judgment was better predicted

with the established cues in the easy test condition ($F(1,152)=3.09, p=.08$). That is, differences in this measure correspond to the extent to which subjects did employ the available information and not something else in making their decisions. The more a subject used the multiplicity of information provided, the larger the coefficient of determination for that subject's policy. Of interest here is the finding that subjects experiencing a positive mood state did not tend to use short-cuts in their allocation decisions. Instead, it appears that test difficulty was most influential in determining the extent to which subjects attended to the cues. Note that these differences stem from initial cognitive demands placed on the subjects, and not from an aroused emotional state.

Discussion

This study was intended to serve as a test of the similarity between attributional egotism and felt injustice. This convergence was alluded to in prior theoretical discussions (Messick & Sentis, 1983; Utne & Kidd, 1980) but has never been empirically investigated. The results of this study should be considered cautiously. The attributional egotism induced in this study was of modest magnitude and is not free of expectancy confirmation biases. However, while some measures were operationally imprecise, this exploratory endeavor illustrated general convergence between the concepts studied.

The prestige and outcome manipulations, when considered seperately, succeeded in differentiating between levels of each factor. The effect of the manipulations appeared to be less powerful when tests for the induction of attributional egotism and relative deprivation were conducted. The mixed support obtained for the operation of egotism and deprivation was somewhat disappointing. Thus, the marginality of the differences found for subjects' allocation preferences should be treated with caution. It is likely that these differences are under-estimated in the results of

this study due to a task that may not have been particularly ego-involving for these subjects. The self-esteem threat posed by failure on the test may have been relatively modest when compared to more successful inductions of attributional egotism (e.g., Miller, 1976). Also, the present laboratory manipulationsdid not possess the "meaning" that real-life events do. The task importance measures given following the prestige manipulation also indicate that subject involvement may have been low. Subjects generally reported that success on the test was of moderate importance. But relative to what? Another experimental task, graduating, getting a job? The ambiguity of this item provides no firm indication of involvement.

Likewise, the induction of relative deprivation seems to have been modest. Perhaps the assumed need or desire of subjects to succeed was overestimated. This could have diminished any felt injustice on behalf of the subjects and so resulted in less discrimination between conditions on these measures. Some measure of aspiration level, perhaps of a relative nature, could have been useful as a covariate in the experiment. This measure could have assessed each subject's acceptable or desirable level of performance relative

to the reference group they were told the test was suited for. In this way, subjects' expectancy disconfirmation would be made more salient and this could possibly have resulted in more intense feelings of injustice.

Contrary to the above interpretation, subjects may have discounted the importance of the test and the implicit feedback that they were failing. If such were the case, then self-esteem defenses may have operated to distance subjects from the test and the implications of failure. However, the self-evaluation measure indicates that subjects did attend to both manipulated factors; if subjects hadn't attended to the explicit (prestige) and implicit (performance) information, then the results obtained would not have followed.

Given that egotism and deprivation were operative, what were the implications for subsequent reward allocations? First of all, the hypothesized differences in average allocations did not emerge. Subjects in the different conditions were generally equivalent in terms of generosity. Though the difference was not significant, subjects in the low prestige-easy test condition did tend to employ more liberal allocation policies. Again, had the egotism and relative deprivation induction been more

successful, there is reason to believe these differences would have reached significance.

There were no differences in the extent to which subjects used the ability cue in their allocations. This might have been mediated by cultural values that in our society augment the use of effort information in merit decisions and discount the importance of ability information (Weiner & Kukla, 1970). If, as mentioned above, the induction of egotism and relative deprivation were only moderately successful, then it is unlikely that such modest effects could overcome established cultural biases.

ANOVA results for the other two cues reveal significant interactions attributable to the prestige and outcome factors. Effort cues covaried to a greater extent with the composite allocation of subjects in the low prestige-easy test condition. The covariation of the effort cue with the composite allocation was identical among remaining conditions. This result cannot be explained by the factors employed in the experiment. Rather, this may be due to an individual difference such as Protestant Ethic endorsement. There may have been a disproportionate number of individuals high on this attribute in the low prestige-easy test condition. If so, and since individuals high in

Protestant Ethic endorsement give greater weight to volitional factors, such a result would not be surprising. Unfortunately, since differences in this attribute were not assessed, no data were available to ascertain that this was the case.

Outcome information was weighted most heavily by subjects in two diametric conditions (low prestige-difficult test, high prestige-easy test). Recall that subjects in the high prestige-easy test condition were predicted to weight this cue most heavily. However, subjects in the low prestige-difficult test condition were not predicted to do the same. Why would individuals choose a criterion of merit that would place themselves, if they were the recipient, at a disadvantage relative to others? Also, why would they do this once having been the victim of a prior injustice? It could be argued that the least ambiguous criterion was used by these subjects in an effort to ensure objectivity for subsequent decisions. But is this really a viable explanation?

Rather, I offer the following reason: first, these subjects had just completed a difficult cognitive exercise (the difficult test), and second, they had just had their self-esteem threatened, and so were in an aroused state. Together, these two factors may

account for their weighting this cue so heavily.

For example, subjects may have merely wished to maintain a favorable self-image; an image of impartiality and fairness. Perhaps their exercise of impartiality or adherence to an objective standard (outcome) reflected a need for them to recoup the loss their self-esteem had suffered due to their performance on the test. In this way, the allocation exercise provided an opportunity to prove to themselves their own self-worth. In a sense, it could be referred to as a "substitute activity" (Lewin, 1926) that fulfills the release of tension instigated by the achievement context, but thwarted by the failure to succeed.

The results also indicate that the equations representing the allocation policies of subjects were less accurate, paramorphically, for those subjects given a difficult test. Arousal alone cannot explain this finding. Rather, a more parsimonious explanation is that subjects who had taken the difficult test were more fatigued from the effort required to solve its problems and so spent less effort in attending to information during the allocation exercise.

The results generally support the contention that defensive styles of ascribing causality and the relative deprivation concept converge in the

explanation of events and affective arousal. This could foreshadow the development of more rigorous tests directed at discovering greater convergence between justice theories and attribution theories (e.g., Carroll & Payne, 1976). Also requiring more attention are the occassions in which they are divergent, such as relative gratification and self-enhancement. In any case, this has illustrated one instance in which attribution theory could be further extended into the domain of justice research.

A finding with greater implications is that allocation preferences are not synonymous across individuals who have had different experiences with a task. However, unlike previous research (Mitchell & Kalb, 1982), no clear-cut pattern emerged among these consequences that facilitates recommendations of how organizationally undesirable allocation preferences can be avoided. What this study did illustrate though, was, that allocation preferences differ as a result of experience and may have implications for employee morale and consequently, organizational effectiveness. What this study did not reveal, though, was if these preferences can be reliably predicted.

This last point illustrates the greatest limit of this study. The absence of results that contribute to

any clear statement regarding the effects of these manipulations jeopardizes the tenability of the present findings. On the one hand, it can be argued that these factors play no role in biasing distribution practices. On the other hand, even if supportive results had been obtained, it can be argued that the measures used were imprecise. What is needed to solve this dilemma is a more rigorous test endowed with greater experimental realism and a more conclusive design.

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Tables and Figures

Table 1

Experimental Questionnnaire Items.

Pre-Examination Items

1. Relative to your past performance on such tasks, how well do you expect to perform on this test? (9) Expectancy Level

2. How confident are you of achieving this level of performance? (9) Certainty of Expected Level of Success

3. How important is it for you to feel that you have succeeded at this task? (9) Importance of Success

Test Quality

4. To what extent do you feel this test was of the appropriate difficulty for the student population it was designed for? (5) Test Appropriateness

5. To what extent do you feel that a person's score on this test would accurately reflect their true ability? (5) True Ability

6. To what extent do you feel that this test should be used to make decisions about students? (5) Decisions

7. How unbiased was this test at measuring your ability? (7) Test Bias

Table 1 continued...

