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Social loafing, social facilitation and self-efficacy theory: Judgments of control in a group psychokinesis task

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SOCIAL LOAFING, SOCIAL FACILITATION AND SELF-EFFICACY THEORY: JUDGMENTS OF CONTROL IN A GROUP PSYCHOKINESIS TASK

BY

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DISSERTATION

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DEDICATION

I dedicate this dissertation to my children, Owen and Kara, neither of whom were around to see the beginning of this journey, and to Bonz, who was there every step of the way.

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

ABSTRACT

SOCIAL LOAFING, SOCIAL FACILITATION AND SELF-EFFICACY THEORY: JUDGMENTS OF CONTROL IN A GROUP PSYCHOKINESIS TASK

by

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This dissertation examines the effects of self-efficacy and group size on the judgments of control of individual subjects in a group situation. It was predicted that highly efficacious subjects would report more judged control, inefficacious subjects would report less judged control. Group size was predicted to affect judgments of control in a manner consistent with the social loafing and social facilitation literature, such that individually unidentifiable subjects should "loaf" and report less control. Individually identifiable subjects should find their prior efficacy beliefs facilitated, such that high believers feel increasing control with increasing group size and low believers feel decreasing control with increasing group size. Experiment 1 measured subjects' judged control over the outcome of a die toss in a situation designed to engender social loafing (subjects' output was indistinguishable from that of other group members). Prior belief in psi abilities reliably predicted judged control, but group size was not a significant predictor. Neither prior belief nor group size significantly affected judged own or other effort. Experiment 2 measured subjects' judged control over a die toss ina situation designed to produce social facilitation (subjects' output was

individually identifiable). Prior belief in psi abilities reliably predicted judged control; group size was positively related to judged group control, but not to individual measures of control or to effort. Results were consistent with previous literature in self-efficacy, and somewhat consistent with loafing and facilitation research.

INTRODUCTION

This dissertation is about individuals' perception of control in a group situation, and how it is affected by the size of the group and the identifiability of the individual's output. Two separate areas of social psychology are involved in this problem: The psychology of control and efficacy, and the psychology of group presence and its influence on the individual.

A person attempting to determine whether his or her efforts affect any change in the physical or social environment is faced with a sometimes simple, sometimes nearly impossible task. Recognizing cause and effect relationships, especially those involved in personal control, can be as simple as recognizing that flicking a switch turns on a lamp, or as difficult as recognizing that a comment made in passing several days ago has caused a change in a friend's behavior today (Jenkins & Ward, 1965). Causation and control may be difficult to judge even when only one actor is involved (e.g., when a weekend mechanic tries to tune up an engine by trial and error), but such judgments are always made more difficult when there are two or more actors working separately at the same problem. When there are others working on the same task, even simple cause and effect relationships may present problems. If I am alone, and flick a switch and a light comes on, I know that I caused the event to occur. If two of us are flicking switches (or at least might be flicking switches), I cannot know for certain (without communicating with the other party) that I was the one who turned on the light. On a much larger scale, how can one member of a nationwide group (whose members logistically cannot know what all other members are doing) determine that what he or she is doing makes a difference? A member will act (write a letter, sign a petition, vote, etc.), and at some time in the future,

there may be a change in his or her environment (a speech on the Senate floor, the end of the Cold War, the election of a candidate to office, etc.). Has the individual's action had some effect? If there is no change seen in the environment, this lack of change could be seen as evidence that the action was ineffectual; on the other hand, it could also simply reflect the difficulty in perceiving, at this enormous scale, a change that did happen. If a change is seen, this could be seen as the direct result of the individual's action; it could also be seen as an unrelated event coincidental to the individual's action, or as the direct result of some unknown actor's influence. That people continue to work at such tasks (to the extent that they do) may not reflect any perception that they have made a difference, but rather "the critical determiner of involvement [may be] one's belief in the possibility of effective personal action" (Fuld & Nevin, 1988). The people who believe they had some control over an event may be those people who believed--before the event even began--that they had the ability to control it.

The present set of experiments examined individuals' judgments of control in situations analogous to the aforementioned large group, situations in which judgments of control come not so much from the perception of some objectively determinable control, but perhaps more from belief in control, a belief that one person's vote counts, that one's own efforts on any scale are not useless. Chapter 1 consists of a review of research literature on control, efficacy, social loafing, social facilitation, and the methodology of the present experiments. Chapter 2 presents the methodology, results and discussion of Experiment 1. Experiment 1 examined the effect of group size and prior belief in psi abilities on judgments of control in a situation where individual output is not identifiable, a situation which should engender social loafing (a decline in effort exerted by individual group members as

group size increases). Chapter 3 presents the methodology, results and discussion of Experiment 2. Experiment 2 examined the effects of group size and prior belief in psi abilities on judgments of control in a situation in which individual output is identifiable, a condition which should engender social facilitation (the enhancement of the individual's dominant response as group size increases). Finally, chapter 4 presents a discussion of the results of both experiments.

CHAPTER I

LITERATURE REVIEW

Judged Efficacy and Judged Control

Bandura has twice (1982, 1986) pointed out the lack of research into group efficacy, a topic closely related to group judgment of control; virtually no research exists in this neglected area. As the present experiments will involve both judgments of personal efficacy and judgments of personal and collective control, a description of efficacy and control is warranted.