Negative Affect

8. To what degree is the test to blame for your performance?
   (7) Blame Test

10. How much is the experimenter to blame for your performance?
    (7) Blame Experimenter

11. How bitter are you about the test?
    (7) Bitter at Test

12. How bitter are you about the experimenter?
    (7) Bitter at Experimenter

13. How would you characterize your feelings after taking this test? (7) Present feelings

14. If you were told that you had to take this test again, how happy or unhappy would you be? (7) Unhappy Anticipation

Table 2

Performance means and standard deviations for the two test forms

	Difficult Test	Easy Test
Omissions	9.0 (12.3)	2.2 * (11.2)
Percent answered correctly	43.4 (13.0)	52.7 ** (13.9)
Correct answers	20.3 (10.5)	18.7 (10.0)

* = $p<.001$
** = $p<.0001$

Table 3

Group means and standard deviations for performance attributions

	Ability	Effort	Task	Luck
High Prestige				
Difficult Test	3.32 (.95)	3.27 (.83)	3.23 (1.07)	2.45 (.74)
Easy Test	3.55 (.69)	3.48 (.63)	2.76 (.74)	2.52 (.78)
Low Prestige				
Difficult Test	3.42 (.70)	3.15 (.83)	3.27 (.78)	2.57 (.76)
Easy Test	3.61 (.99)	3.04 (.93)	3.26 (.75)	2.04 (.77)
Control				
Difficult Test	3.56 (.65)	3.20 (.82)	2.92 (.91)	2.28 (.68)
Easy Test	3.92 (.57)	3.32 (.63)	2.92 (.95)	2.20 (.65)

Table 4

Mean scores and standard deviations for test perception measures

	Test Appropriateness	True Ability	Decisions	Bias
High Prestige				
Difficult Test	3.27 (.88)	2.59 (.73)	2.27 (.88)	3.86 (1.75)
Easy Test	2.62 (.90)	2.38 (.78)	1.97 (.68)	3.48 (1.40)
Low Prestige				
Difficult Test	3.19 (1.09)	2.19 (.75)	2.12 (.77)	3.92 (1.22)
Easy Test	3.17 (.83)	2.70 (.76)	2.22 (.88)	3.61 (1.44)
Control				
Difficult Test	0.00 ----	2.68 (.75)	2.20 (.58)	4.04 (1.31)
Easy Test	0.00 ----	2.60 (.71)	2.36 (.86)	3.80 (1.26)

Note. This item was omitted from questionnaires used by control groups.

Table 5

Means and standard deviations of affective responses for each group

	Bitter at Experimenter	Bitter at Test	Blame Test	Blame Experimenter
High Prestige				
Difficult Test	1.27 (.88)	2.09 (1.51)	3.77 (1.19)	1.86 (1.61)
Easy Test	1.07 (.37)	1.90 (1.26)	3.44 (1.64)	1.45 (1.27)
Low Prestige				
Difficult Test	1.27 (.83)	3.15 (1.71)	4.12 (1.28)	2.57 (2.30)
Easy Test	1.43 (1.37)	2.13 (1.52)	3.30 (1.72)	1.96 (1.82)
Control				
Difficult Test	1.20 (.41)	2.08 (1.26)	3.52 (1.45)	1.72 (1.28)
Easy Test	1.08 (.28)	2.16 (1.77)	3.28 (1.46)	1.20 (.65)

Table 5 continued...

	Present Feelings	Unhappy Anticipation
High Prestige		
Difficult Test	3.95 (.79)	4.95 (1.42)
Easy Test	4.45 (1.09)	4.95 (1.19)
Low Prestige		
Difficult Test	3.38 (.94)	4.42 (1.70)
Easy Test	4.30 (1.14)	4.38 (1.24)
Control		
Difficult Test	3.92 (.81)	4.68 (1.19)
Easy Test	4.46 (1.19)	5.04 (1.17)

Note. The items "Bitter at Experimenter", and "Blame Experimenter" were omitted from the analyses due to extreme heteroscedasticity.

Table 6

Principal-components varimax-rotated factor matrix

	Factors					
	1	2	3	4	5	6
Blame Test	75	11	02	08	-07	-13
Present Feelings	-73	-01	21	08	05	-29
Luck Attributions	61	-27	-23	-08	-08	-13
Ability Attributions	-45	39	31	37	-13	04
Bitter at Test	69	03	-08	11	22	23
Expectancy of Success	05	78	08	-10	-03	-11
Outcome Certainty	-02	72	12	15	-06	-01
True Ability	-13	01	84	01	02	-08
Decision	-14	18	76	00	01	14
Effort Attributions	-01	-09	10	76	-04	18
Task Importance	10	26	-15	69	02	-08
Blame Experimenter	29	20	-09	-36	52	14
Bitter at Experimenter	-05	-20	09	-04	77	-05
Unhappy Anticipation	34	-43	15	-01	-48	-03
Task Diff. Attributions	17	-19	13	22	04	80
Test Bias	36	-23	15	24	43	-45

Note. Decimal points have been omitted.

Table 7

Means and standard deviations of factor scores for each group

Factor	Defensiveness	Test Quality	Motivation	Experimenter
High Prestige				
Difficult Test	.10 (.94)	.62 (2.32)	.27 (2.44)	.44 (2.35)
Easy Test	-.26 (1.10)	-.33 (1.03)	.17 (.79)	-.29 (.61)
Low Prestige				
Difficult Test	.64 (.92)	.06 (2.14)	.38 (2.11)	.69 (2.30)
Easy Test	-.23 (1.06)	.60 (2.23)	.15 (2.45)	.71 (2.36)
Control				
Difficult Test	.04 (.80)	.10 (.99)	.09 (1.10)	.77 (.77)
Easy Test	-.29 (.95)	.27 (1.00)	.15 (.70)	-.29 (.58)

Table 8

Mean and standard deviation of allocations for each group

	Mean Allocation	Standard Deviation[a]
High Prestige		
Difficult Test	13.44	1.51
Easy Test	13.43	1.89
Low Prestige		
Difficult Test	14.05	1.42
Easy Test	12.94	1.67
Control		
Difficult Test	13.44	1.86
Easy Test	13.21	2.25

[a]This was obtained by calculating a standard deviation for each subject and then averaging these within each condition.

Table 9

Standardized coefficients of the multiple regression equation representing the average allocation policy of subjects in each group.