Judged self-efficacy is an individual's belief that he or she has the capability to perform a given task (Bandura, 1977, 1984); judged control is the individual's belief that he or she has successfully exercised control in a specific situation or generally (Skinner, 1985). These definitions are by no means universally agreed upon; various authors have used the terms interchangeably, or have introduced different terms altogether. Terms such as mastery, sense of agency, perceived contingency, levels of personal helplessness, and sense of competence have been advanced for what is essentially judged control (Thompson & Spacapan, 1991); the term self-confidence has been used as a synonym for self-efficacy (Weinberg, Gould, Yukelson, & Jackson, 1981).

Judged Efficacy

Judged self-efficacy may be seen as a partial determinant of performance; judged control must be assessed only following a given performance. The relation between the two concepts may be reciprocal: increased self-efficacy is associated with behavior patterns (e.g., intensified

effort in the face of failure, perseverance, etc.. See Bandura, 1984 for a review of these and related findings.) which lead to increased success, and past judgments of success may alter current perceptions of self-efficacy (Bandura, 1984). In a typical series of experiments, Bandura, Reese and Adams (1982) induced self-efficacy (through enactive mastery--breaking down the feared task into smaller sequential units which were individually mastered at a manageable pace--and through modeling) in persons suffering from snake phobias (Experiment 1) and spider phobias (Experiments 2 and 3), then measured performance at handling the creatures. In snake-phobic subjects, self-efficacy was a better predictor of post-test performance than was performance during treatment; in spider-phobic subjects, greater self-efficacy was predictive of lower levels of fear and physiological arousal. In each experiment, increased self-efficacy led to conditions favorable to greater success. In the first experiment, experience with success in "easily mastered steps" (p. 8) was used in inducing higher self-efficacy (although Experiments 2 and 3 show that behavioral success is not the only way to induce self-efficacy). The reciprocal nature of efficacy and control (or at least success in controlling one's behavior; whether it is perceived as control is not discussed) is evident (Bandura et al., 1982).

Self-efficacy for a given task includes judgments of ability to perform not only the mechanical aspects of the task, but also the cognitive and social aspects of the task (Bandura, 1984). Self-efficacy for computer programming, for instance, might encompass judgments of the mechanical ability to type on a keyboard, the cognitive ability to comprehend and create computer programs logically, and the social ability to withstand the strain on one's personal life inherent in such a time-consuming task.

Research on the association between self-efficacy and performance (reviewed in Bandura, 1984) suggests that self-efficacy is a mediator between past experience and present performance. When self-efficacy is artificially manipulated through false feedback prior to a performance, illusory self-efficacy significantly affects performance both positively (when self-efficacy was enhanced) and negatively (when self-efficacy was lowered). In an empirical test of Bandura's (1977) self-efficacy theory, subjects were asked to compete at a muscular endurance task (holding one leg extended horizontally, while seated, for as long as possible) under conditions of artificially manipulated self-efficacy. For low self-efficacy subjects, the student against whom they would be competing (a confederate) identified himself or herself as a member of the track team who lifts weights in order to strengthen the leg muscles. Faced with such an opponent, subjects reported little expectation of keeping their leg extended longer than their opponent (low efficacy). High efficacy subjects were confronted with an opponent who claimed to have a knee injury. Faced with this opponent, subjects felt confident that they could keep their leg extended longer than he or she could (high efficacy). The experiment arranged for subjects in both conditions to lose the first trial (because perseverance in the face of defeat is, in theory, dependent on one's level of self-efficacy) and to lose again on the second trial, in order to measure how long they were able to keep their leg elevated after the initial defeat. Subjects whose self-efficacy had been enhanced (high self-efficacy) kept their legs raised significantly longer than those whose self-efficacy had been lowered (low self-efficacy); moreover, there was a significant efficacy by trials interaction, such that high self-efficacy subjects increased their times on the second trial (after losing the first trial) while low self-efficacy subjects' times fell (Weinberg, Gould, & Jackson, 1979). A replication

(Weinberg, Gould, Yukelson, & Jackson, 1981) showed that both preexisting self-efficacy and the artificially manipulated self-efficacy employed in these experiments significantly predicted performance at the leg-raising task. In the replication, however, preexisting self-efficacy significantly influenced only the first trial, and the artificially manipulated self-efficacy significantly influenced only the second. Weinberg et al. (1981) suggest that the effects of manipulated self-efficacy were felt more strongly during the second trial of the experiment because preexisting self-efficacy partially masked the manipulated self-efficacy until failure in the first trial made the manipulated self-efficacy more salient.

Judged Control

Judged control and perceived control[1] are concerned with the degree to which individuals or groups affect events. Various researchers have proposed different definitions of control, usually emphasizing the attempts by individuals to actively change their environment to better suit their needs (Rothbaum, Weisz, & Snyder, 1982). Averill (1973) distinguishes three different types of personal control: behavioral, cognitive and decisional. Behavioral control is "the availability of a response which may directly influence or modify the objective characteristics of a[n]...event" (Averill, 1973, pp 286-287). Cognitive control involves interpretation or classification of (perhaps behaviorally uncontrollable) events into an understandable form; Rothbaum et al. (1982) call this form of control "interpretive control."

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[1]Judged control and perceived control are virtually identical concepts, with one important difference--the availability of an objective degree of control. When there is some objective degree of control, the term "perceived control" is used to refer to the accuracy with which this objective control is perceived (e.g., Schorr & Rodin, 1982). When no objective degree of control exists, a subject's (sometimes illusory) feelings of control are termed "judged control" (e.g., Jenkins & Ward, 1965). There are also many researchers who inaccurately use "judged control" as a synonym for judged efficacy (e.g., Wallston, Wallston, Smith & Dobbins, 1987).

Decisional control refers to "the opportunity to choose among various courses of action" (Averill, 1973, p 287). Although all three types of control have been examined experimentally, behavioral control and its perception are most frequently studied and are of greatest importance to the present study.

Various aspects of control have been investigated, including the illusion of control (when perceptions of control exceed the objective control; Langer, 1975), learned helplessness ("the psychological state that frequently results when events are uncontrollable"; Seligman, 1975, p. 9), locus of control (whether people feel they control events in their lives, or whether these events are perceived as being controlled by other forces (including institutions, other people, or chance; Rotter, 1975), and the relationship between judged control and various actions (including health; Wallston et al., 1987; Schorr & Rodin, 1982) or states (e.g., depression, Alloy & Abramson, 1979). Thompson and Spacapan (1991) noted that all this and more, including control ideology, powerlessness, and self-efficacy are at times subsumed into the "control" literature, which may also split control into categories based on type of control or even based on what it is that the individual is attempting to exert control over.

Laboratory studies of individuals' perceptions of control have for the most part employed a task in which two dichotomous variables are presented (subject's action and the resulting feedback), the covariation of which the subjects are to determine (e.g., Jenkins & Ward, 1965). This is a very simple task, involving the actions of one person and the results of those actions. Crocker (1981) divided the control judgment task into six steps, "(1) decide what kinds of data to collect, (2) sample cases from the population of cases, (3) interpret the cases (i.e., code the data), (4) recall the data that have been

collected and estimate the frequencies of confirming and disconfirming cases, (5) integrate the evidence, and (6) use the estimate as a basis for making predictions or judgments" (p. 273). In a control judgment experiment these steps are often clarified for the subject. For example, the instructions for Newman and Benassi's (1989) control-judgment experiment "explained in detail the task's objective (to learn what degree of control the subject had over the onset of the blue light on the two problems), the subject's required activity, the four outcomes possible on each trial, the concept of control, and the values of the control scale" (p. 878). The subjects in this type of experiment have access, at least in theory, to all the information necessary to make the several steps needed to arrive at a correct perception of their individual control. This is not necessarily the case if there are two or more subjects working on the task at the same time. Without complete knowledge of the actions of the other subjects at the task, a subject cannot check confirming and disconfirming cases[2] (Crocker, 1981), and thus cannot make an accurate assessment of covariation or control.

Other laboratory experiments of perceived control (e.g., Chan, Karbowski, Monty, & Perlmuter, 1986; Perlmuter & Chan, 1983) have exposed subjects to a paired-associate task in which they either did or did not have decisional control over the response words to be used; the effects of perceived control were then measured as subjects' expectations of success on a chance activity (a dice game). Additionally, one study (Chan et al., 1986) gave some subjects control over white noise played over headphones during the trials.

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[2] Confirming cases are those in which a subject's actions and the outcome of the trial indicate control: acting and getting a response, or not acting and not getting a response. Disconfirming cases indicate a lack of control: acting and not getting a response, or not acting and getting a response (Crocker, 1981).

The noise would cease temporarily when the choice of response word was made, enhancing the salience of the decisional control. This method has the advantage of allowing measurement of several variables which might have some relation to the judgment of control, including decision time (for choosing the response word) expectancy for success at the dice game, bet size at the dice game, and memory performance at the paired-associate task, as well as self-report questionnaires about perception of control during the experiment. Again, however, the method does not lend itself neatly to the study of control judgments in groups, as both the paired-associate task and the dice game are individual activities which are necessarily complicated by the actions of another subject. Also, these were experiments in decisional control rather than behavioral control, and the two may not be positively correlated. Although decisional control led to increased expectancies for success in the dice game,there were other measures for which increased control seemed to have a negative effect. For decision time in choosing the response word, Perlmuter and Chan (1983) surmised that decisional control is "unlikely to be associated with an enhanced perception of control. . . . On the contrary, the elimination of such control would be expected to enhance the perception of control" (p. 353). This conclusion--that eliminating one type of objective control may enhance the subjective perception of control--may be due to the particular nature of their experimental procedure; subjects who were given decisional control tended to make quicker decisions (perhaps thinking that they had less time for decisions because making the decision ended the trial), and quicker decisions--for subjects in the decisional control condition--were associated with less perceived control.