High Prestige		
	Difficult Test	Y = -.022(sex) + .027(can) + .405(try) + .692(outcome)
	Easy Test	Y = -.031(sex) - .012(can) + .428(try) + .815(outcome)
Low Prestige		
	Difficult Test	Y = +.008(sex) - .025(can) + .405(try) + .804(outcome)
	Easy Test	Y = -.013(sex) - .017(can) + .650(try) + .696(outcome)
Control		
	Difficult Test	Y = -.033(sex) - .007(can) + .552(try) + .695(outcome)
	Easy Test	Y = +.011(sex) + .129(can) + .478(try) + .766(outcome)

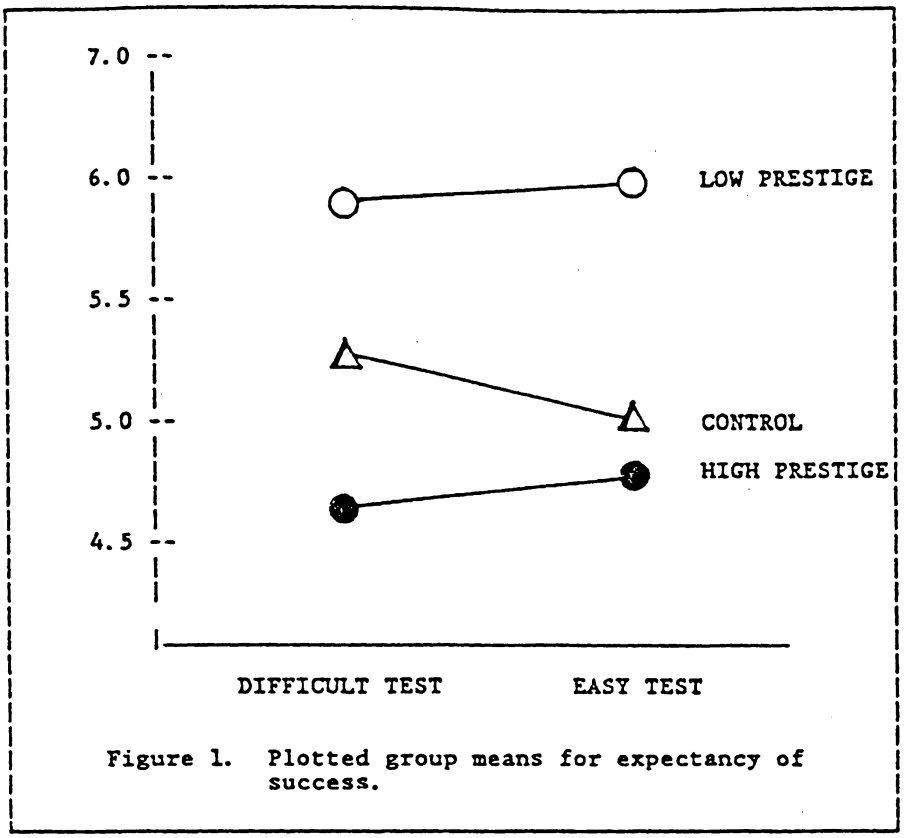

Figure 1. Plotted group means for expectancy of success.

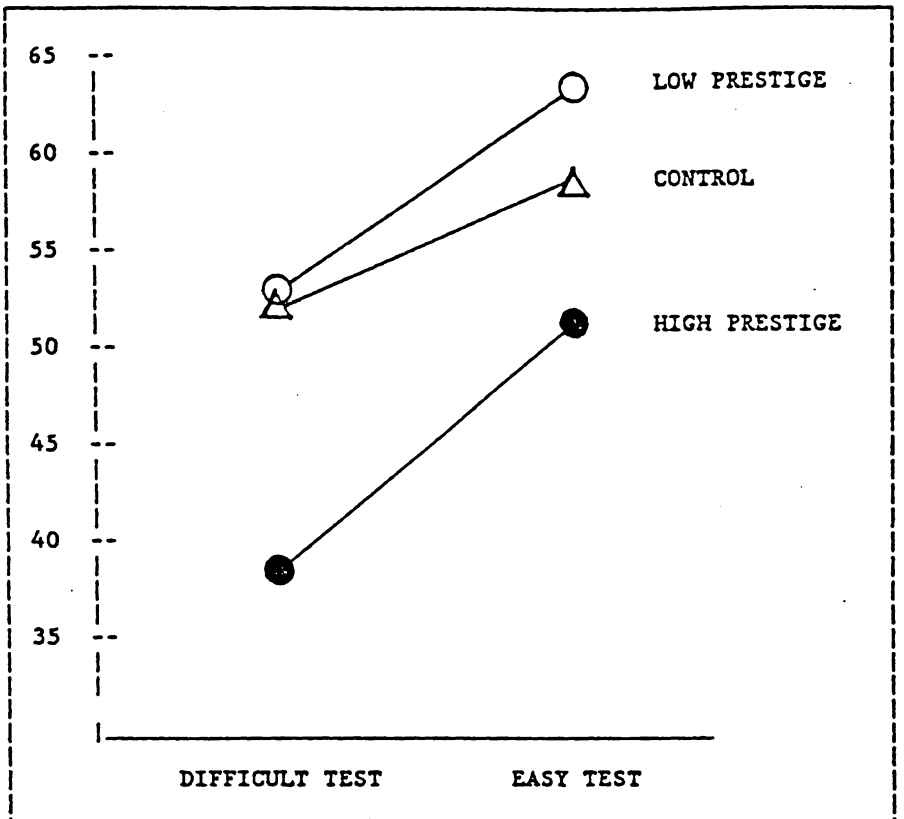

Figure 2. Plotted group percentile means for self-evaluation relative to targeted reference group.

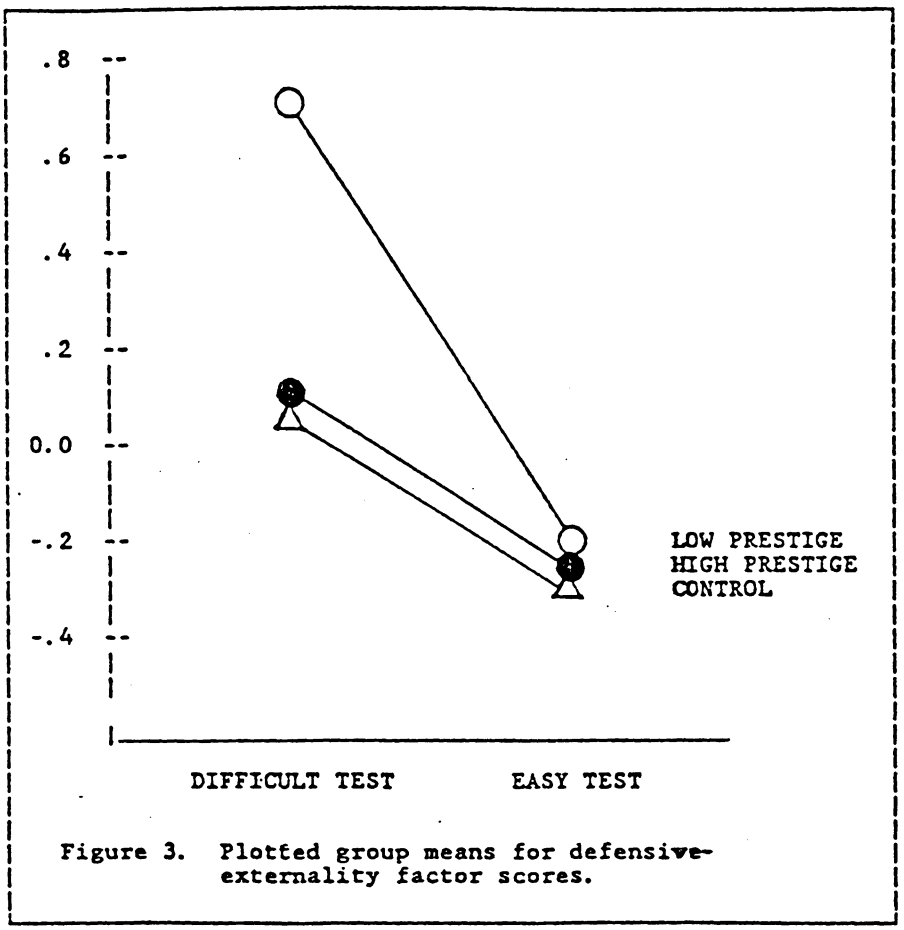

Figure 3. Plotted group means for defensive-externality factor scores.