Efficacy and Control in Groups

A recent (Feltz, 1992) review of self-efficacy studies in sports noted that self-efficacy "is one of the most frequently cited psychological factors thought to affect sport performance" (p. 93). Virtually all of the self-efficacy studies, however, examined individual performance. Feltz found only one study (Feltz, Bandura, & Lirgg, 1989, reported in Feltz, 1992) that investigated the role of team efficacy; this despite the fact that "in most sports . . . individuals perform as members of teams rather than as independent competitors" (Feltz, 1992, p. 100). In this sole study of team efficacy, hockey players completed self-efficacy and team efficacy measures, which were then compared to actual performance in games. Although neither efficacy measure was a predictive of actual performance as was individual perception of team rank, team efficacy was a marginally better predictor of team performance than was self-efficacy.

Most studies which claim to investigate judgments of efficacy and control in group situations are focused on group decision-making situations (e.g., Sniezek & Henry, 1989) where the judgment of the group, rather than that of the individual, is of interest. Kerr (1989) began to address the problem from the standpoint of the individual, exploring the effects of membership in a large group on perceived personal and collective efficacy. Kerr's subjects were asked to imagine themselves part of a group of 9, 54, or 324 people. As part of the group, they were to participate in a social dilemma problem in which, if a certain proportion-- either 33 or 67 percent--of the group "invested" ten dollars, all members of the group would receive a payoff of 20 dollars (all subjects participated in all six group size and proportion problems, with the order of presentation randomized between subjects). Kerr asked his subjects to report the degree of efficacy they felt they had in the task, that is, "how much a decision to invest by [the subject] will increase the group's

probability of earning the investment payoff" (1989, p. 290). As group size increased, subjects correctly recognized that their contribution shrank proportionately. In two follow-up experiments, subjects also believed that their contribution shrank as group size grew, even when this was not the case (due to manipulation of the probability of other group members participating). A fourth experiment found similar results (though complicated by interactions--see Kerr, 1989) for collective efficacy as the first three experiments did for personal efficacy: As group size increased, collective efficacy decreased, even when the experiment ensured that objective collective efficacy remained constant. Kerr's methodology, however, suggests that he was measuring not perceived self-efficacy, which is described as "concerned with judgments of how well one can execute courses of action required to deal with prospective situations" (Bandura, 1982). Kerr is, at best, exploring one determinant of an individual's perceived self-efficacy in a group situation, that determinant being the individual's recognition "that their contributions matter less as their group gets larger, at least in [Kerr's procedure]" (Kerr, 1989). The possession of this skill, while important, is substantially different from the more general construct of self-efficacy (Bandura, 1984).

Applying the methods used in studying individual judgments of control to a group task imparts some other problems unique to the group situation. Although the previously mentioned effects of individual differences and situational factors still exist, they are compounded by a new set of factors. Perceived collective efficacy, group cohesiveness, beliefs about group processes, and group size, all may influence the perceived personal efficacy of the individual subject in the group (Kerr, 1989). Past experience with successful group action and the influence of familial socialization are

both among the few variables which so far have been linked to higher degrees of collective efficacy (Bandura, 1982). The preponderance of the research in collective efficacy thus far has been the work of sociologists and political scientists; the perspective of the individual in the group has been neglected. The addition of a small-group experimental approach to the theoretical and survey-type research might prove beneficial (cf. Kerr, 1983).

The need for a study of individual efficacy and perceived control in group situations seems clear; lessons learned from such study might apply to voter turnout in elections (assuming that many people do not vote because they believe they have little or no control over the outcome of the election), participation in group projects (if individual contributions are not readily identifiable, members may feel they have no effect on group activities), and a myriad of other situations. A recent decision-theory analysis of peace protest activities by Fuld and Nevin (1988) asserted that people do not work for peace because they believe their work would not matter on a national or global scale; they believe they have little control over events at that level. Also looking at collective efficacy and nuclear war, Oliver (1990) found that "citizen and national efficacy", when found, are not necessarily accompanied by feelings of self-efficacy. Apparently, even when one believes that one's country can make a difference, one's own contribution may be negligible.

Group Presence Effects: Social Loafing and Social Facilitation

Two topics which appear related to control judgment in group situations are social loafing and its complement, social facilitation. Social loafing (Latane, Williams, & Harkins, 1979) is the tendency for individuals to exert less effort at a task when in a group than when alone; conversely, social facilitation is the enhancement of a behavior due to the presence of other people (Harkins, 1987). In both cases, the effect of the presence of others is

indirect; there is no explicit attempt by the others present to influence the individual's behavior. Both social loafing and social facilitation affect individual performance in group situations, though in different ways, and both merit further consideration as having some connection with judged control in groups; neither has been considered in this light to date.

Social Loafing

Research on social loafing traces its roots to 1913, when Ringelmann measured pulling power of workers pulling alone or in groups of two, three, or eight and found that individual effort decreased as group size grew (Dashiell, 1935; Harkins, 1987). At that time, social loafing, social facilitation, audience effects, conformity effects, group interaction effects, and others all were considered part of the same area of social psychology, that of the experimental study of social situations. Social loafing and social facilitation were combined in a sub-area examining "the effects of co-workers upon the individual's work"(Dashiell, 1935, pp. 1106-1115). By 1966, however, the "Ringelmann effect" had migrated to the "group performance" chapter of Zajonc's (1966) social psychology textbook, while social facilitation effects were considered in chapters on "behavior in the presence of others" and "coaction." Ringelmann's findings were attributed to coordination losses until a replication (Ingham, Levinger, Graves, and Peckham, 1974, reported in Harkins, 1987) showed that coordination losses did not account for all the reduction in individual output. Their experiment isolated the output from each individual by measuring performance alone and in pseudogroups, wherein the subjects pulled alone but believed they pulled with others. Despite the elimination of any coordination losses, subjects still exerted less effort when they believed they were working with others than when they believed they were working alone.

Latane, Williams, and Harkins (1979) separated the Ringelmann effect into a combination of coordination losses which may effect group output on additive tasks and "social loafing," the "decrease in individual effort due to the social presence of other persons" (p. 823). In an experiment designed to replicate the Ringelmann effect with a different task, Latane et al. asked subjects to clap and shout as loud as they possibly could, alone and in groups of two, four, or six. Although total sound output increased with increasing group size, the amount of noise generated per person dropped as group size increased, indicating that the Ringelmann effect generalized across experiment tasks (1979). A second experiment (Experiment 2, Latane et al., 1979) employed "pseudogroups" in which subjects were blindfolded, wore headphones to mask the sound of the other group members, and shouted in a soundproofed room. Each subject's noise output was measured while he was shouting alone and in groups of two and six, and while he was shouting alone but was led to believe that he was in groups of two and of six. During all trials, the headphones played a recording of six people shouting, so that subjects could not tell when they were not in actual groups. This experiment allowed Latane et al. to analyze separately the effects of coordination losses (for instance, complementary sound waves from more than one person shouting would eliminate each other, reducing total sound output) and of reduced effort ("social loafing"). Consistent with their first experiment, total sound increased and sound per person decreased with increasing group size. Coordination losses were responsible for about half of the decrease in noise output, with social loafing making up the remaining half.

The same researchers (Williams, Harkins, & Latane, 1981) examined possible determinants of social loafing. Using variations of the same shouting experiment, Williams et al. manipulated the perceived

identifiability of each subject. The same subjects shouted alone and in groups and pseudogroups of two and six, first in conditions of relative anonymity, then with individual microphones and instructions which emphasized their individual identifiability. When shouting in pseudogroups social loafing was seen for trials in which subjects were not individually identifiable, but the diminution of effort was not statistically significant in trials where subjects believed they were individually identifiable.

The rope-pulling, shouting, and clapping tasks above all measure physical effort or physical output. Social loafing has also been identified in cognitive tasks. Weldon and Gargano (1988) asked subjects to rate potential part-time jobs on a number of dimensions, including supervisor and coworker relations, importance of job, number of different tasks involved and desirability of work schedule. Subjects were led to believe that they were either the only person rating a particular job or that they were one of sixteen individuals rating the job (the two levels of the "shared responsibility" manipulation). In addition, half the subjects in each condition were asked for identification (the "accountability" manipulation), ostensibly because the investigators may wish to examine what factors went into various decisions and would ask subjects to explain their ratings. Cognitive effort involved in the rating task, defined by the level of complexity of judgment strategies, was assessed through regression models of decision-making using the job evaluations (Weldon & Gargano, 1988). Consistent with physical effort (Williams et al., 1981), cognitive effort was diminished for unaccountable judges who believed they shared responsibility with 15 other raters. Accountability, however, eliminated the differences in effort between judges who believed they had full responsibility and those who believed they shared

responsibility with others. Accountability in cognitive tasks seems to play the same role as individual identifiability in physical tasks.

Explanations for social loafing effects take three different forms (Geen, 1991). Individuals who are working collectively on a boring task may respond by loafing unless motivated by personal involvement (Brickner, Harkins, & Ostrom, 1986) or subject to evaluation by the others present (Harkins, 1987). If the presence of others serves not as a source of possible evaluation but rather as a way to hide (e.g., with pooled outputs, such that no individual's efforts are directly measured) loafing is apparently the natural response to an uninteresting task. Alternatively, social loafing may be the result of social comparison, wherein individuals try to match outputs with the other individuals in the group (Geen, 1991). In this case, the assumption is made that individuals expect others in the group to loaf, and try to match their level of loafing. The third explanation of social loafing (Szymanski & Harkins, 1987) suggests that loafing will occur when no objective standard of performance is available to the individual. The presence of some form of standard (an expected output or an average output of previous individuals) will eliminate social loafing even if there is no possibility of evaluation by the experimenter or others in the group.

Social Facilitation

Research on social facilitation dates back to the earliest social psychological experiments. Triplett's (1897) observations of bicycle racers, who raced faster times in paced races (with a pace-setting multicycle) than when racing alone (against the clock), led him to examine the effect of competition on speed. His analysis of professional and amateur races, for distances ranging from one to 100 miles, found consistent gains for paced riders over unpaced riders. Several overlapping theories help explain the

enhanced performance (and when translated appropriately to other tasks, may offer explanations for facilitation effects for those tasks as well). The first two theories offered--the "Suction Theory" and "The Shelter Theory" (Triplett, 1897, p. 514)--address the possibility of ergonomic advantage gained from coactors. The suction theory proposes that a pacing bicycle creates a vacuum in its wake, pulling the racer along at a higher speed. The shelter theory (apparently the favorite of the racers themselves) merely posits a wind-shielding effect. A third theory, the "encouragement theory", emphasizes the social support given by "(t)he presence of a friend on the pacing machine" (p. 514). Three more theories (p. 515) focus on the relative burdens of mental activity on the parts of the leader (or solo rider) and the follower. "The Brain Worry Theory" emphasizes the "greater amount of brain worry" involved in leading as opposed to following in a race. The "Theory of Hypnotic Suggestion" proposes a "hypnotism" which leads to increased endurance. "The Automatic Theory," in agreement with the Brain Worry Theory above, emphasizes the automatic nature of the follower's racing task. The reduced mental effort is likened to a palpable physical force helping to propel the rider.