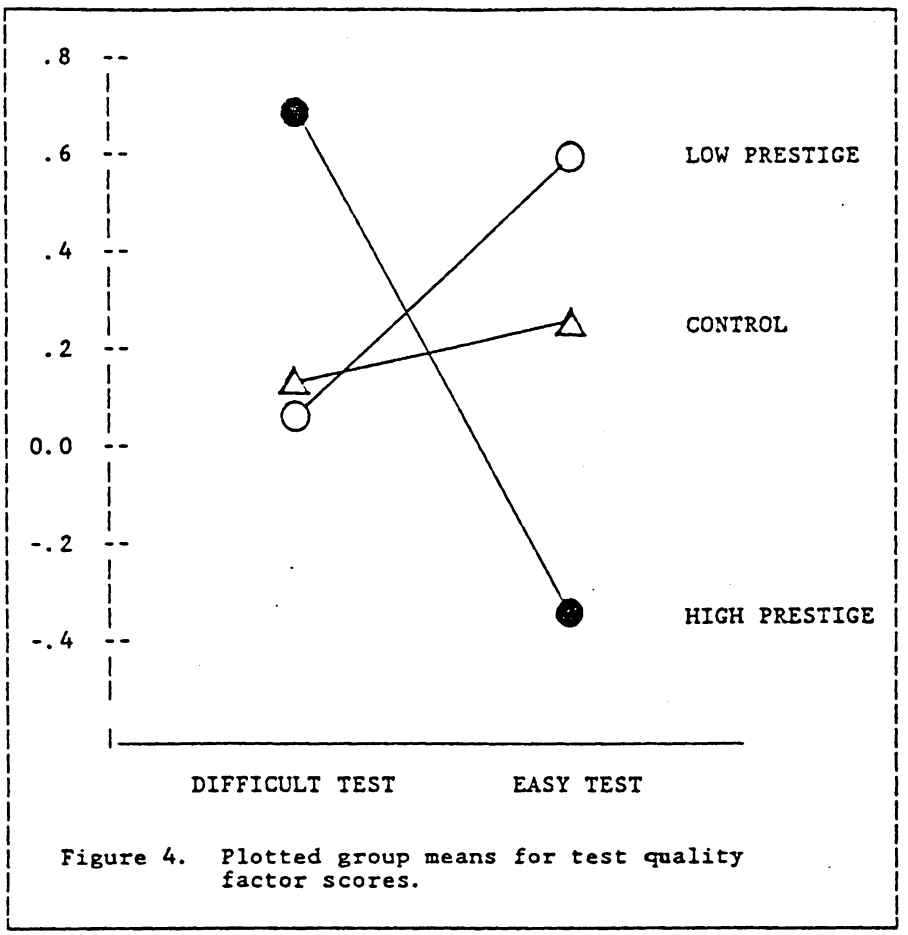

Figure 4. Plotted group means for test quality factor scores.

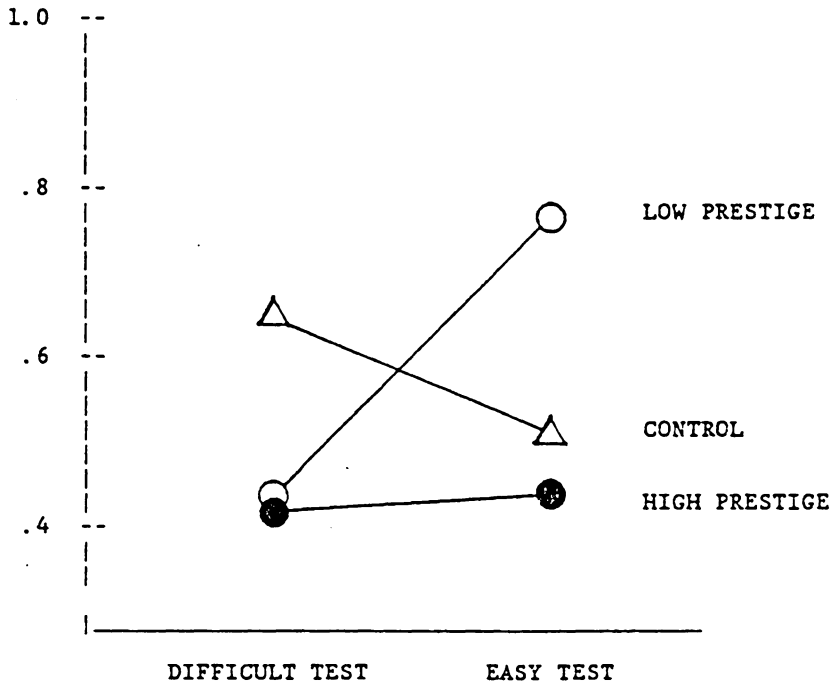

Figure 5. Plotted group mean Fisher z-scores for effort Betas.

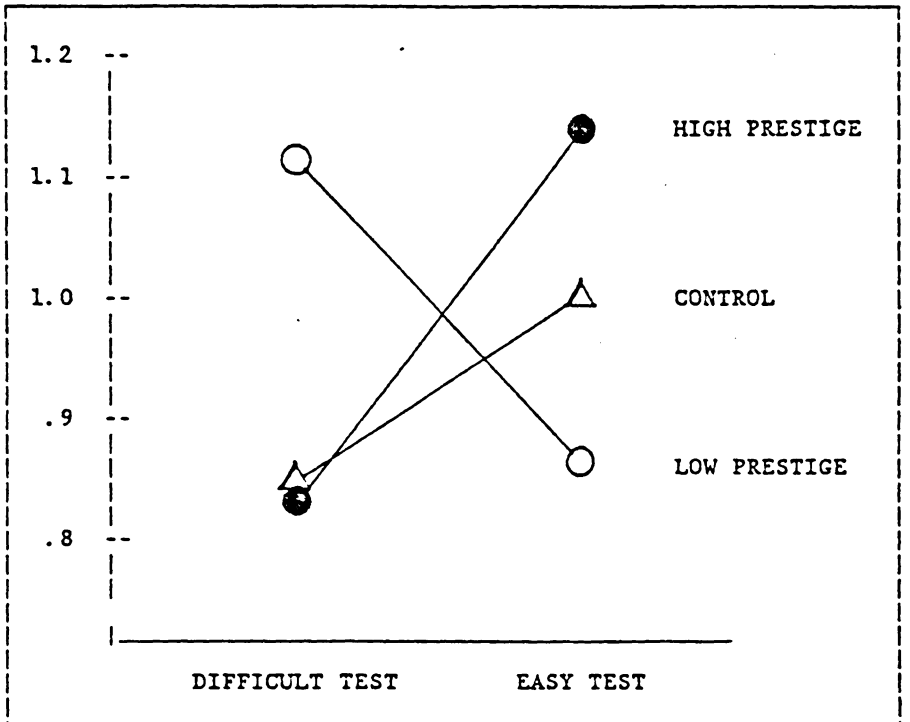

Figure 6. Plotted group mean Fisher z-scores for outcome Betas.

APPENDIX A

APPENDIX A

Key to Variable Labels Used in ANOVA Summary Tables

Variable Name	Abbreviation in Appendix
Independent Variables	
Prestige Manipulation Factor	PRESTIGE
Test Difficulty Factor	TASK
Dependent Variables	
Expectancy of Success	EXPECT
Certainty of Success	SEFFMAG
Task Importance	IMPORT
Ability Attributions	ABILITY
Effort Attributions	EFFORT
Task Difficulty Attributions	TASKDIFF
Luck Attributions	LUCK
Test Appropriateness	STUPOPAB
True Ability	TRUEABIL
Decisions	DECISION
Test Bias	BIAS
Blame Test	BLAMETST
Blame Experimenter	BLAMEXP
Bitter at Test	BITTERT
Bitter at Experimenter	BITTERX
Present Feelings	FEELINGS
Unhappy Anticipation	UNHAPPY
Defensive Externality Factor	NEGAFF
Test Quality Factor	TESTQUAL
Motivation Factor	MOTIVE
Experimenter Bias Factor	EXPTR
Mean Allocation	MEANALLO
___ Standardized Partial Regression Coefficients ___ for Stimulus Cues Transformed to Fisher z-scores	
Sex of Hypothetical Students	ZSEX
Ability of Hypothetical Students	ZCAN

Effort of Hypothetical Students	ZTRY
Outcome of Hypothetical Students	ZOUTCOME
Coefficient of Determination	RSQUARE

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

EXPECT
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF		MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	28.665	3	K-1	9.555	8.372	0.000
PRESTIGE	28.569	2	K-1	14.284	12.516	0.000
TASK	0.008	1	K-1	0.008	0.007	0.934
2-WAY INTERACTIONS	1.699	2	(a-1)(b-1)	0.849	0.744	0.477
PRESTIGE TASK	1.699	2		0.849	0.744	0.477
EXPLAINED	29.950	5		5.990	5.248	0.000
RESIDUAL	152.935	134		1.141		
TOTAL	182.885	139	n-1	1.316		

153 CASES WERE PROCESSED.
13 CASES (8.5 PCT) WERE MISSING.

- - - - - - - - - - - - - - ONE WAY ANOVA - - - - - - - - - - - - - -

VARIABLE EXPECT

BY VARIABLE PRESTIGE

SOURCE	D.F.	SS	MS	F RATIO	PROB.
BETWEEN GROUPS	2	37.907	18.85	16.82	0.000
WITHIN GROUPS	150	169.033	1.12		
total	152	206.940			

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/24/84) PROGRAM

\- O N E W A Y - - - - - - - -

VARIABLE EXPECT
BY VARIABLE PRESTIGE

CONTRAST COEFFICIENT MATRIX

	GRP01	GRP02	GRP03
CONTRAST 1	0.0	1.0	-1.0
CONTRAST 2	1.0	-0.5	-0.5

	VALUE	S. ERROR	POOLED VARIANCE ESTIMATE T VALUE	D.F.	T PROB.
CONTRAST 1	1.1919	0.2092	5.697	150.0	0.000
CONTRAST 2	-0.2044	0.1830	-1.117	150.0	0.266

TESTS FOR HOMOGENEITY OF VARIANCES

COCHRANS C = MAX. VARIANCE/SUM(VARIANCES) = 0.4824, P = 0.013 (APPROX.)
BARTLETT-BOX F = 5.855, P = 0.003
MAXIMUM VARIANCE / MINIMUM VARIANCE = 2.704

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

SEFFMAG
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	10.836	3	3.612	1.468	0.226
PRESTIGE	10.815	2	5.407	2.197	0.115
TASK	0.000	1	0.000	0.000	0.995
2-WAY INTERACTIONS	5.885	2	2.943	1.196	0.306
PRESTIGE TASK	5.885	2	2.943	1.196	0.306
EXPLAINED	15.972	5	3.194	1.298	0.268
RESIDUAL	329.768	134	2.461		
TOTAL	345.740	139	2.487		

153 CASES WERE PROCESSED.
13 CASES (8.5 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

IMPORT
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	11.956	3	3.985	1.542	0.207
PRESTIGE	6.543	2	3.271	1.266	0.285
TASK	4.933	1	4.933	1.908	0.169
2-WAY INTERACTIONS	14.265	2	7.132	2.759	0.067
PRESTIGE TASK	14.265	2	7.132	2.759	0.067
EXPLAINED	25.014	5	5.003	1.935	0.093
RESIDUAL	346.380	134	2.585		
TOTAL	371.395	139	2.672		

153 CASES WERE PROCESSED.
13 CASES (8.5 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/05/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

OMITTED
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	2499.195	3	833.065	6.092	0.001
PRESTIGE	685.273	2	342.637	2.506	0.085
TASK	1660.191	1	1660.191	12.141	0.001
2-WAY INTERACTIONS	63.932	2	31.966	0.234	0.792
PRESTIGE TASK	63.932	2	31.966	0.234	0.792
EXPLAINED	2537.266	5	507.453	3.711	0.003
RESIDUAL	20100.664	147	136.739		
TOTAL	22637.930	152	148.934		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/05/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

PERCENT
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	0.412	3	0.137	7.624	0.000
PRESTIGE	0.082	2	0.041	2.272	0.107
TASK	0.306	1	0.306	16.984	0.000
2-WAY INTERACTIONS	0.008	2	0.004	0.211	0.810
PRESTIGE TASK	0.008	2	0.004	0.211	0.810
EXPLAINED	0.418	5	0.084	4.642	0.001
RESIDUAL	2.649	147	0.018		
TOTAL	3.067	152	0.020		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/09/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

CORRECT
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	366.669	3	128.890	1.211	0.308
PRESTIGE	285.238	2	143.119	1.345	0.264
TASK	92.212	1	92.212	0.867	0.353
2-WAY INTERACTIONS	43.453	2	21.727	0.204	0.816
PRESTIGE TASK	43.453	2	21.727	0.204	0.816
EXPLAINED	424.543	5	84.909	0.798	0.553
RESIDUAL	15639.516	147	106.391		
TOTAL	16064.059	152	105.685		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

RELSUC
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	9212.961	3	3070.987	9.399	0.000
PRESTIGE	5730.801	2	2865.400	8.770	0.000
TASK	4232.918	1	4232.918	12.955	0.000
2-WAY INTERACTIONS	417.888	2	208.944	0.639	0.529
PRESTIGE TASK	417.883	2	208.944	0.639	0.529
EXPLAINED	9456.254	5	1891.251	5.788	0.000
RESIDUAL	47051.344	144	326.745		
TOTAL	56507.598	149	379.246		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

ABILITY
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	4.834	3	1.611	2.770	0.044
PRESTIGE	2.405	2	1.248	2.145	0.121
TASK	2.510	1	2.510	4.315	0.040
2-WAY INTERACTIONS	0.201	2	0.101	0.173	0.841
PRESTIGE TASK	0.201	2	0.101	0.173	0.841
EXPLAINED	5.064	5	1.013	1.741	0.129
RESIDUAL	83.769	144	0.582		
TOTAL	88.832	149	0.596		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

EFFORT
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	2.250	3	0.750	1.236	0.299
PRESTIGE	1.938	2	0.969	1.597	0.206
TASK	0.199	1	0.199	0.329	0.567
2-WAY INTERACTIONS	0.673	2	0.336	0.554	0.576
PRESTIGE TASK	0.673	2	0.336	0.554	0.576
EXPLAINED	2.987	5	0.597	0.985	0.429
RESIDUAL	87.385	144	0.607		
TOTAL	90.372	149	0.607		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

TASKDIFF
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	4.383	3	1.461	1.941	0.126
PRESTIGE	3.257	2	1.628	2.163	0.119
TASK	0.941	1	0.941	1.250	0.265
2-WAY INTERACTIONS	1.792	2	0.896	1.190	0.307
PRESTIGE TASK	1.792	2	0.896	1.190	0.307
EXPLAINED	6.269	5	1.254	1.666	0.147
RESIDUAL	108.403	144	0.753		
TOTAL	114.672	149	0.770		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

LUCK
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	2.678	3	0.893	1.668	0.176
PRESTIGE	1.604	2	0.802	1.499	0.227
TASK	1.254	1	1.254	2.344	0.128
2-WAY INTERACTIONS	2.386	2	1.193	2.230	0.111
PRESTIGE TASK	2.386	2	1.193	2.230	0.111
EXPLAINED	5.235	5	1.047	1.957	0.089
RESIDUAL	77.038	144	0.535		
TOTAL	82.272	149	0.552		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