There remains a different category of factors, those related to the stimulating effects of the mere physical presence of another rider, through "competitive instinct," "releasing. . .nervous energy" or "suggest[ion of] a higher rate of speed" (p. 516). These factors were the subject of the Triplett's (1897) experiment.

His apparatus, a dual string-reeling device, eliminated some of the noncompetition confounds of the bicycle race (e.g., decreased wind resistance from riding behind the multicycle) and isolated the competitive factors; subjects (children and adults) were timed reeling in 16 meters of string either

alone or in competition with one other person. Of 40 subjects, 20 showed significant improvements in the competitive trials over their performance in individual trials. Ten subjects showed losses, but seemed to have been over-stimulated by the competition, exhibiting "labored breathing, flushed faces and a stiffening or contraction of the muscles of the arm" (p. 523). Ten other subjects showed little or no improvement in the competitive trials, perhaps because their solo trials were faster than the average, and there was little room for gain. Two other experiments using different methodologies isolated two separate reasons for the increased output of subjects working together. Simply viewing the movements of other workers seemed to induce similar movements in subjects (an experience familiar to viewers of athletic events, who sometimes find their muscles twitching in empathetic movement with the athletes they observe). The suggestion of a higher rate of speed was also seen as contributing to increased performance in subjects. Triplett concluded that "the bodily presence of another contestant participating simultaneously in the race serves to liberate latent energy not ordinarily available" (p. 533), such that a person in competition with another has a larger pool of energy to draw from than a person racing the clock, and that the sight of the competitor's physical movements and the suggestion of higher rate of speed "are probably in themselves dynamogenic factors of some consequence" (Triplett, 1897, p. 533).

By 1924, social facilitation effects had been found on enough different tasks, with enough variation in findings, to warrant most of a chapter in Allport's (1924) Social Psychology textbook. By this time research and theory on the effects of group presence on individual action had recognized two types of group, the "co-acting" and the "face-to-face." Social facilitation was seen as a characteristic of co-acting groups, and little research at all had been

done on face-to-face groups (Allport, 1924). Social facilitation and "rivalry" effects, which had not been seen as separate issues in Triplett's experiment, now were seen as distinct factors, both of which were involved in competition. Allport (1924) defined social facilitation as "an increase of response merely from the sight or sound of others making the same movements" (p. 262). Rivalry, on the other hand, is "an emotional reinforcement of movement accompanied by the consciousness of a desire to win" (p. 262). Allport reported that social facilitation effects had been found for writing tasks, memory tasks, multiplication tasks, vowel cancellation tests (crossing out all the vowels in a column of newsprint), reversing a Necker cube (the number of reversals in one minute), and others. Subjects included German grade-school children and American graduate students. As a general rule, the presence of others was seen to increase speed and quantity of work, but sometimes decrease quality of work (Allport, 1924).

By 1935, the contrasted increases and decreases found due to the presence of co-workers had become bothersome. Dashiell (1935) warned that "pure 'alone' and pure 'co-working'" conditions are difficult to obtain, and so was not surprised at the apparent disagreement between some findings. "Before solid findings can be achieved in any science a great amount of grub-work must needs be done in the way of clarification of problems and trying-out of techniques"(Dashiell, 1935, p. 1115).

Whether because of imperfect experimenting (as Dashiell suggested) or not, social facilitation researchers have found both increases and decreases in performance due to the presence of other people, depending on the nature of the task. Zajonc (1965, 1966) explained both findings as due to an increased drive caused by the presence of others, which leads people to act according to their dominant response in that situation. When that dominant response is

appropriate for the task at hand, increases in performance arise; when the dominant response is inappropriate, performance declines. As a result, the presence of others tends to aid in the performance of simple, well-learned tasks, and to hamper performance of complex or not-well-learned tasks. Social facilitation effects have been found for many different experimental tasks, from the 1897 Triplett reel-winding study to maze-completion, word association, rotor-pursuit, and others (see, Guerin, 1986, for a review of social facilitation effects).

Possible explanations for social facilitation effects include Zajonc's (1965) mere presence effects, as well as evaluation effects, self-presentation effects, and other effects (Guerin, 1986). Geen (1991) divided the various proposed explanations for social facilitation into two major categories: The arousal hypothesis, which subsumes mere presence, evaluation apprehension, expected failure, distraction, and self-presentation hypotheses, and a cognitive overload hypothesis, emphasizing the finite capacity of the individual to focus on aspects of the problem at hand and the taxing of that capacity by distraction. The two categories need not be mutually exclusive; arousal may be a source of distraction, and distraction, as mentioned above, is seen as one source of arousal.