* A N A L Y S I S O F V A R I A N C E *

TESTS OF SIGNIFICANCE FOR ABILITY USING UNIQUE SUMS OF SQUARES

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIG. OF F
WITHIN+RESIDUAL	60.60615	130	.46620		
CONSTANT	53.83562	1	53.83562	115.47722	0.0
SEFFMAG	10.80750	1	10.80750	23.18206	0.0
PRESTIGE	.54538	2	.27269	.58492	.559
TASK	.09969	1	.09969	.21384	.645
PRESTIGE BY TASK	8.05564	2	4.02782	8.63966	.000
SEFFMAG BY PRESTIGE + SEFFMAG BY TASK + SEFFMAG BY PRESTIGE BY TASK	8.85913	5	1.77183	3.80056	.003

ESTIMATES FOR ABILITY

CONSTANT

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
1	2.4792254960	.23071	10.74603	0.0	2.02279	2.93566

SEFFMAG

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
2	.1845815875	.03834	4.81477	.000	.10874	.26043

PRESTIGE

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
3	-.3603162007	.33901	-1.06284	.290	-1.03101	.31038
4	.2786675420	.34134	.81640	.416	-.39662	.95396

TASK

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
5	-.1066878504	.23071	-.46243	.645	-.56312	.34975

* A N A L Y S I S O F V A R I A N C E *

TESTS OF SIGNIFICANCE FOR EFFORT USING UNIQUE SUMS OF SQUARES

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIG. OF F
WITHIN+RESIDUAL	72.54712	133	.54547		
CONSTANT	57.70138	1	57.70138	105.78344	0.0
IMPORT	6.03924	1	6.03924	11.07168	.001
PRESTIGE	.82152	2	.41076	.75305	.473
TASK	1.99734	1	1.99734	3.66171	.058
PRESTIGE BY TASK	2.88274	2	1.44637	2.65162	.074
IMPORT BY PRESTIGE + IMPORT BY TASK + IMPORT BY PRESTIGE BY TASK	4.68540	5	.93708	1.71794	.135

ESTIMATES FOR EFFORT

CONSTANT

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
1	2.4815140962	.24127	10.28511	0.0	2.00429	2.95874

IMPORT

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
2	.1326822304	.03988	3.32741	.001	.05381	.21155

PRESTIGE

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
3	-.3012728641	.34875	-.86387	.389	-.99108	.38854
4	-.0722632533	.35336	-.20450	.838	-.77120	.62667

TASK

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
5	-.4616891592	.24127	-1.91356	.058	-.93892	.01554

* A N A L Y S I S O F V A R I A N C E *

TESTS OF SIGNIFICANCE FOR LUCK USING UNIQUE SUMS OF SQUARES

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIG. OF F
WITHIN+RESIDUAL	62.79082	130	.48307		
CONSTANT	72.64759	1	72.64759	150.38797	0.0
SEFFMAG	2.85142	1	2.85142	5.90272	.016
PRESTIGE	.08823	2	.04412	.09132	.913
TASK	.16592	1	.16592	.34347	.559
PRESTIGE BY TASK	4.77175	2	2.38588	4.93901	.009
SEFFMAG BY PRESTIGE + SEFFMAG BY TASK + SEFFMAG BY PRESTIGE BY TASK	7.18994	5	1.43799	2.97678	.014

ESTIMATES FOR LUCK

CONSTANT

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
1	2.8799953641	.23485	12.26328	0.0	2.41538	3.34461

SEFFMAG

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
2	-.0948104004	.03902	-2.42955	.016	-.17201	-.01761

PRESTIGE

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
3	.0979531617	.34509	.28385	.777	-.58477	.78067
4	-.1492476992	.34746	-.42667	.670	-.83565	.53915

TASK

PARAMETER	COEFF.	STD. ERR.	T-VALUE	SIG. OF T	LOWER .95 CL	UPPER .95 CL
5	-.1376350206	.23485	-.58606	.559	-.60225	.32698

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

STUPOPAR
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	4.589	2	2.295	2.606	0.079
PRESTIGE	1.381	1	1.381	1.568	0.214
TASK	2.777	1	2.777	3.153	0.079
2-WAY INTERACTIONS	2.480	1	2.480	2.817	0.097
PRESTIGE TASK	2.480	1	2.480	2.817	0.097
EXPLAINED	7.306	3	2.435	2.766	0.046
RESIDUAL	84.533	96	0.881		
TOTAL	91.839	99	0.928		

153 CASES WERE PROCESSED.
53 CASES (34.6 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

TRUEABIL
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	1.246	3	0.415	0.743	0.528
PRESTIGE	1.061	2	0.531	0.949	0.390
TASK	0.185	1	0.185	0.332	0.566
2-WAY INTERACTIONS	3.565	2	1.782	3.189	0.044
PRESTIGE TASK	3.565	2	1.782	3.189	0.044
EXPLAINED	4.980	5	0.996	1.782	0.120
RESIDUAL	80.493	144	0.559		
TOTAL	85.473	149	0.574		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

DECISION
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	0.697	3	0.232	0.417	0.741
PRESTIGE	0.678	2	0.339	0.609	0.545
TASK	0.010	1	0.010	0.018	0.893
2-WAY INTERACTIONS	1.802	2	0.801	1.438	0.241
PRESTIGE TASK	1.802	2	0.801	1.438	0.241
EXPLAINED	2.465	5	0.493	0.885	0.493
RESIDUAL	79.641	143	0.557		
TOTAL	82.106	148	0.555		

153 CASES WERE PROCESSED.
4 CASES (2.6 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

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BIAS
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	5.376	3	1.792	0.915	0.435
PRESTIGE	1.557	2	0.778	0.397	0.673
TASK	3.570	1	3.570	1.822	0.179
2-WAY INTERACTIONS	0.124	2	0.062	0.032	0.969
PRESTIGE TASK	0.124	2	0.062	0.032	0.969
EXPLAINED	5.581	5	1.116	0.570	0.723
RESIDUAL	280.108	143	1.959		
TOTAL	285.688	148	1.930		

153 CASES WERE PROCESSED.
4 CASES (2.6 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

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BLAMETST
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	10.430	3	3.477	1.606	0.191
PRESTIGE	2.481	2	1.241	0.573	0.565
TASK	7.822	1	7.822	3.612	0.059
2-WAY INTERACTIONS	2.338	2	1.169	0.540	0.584
PRESTIGE TASK	2.338	2	1.169	0.540	0.584
EXPLAINED	12.854	5	2.571	1.187	0.318
RESIDUAL	311.835	144	2.166		
TOTAL	324.689	149	2.179		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

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BLAMEXP
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	28.735	3	9.595	3.902	0.010
PRESTIGE	17.438	2	8.719	3.546	0.031
TASK	10.007	1	10.007	4.069	0.046
2-WAY INTERACTIONS	0.260	2	0.130	0.053	0.949
PRESTIGE TASK	0.260	2	0.130	0.053	0.949
EXPLAINED	29.067	5	5.813	2.364	0.043
RESIDUAL	354.098	144	2.459		
TOTAL	383.166	149	2.572		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

BITTERT
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	18.204	3	6.068	2.650	0.051
PRESTIGE	11.638	2	5.819	2.542	0.082
TASK	5.352	1	5.352	2.336	0.128
2-WAY INTERACTIONS	8.127	2	4.064	1.775	0.173
PRESTIGE TASK	8.127	2	4.064	1.775	0.173
EXPLAINED	26.672	5	5.334	2.330	0.045
RESIDUAL	329.694	144	2.290		
TOTAL	356.366	149	2.392		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

BITTERX
BY PRESTIGE
TASK

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SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	1.436	3	0.479	0.814	0.488
PRESTIGE	1.292	2	0.646	1.096	0.337
TASK	0.103	1	0.103	0.176	0.676
2-WAY INTERACTIONS	0.923	2	0.462	0.784	0.459
PRESTIGE TASK	0.923	2	0.462	0.784	0.459
EXPLAINED	2.340	5	0.468	0.794	0.555
RESIDUAL	84.833	144	0.589		
TOTAL	87.173	149	0.585		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

FEELINGS
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	23.267	3	7.756	7.590	0.000
PRESTIGE	4.681	2	2.340	2.290	0.105
TASK	17.433	1	17.433	17.060	0.000
2-WAY INTERACTIONS	1.155	2	0.578	0.565	0.569
PRESTIGE TASK	1.155	2	0.578	0.565	0.569
EXPLAINED	24.350	5	4.870	4.766	0.000
RESIDUAL	147.149	144	1.022		
TOTAL	171.499	149	1.151		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/25/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

UNHAPPY
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	6.981	3	2.327	1.313	0.273
PRESTIGE	6.595	2	3.297	1.860	0.159
TASK	0.157	1	0.157	0.088	0.767
2-WAY INTERACTIONS	2.177	2	1.089	0.614	0.543
PRESTIGE TASK	2.177	2	1.089	0.614	0.543
EXPLAINED	9.775	5	1.755	0.990	0.426
RESIDUAL	251.731	142	1.773		
TOTAL	260.506	147	1.772		

153 CASES WERE PROCESSED.
5 CASES (3.3 PCT) WERE MISSING.

VARIABLE	EST COMMONALITY	FACTOR	EIGENVALUE	PCT OF VAR	CUM PCT
EXPECT	1.00000	1	3.10824	19.4	19.4
SEFFMAG	1.00000	2	1.79245	11.2	30.5
IMPORT	1.00000	3	1.55285	9.7	40.3
TRUEABIL	1.00000	4	1.38982	8.7	49.1
EFFORT	1.00000	5	1.09657	6.9	56.0
LUCK	1.00000	6	1.00818	6.3	62.2
ABILITY	1.00000	7	0.91519	5.7	68.0
TASKDIFF	1.00000	8	0.79364	5.0	72.9
DECISION	1.00000	9	0.74053	4.6	77.5
BIAS	1.00000	10	0.63971	4.0	81.5
BLAMEIST	1.00000	11	0.62612	3.9	85.4
BLAMEAP	1.00000	12	0.56321	3.5	89.0
FEELINGS	1.00000	13	0.55730	3.5	92.5
BITTERT	1.00000	14	0.41965	2.6	95.2
BITTERA	1.00000	15	0.40042	2.5	97.7
UNHAPPY	1.00000	16	0.36604	2.3	100.0

	FACTOR 1	FACTOR 2	FACTOR 3	FACTOR 4	FACTOR 5	FACTOR 6
EXPECT	0.05078	0.77592	0.07822	-0.09620	-0.02931	-0.10812
SEFFMAG	-0.01964	0.72273	0.11633	0.14953	-0.05997	-0.00734
IMPORT	0.09566	0.25802	-0.15113	0.69736	-0.03645	0.18026
TRUEABIL	-0.15569	0.01961	0.84254	0.01230	0.02035	-0.07730
EFFORT	-0.00941	-0.09990	0.10119	0.75867	-0.04221	0.00999
LUCK	0.61177	-0.47264	-0.23285	-0.08374	-0.07541	-0.13394
ABILITY	-0.45068	0.34905	0.31042	0.37000	-0.12980	0.03961
TASKDIFF	0.16829	-0.18363	0.12699	0.22141	0.04159	0.79635
DECISION	-0.13896	0.17756	0.75530	0.00406	0.01008	0.14059
BIAS	0.36514	-0.42690	0.15394	0.25519	0.42770	-0.45360
BLAMETST	0.74889	0.10952	0.01585	0.07964	-0.06533	-0.13446
BLAMEXP	0.29233	0.20048	-0.09390	-0.35091	0.52472	0.14196
FEELINGS	-0.72981	-0.00729	0.20756	0.07287	0.05399	-0.29408
BITTENT	0.69293	0.02479	-0.08291	0.10917	0.21447	0.23302
BITTERX	-0.05450	-0.19734	0.08979	-0.03525	0.76729	-0.04728
UNHAPPY	0.34172	-0.43378	0.15147	-0.01388	-0.47882	-0.02084

TRANSFORMATION MATRIX

	FACTOR 1	FACTOR 2	FACTOR 3	FACTOR 4	FACTOR 5	FACTOR 6
FACTOR 1	-0.01099	0.56464	0.41989	0.17637	-0.10192	-0.05896
FACTOR 2	0.42103	0.37746	0.11680	0.58879	-0.11253	0.30923
FACTOR 3	0.10781	0.09256	-0.14097	-0.49645	0.46259	-0.16952
FACTOR 4	0.20152	-0.22186	0.89159	0.13861	0.04256	-0.00580
FACTOR 5	0.32503	0.00217	0.04568	-0.44582	-0.57574	-0.25452
FACTOR 6	0.81450	0.62373	-0.11993	0.41198	0.01548	-0.89871

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/08/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

NEGAFF
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	13.819	3	4.606	4.902	0.003
PRESTIGE	3.194	2	1.597	1.700	0.186
TASK	9.939	1	9.939	10.577	0.001
2-WAY INTERACTIONS	2.249	2	1.124	1.197	0.305
PRESTIGE TASK	2.249	2	1.124	1.197	0.305
EXPLAINED	16.087	5	3.217	3.424	0.006
RESIDUAL	135.311	144	0.940		
TOTAL	151.398	149	1.016		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/08/84) PROGRAM

* * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

TESTQUAL
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	2.166	3	0.722	0.731	0.535
PRESTIGE	2.086	2	1.043	1.057	0.350
TASK	0.094	1	0.094	0.095	0.758
2-WAY INTERACTIONS	7.010	2	3.505	3.551	0.031
PRESTIGE TASK	7.010	2	3.505	3.551	0.031
EXPLAINED	9.658	5	1.932	1.957	0.089
RESIDUAL	142.130	144	0.987		
TOTAL	151.788	149	1.019		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/08/84) PROGRAM

* * * * * * * * * * ANALYSIS OF VARIANCE * * * * * * * * * *

MOTIVE
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	1.668	3	0.556	0.546	0.652
PRESTIGE	1.577	2	0.788	0.774	0.463
TASK	0.071	1	0.071	0.070	0.792
2-WAY INTERACTIONS	2.452	2	1.226	1.204	0.303
PRESTIGE TASK	2.452	2	1.226	1.204	0.303
EXPLAINED	4.027	5	0.805	0.791	0.558
RESIDUAL	146.674	144	1.019		
TOTAL	150.701	149	1.011		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 05/08/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

EXPTR
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	8.824	3	2.941	2.942	0.035
PRESTIGE	6.756	2	3.378	3.379	0.037
TASK	1.618	1	1.618	1.619	0.205
2-WAY INTERACTIONS	0.618	2	0.309	0.309	0.735
PRESTIGE TASK	0.618	2	0.309	0.309	0.735
EXPLAINED	9.404	5	1.881	1.881	0.101
RESIDUAL	143.961	144	1.000		
TOTAL	153.365	149	1.029		

153 CASES WERE PROCESSED.
3 CASES (2.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * ANALYSIS OF VARIANCE * * * * * * * * * *

MEANALLO
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	7.771	3	2.590	0.809	0.491
PRESTIGE	1.734	2	0.867	0.271	0.763
TASK	5.988	1	5.988	1.869	0.174
2-WAY INTERACTIONS	9.658	2	4.829	1.508	0.225
PRESTIGE TASK	9.658	2	4.829	1.508	0.225
EXPLAINED	17.772	5	3.554	1.110	0.358
RESIDUAL	470.904	147	3.203		
TOTAL	488.677	152	3.215		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