Combining Social Loafing and Social Facilitation

A puzzling aspect of social loafing and social facilitation research was the different effects of loafing and facilitation being found in very similar situations; simple tasks with people working together sometimes produced facilitation effects and sometimes produced loafing effects. This seeming paradox is easily explained (Harkins, 1987); social loafing occurs when individual contributions are not identifiable, whereas facilitation occurs

when individual inputs can be evaluated.[3] Furthermore, social facilitation only increases the dominant (most probable for this particular individual) response; if this response is appropriate, individual performance will be enhanced, but if the dominant response is incorrect, performance will deteriorate. Indeed, when the dominant response is inappropriate (e.g., anxiety on a complex task), a social loafing condition can actually improve performance by reducing anxiety due to evaluation (Jackson & Williams, 1985).

Geen (1991) included not only social loafing and social facilitation, but also social anxiety (involved in impression management and self-presentation) in his review of the more general concept of social motivation. He argued that social loafing and social facilitation are the result of varying levels of evaluation apprehension (consistent with Harkins, 1987), which itself may be best understood as an aspect of self-presentation. Individuals wish to be seen in the best possible light, to avoid criticism, to obtain approval, and this is the motivation underlying social loafing and social facilitation. The extent to which they believe they have the ability to act to make the desired impression and the extent to which they believe their action will have the desired effect are both correlated with social anxiety, and are seen as instrumental in determining whether social anxiety (and, presumably, social facilitation or social loafing) will occur. Efficacy--the extent to which individuals believe they have the ability to perform a given act--is

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[3]Most of the work on social loafing and social facilitation has utilized additive tasks, wherein all subjects' efforts are added together. Disjunctive tasks (where the performance of the most able group member determines the outcome of the task for the entire group) and conjunctive tasks (where group outcome depends on the performance of the least able member) can present a different problem, that of the "free rider effect" (Kerr & Bruun, 1983). In such cases, even where individual output is readily identifiable, motivation losses occur with increased group size and task difficulty (Kerr & Bruun, 1983).

thus arguably one of the determining factors in social loafing and social facilitation (Geen, 1991). Sanna (1992) has tested this hypothesis, using a vigilance task (similar to Harkins, 1987) with false feedback in order to manipulate perceived efficacy. Subjects in one experiment (Experiment 1, Sanna, 1992) were asked to watch for dots which would flash on a computer screen and to report any sightings by pressing a key. After a 4 minute practice trial, subjects were given false feedback which placed them in the upper 80th percentile (high efficacy manipulation) or the lower 20th percentile (low efficacy manipulation). Their performance on a subsequent 9-minute trial was then measured, with subjects performing alone, in a coacting group (working at the same task but with individually identifiable output), or in a collective group (working at the same task, with pooled--and thus individually unidentifiable--output). Sanna's results supported the notion that self-efficacy is a determining factor in social loafing and social facilitation, with "high efficacy" subjects performing better and "low efficacy" subjects performing worse when subjects' outputs were individually identifiable (the facilitation condition). Easy and difficult tasks engendered high- and low-efficacy expectancies, using a different methodology (Experiment 2, Sanna, 1992) in which no false feedback was given; results again showed facilitation effects differentiated by expectancies.

Major Hypotheses

The present experiments investigate several questions. Following the social loafing literature, the performance of non-identifiable individuals in groups is examined. The social loafing literature shows that individual behavioral and cognitive performance deteriorates with increasing group size (Harkins, 1987); Experiment 1 examines the effects of increasing group size on individual judgments of control in groups in which the contributions of

individual members are not identifiable. Following the social facilitation literature, the performance of identifiable individuals in groups is examined. The social facilitation literature predicts that one's dominant response--whether helpful or harmful--will be facilitated by the presence of others (Harkins, 1987); Experiment 2 examines whether the presence of others facilitates one's prior beliefs about control and thus influences one's judgments of control in groups in which the contributions of individual members are identifiable. Individuals with the belief that they can control the outcome may have that belief enhanced; those with the belief that they have no control may have that belief enhanced.

The literature on efficacy and control suggest that judgments of control following an event may be affected by feelings of personal efficacy about the event. Classic efficacy theory (Bandura, 1977) predicts that highly efficacious individuals (those people who feel they possess the skills necessary to succeed or even excel at the task at hand) will exert more effort, and report more control, than will highly inefficacious individuals. Inefficacious individuals, believing they do not possess the skills necessary to succeed at the task, will exert less effort and report less judged control in individual as well as group situations. More recent formulations of self-efficacy theory, which include an "outcome expectancy" measure (an individual's expectation of the positive or negative reactions from those who observe their performance), suggest that inefficacious individuals will expect negative reactions from observers and will have their performance impaired by the presence of others (Sanna, 1992). The present experiments operationally define efficacy as prior belief in paranormal events in general (measured by the Jones, Russell, & Nickel, 1977 Belief in Paranormal scale), and as prior belief in personal parapsychological ability in specific (measured by the Belief in Psi Abilities Inventory).