ZSEX
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	0.015	3	0.005	0.984	0.402
PRESTIGE	0.015	2	0.007	1.438	0.241
TASK	0.001	1	0.001	0.158	0.692
2-WAY INTERACTIONS	0.031	2	0.016	2.984	0.054
PRESTIGE TASK	0.031	2	0.016	2.984	0.054
EXPLAINED	0.048	5	0.010	1.857	0.105
RESIDUAL	0.765	147	0.005		
TOTAL	0.813	152	0.005		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

ZCAN
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	0.248	3	0.083	2.021	0.113
PRESTIGE	0.154	2	0.077	1.875	0.157
TASK	0.104	1	0.104	2.535	0.113
2-WAY INTERACTIONS	0.114	2	0.057	1.396	0.251
PRESTIGE TASK	0.114	2	0.057	1.396	0.251
EXPLAINED	0.363	5	0.073	1.770	0.122
RESIDUAL	6.021	147	0.041		
TOTAL	6.384	152	0.042		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

ZTRY
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	0.986	3	0.329	1.894	0.133
PRESTIGE	0.741	2	0.370	2.134	0.122
TASK	0.308	1	0.308	1.775	0.185
2-WAY INTERACTIONS	1.284	2	0.642	3.701	0.027
PRESTIGE TASK	1.284	2	0.642	3.701	0.027
EXPLAINED	2.268	5	0.454	2.614	0.027
RESIDUAL	25.508	147	0.174		
TOTAL	27.776	152	0.183		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSES

FILE RELDEP (CREATION DATE = 04/30/84) PROGRAM

* * * * * * * * * ANALYSIS OF VARIANCE * * * * * * * * *

ZOUTCOME
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	0.301	3	0.100	0.429	0.733
PRESTIGE	0.104	2	0.052	0.223	0.801
TASK	0.186	1	0.186	0.796	0.374
2-WAY INTERACTIONS	1.916	2	0.958	4.098	0.019
PRESTIGE TASK	1.916	2	0.958	4.098	0.019
EXPLAINED	2.267	5	0.453	1.939	0.091
RESIDUAL	34.362	147	0.234		
TOTAL	36.628	152	0.241		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

RELDEP ANALYSIS

FILE RELDEP (CREATION DATE = 06/02/84) PROGRAM

* * * * * * * * * * A N A L Y S I S O F V A R I A N C E * * * * * * * * * *

RSQUARE
BY PRESTIGE
TASK

* *

SOURCE OF VARIATION	SUM OF SQUARES	DF	MEAN SQUARE	F	SIGNIF OF F
MAIN EFFECTS	0.082	3	0.027	1.622	0.186
PRESTIGE	0.028	2	0.014	0.826	0.440
TASK	0.052	1	0.052	3.085	0.081
2-WAY INTERACTIONS	0.017	2	0.009	0.505	0.605
PRESTIGE TASK	0.017	2	0.009	0.505	0.605
EXPLAINED	0.102	5	0.020	1.208	0.308
RESIDUAL	2.477	147	0.017		
TOTAL	2.579	152	0.017		

153 CASES WERE PROCESSED.
0 CASES (0.0 PCT) WERE MISSING.

PEARSON CORRELATION COEFFICIENTS - - - - - - - - - - - - - - - - - - -

	RELSUC	STUPOPAB	TRUEABIL
EXPECT	.41 (153) p=.000	.12 (100) p=.123	.01 (150) p=.429
SEFFMAG	.29 (145) p=.000	-.06 (95) p=.288	.12 (142) p=.070
IMPORT	.05 (148) p=.291	-.11 (97) p=.152	-.06 (145) p=.247

- -

	EFFORT	LUCK	ABILITY	TASKDIFF
EXPECT	-.088 (150) p=.141	-.111 (150) p=.089	.222 (150) p=.003	-.139 (150) p=.046
SEFFMAG	.035 (142)	-.209 (142)	.352 (142)	-.018 (142)
IMPORT	.265 (145) p=.001	-.056 (145) p=.252	.147 (145) p=.038	.168 (145) p=.022

PEARSON CORRELATION COEFFICIENTS- - - - - - - - - - - - - - - - - - -

	ZSEX	ZCAN	ZTRY	ZOUTCOME
SEX OF SUBJECT	.007 (153) p=.465	-.048 (153) p=.283	-.067 (153) p=.205	-.024 (153) p=.384

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	ZCAN	ZTRY	ZOUTCOME
ABILITY	-.094 (150) p=.127	-.118 (150) p=.085	.117 (150) p=.077
EFFORT	-.043 (150) p=.299	.015 (150) p=.427	.040 (150) p=.314
LUCK	-.078 (150) p=.171	.107 (150) p=.096	-.026 (150) p=.369
TASKDIFF	-.092 (150) p=.132	-.005 (150) p=.477	-.021 (150) p=.398

PEARSON CORRELATION COEFFICIENTS - - - - - - - - - - - - - - - - -

	ZCAN	ZTRY	ZOUTCOME
DECISION	.073	-.102	-.011
	(150)	(150)	(150)
	p=.187	p=.107	p=.445
BIAS	.133	.030	.009
	(149)	(149)	(149)
	p=.053	p=.357	p=.454

- -

	ZCAN	ZTRY	ZOUTCOME
FEELINGS	.014	-.057	.041
	(150)	(150)	(150)
	p=.429	p=.245	p=.309
UNHAPPY	-.163	.015	.103
	(148)	(148)	(148)
	p=.024	p=.427	p=.106

PEARSON CORRELATION COEFFICIENTS- - - - - - - - - - - - - - - - - - -

	MEANALLO	ZCAN	ZTRY	ZOUTCOME
NEGAFF	.133 (150) p=.052	.053 (150) p=.262	.019 (150) p=.407	-.026 (150) p=.374
TESTQUALITY	.009 (153) p=.454	.107 (153) p=.094	-.087 (153) p=.144	-.008 (153) p=.461

PEARSON CORRELATION COEFFICIENTS- - - - - - - - - - - - - - - - - - -

	MEANALLO	ZCAN	ZTRY	ZOUTCOME
MOTIVE	.118 (153) p=.073	.051 (153) p=.266	-.041 (153) p=.306	-.026 (153) p=.375

ANOVA Tables for Simple Effects Analyses
-only significant ($p<.10$) effects are listed-

True Ability

		SS	DF	MS	F	SIG
	Outcome Effects	3.09	1	3.09		
LOW PRESTIGE					5.53	$p<.05$
	Residual	80.49	144	.56		

Test Quality

		SS	DF	MS	F	SIG
	Outcome Effects	11.75	1	11.75		
HIGH PRESTIGE					11.91	$p<.001$
	Residual	151.78	144	.99		
	Outcome Effects	3.69	1	3.69		
LOW PRESTIGE					3.73	$p<.10$
	Residual	151.78	144	.99		

Effort Beta
(transformed to Fisher z-score)

		SS	DF	MS	F	SIG
	Outcome Effects	1.46	1	1.46		
LOW PRESTIGE					8.47	$p<.01$
	Residual	25.51	144	.17		

Outcome Beta
(transformed to Fisher z-score)

		SS	DF	MS	F	SIG
	Outcome Effects	1.07	1	1.07		
HIGH PRESTIGE					4.57	$p<.05$
	Residual	34.36	144	.234		
	Outcome Effects	.72	1	.72		
LOW PRESTIGE					3.07	$p<.10$
	Residual	34.36	144	.234		

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