Social Loafing Condition (group members not individually identifiable)

Are individual judgments of control subject to social loafing? That is, under conditions of relative anonymity, will judged control decrease as a function of increasing group size? Previous research has shown loafing to occur on physical tasks (Latane et al., 1979) and cognitive tasks (Weldon & Gargano, 1988); Experiment 1 examines whether loafing occurs with judged control. Kerr's (1989) experiment and Fuld and Nevin's (1988) analysis suggest that people will feel less individual control in a large group. Experiment 1 will examine the extent to which the lack of identifiability--the key factor in social loafing (Harkins, 1987) will lead to decreased judged control.

Are individual judgments of group control subject to social loafing? Under these same conditions of relative anonymity, will an individual's judgments of group control decrease as a function of increasing group size? The rationale for this question is roughly the same as for individual control above, although group performance has usually increased in social loafing studies, with only the contribution of individuals in the group working less hard (Latane et al., 1979).

Is there a relation between efficacy and social loafing? Under conditions of relative anonymity, will individuals' predictions of future performance indicate a decline in efficacy as a function of increasing group size? Again, this question simply applies previous social loafing literature to a new concept, namely judged efficacy. Because efficacy judgments are essentially predictions of future performance, this question may assess subjects' beliefs about the effect of the social loafing situation on their own future performance.

Is there a relation between perceived effort and social loafing? Under conditions of relative anonymity, will individual's reports of their own effort at the experiment task decline as a function of increasing group size? Will their reports of group effort at the task decline as a function of increasing group size? Previous social loafing literature reports a decrease in effort; the present Experiment 1 will assess whether that decrease in effort--if it occurs--is perceivable by the subjects in the experiment.

Social Facilitation Condition (group members individually identifiable)

Are individual judgments of control subject to social facilitation? That is, under conditions in which subjects' outputs are individually identifiable, will subjects' initial beliefs about their performance be facilitated as a function of increasing group size? Subjects with high previous belief in personal psychokinetic ability (high psi efficacy) would be expected to report greater control with increasing group size; subjects with low previous belief in personal psychokinetic ability (low psi efficacy) would be expected to report less control with increasing group size. As in the social loafing literature, facilitation effects have been found in numerous behavioral and cognitive tasks (Sanna, 1992); Experiment 2 simply applies the facilitation paradigm to judged control.

Are individual judgments of group control subject to social loafing? Again, under conditions in which subjects' outputs are individually identifiable, will subjects' initial beliefs about psychokinesis be facilitated as a function of increasing group size? Subjects with high previous belief in psychokinetic ability (high psi efficacy) would be expected to report greater control with increasing group size; subjects with low previous belief in psychokinetic ability (low psi efficacy) would be expected to report less control with increasing group size (cf. Sanna, 1992). Also, which measure of prior

belief--belief in personal psi abilities or a more general belief in the paranormal--will be the most predictive measure of prior belief?

Is there a relation between efficacy and social facilitation? Under conditions in which subjects are individually identifiable, will individuals' predictions of future performance reflect a facilitation of prior efficacy beliefs as a function of increasing group size? Will prior efficacy beliefs become facilitated, and thus polarized, with increasing group size?

Is there a relation between perceived effort and group size? Under conditions in which subjects are individually identifiable, will individual's reports of their own effort at the experiment task change as a function of increasing group size? Will their reports of group effort at the task change as a function of increasing group size?

Other Questions (asked in both identifiable and non-identifiable conditions)

Is control judgment related to one's judgments of how much other group members believe in psychokinesis? Bandura (1986) discussed "normative standards" to which individuals compare their performance. Geen (1991) proposed an effect of "social comparison," the matching of outputs with other group members. If group members adjust their perceptions of control and/or effort by trying to "fit in" with other group members, then their judgments of how much other group members believe in psychokinesis may be predictive of subjects' control or effort judgments.

Is control judgment related to one's judgments of how much the experimenter believes in psychokinesis? Evaluation of one's effort and output have been shown to play a role in social facilitation; without the possibility of evaluation, facilitation effects are generally not found (Harkins, 1987). It may be the case that the effects of experimenter evaluation when subjects are individually identifiable will depend to some extent on their

judgment of how much the experimenter believes in psychokinesis. If subjects feel that the experimenter does not believe in the phenomenon allegedly being tested, they may feel less pressure to perform for that experimenter.

Do subjects think that working with others has an effect on psi abilities? The psychokinesis task may be special in that, unlike most group tasks, individuals may believe that working with others enhances their efforts, in the same manner that the presence of a group may be seen as essential for a seance. Many paranormal phenomena are associated with the presence of others, in various capacities (Hines, 1988). The experiment took place with subjects seated around a table surrounding the apparatus, and may have resembled subjects' ideas of a seance. If subjects think that psi abilities, in particular, are enhanced by the presence of others, this may influence the impact of group size on judgments of control.

The Current Methodology

The methodology for my experiments is adapted from an experiment in perceived success at a psychokinesis task (Benassi, Sweeney, & Drevno, 1979), in which subjects were asked to attempt to psychokinetically influence the outcome of a die throw. A special apparatus is used which has a device (the "tumbler") at one end that mechanically tosses a six-sided die into a large plywood funnel. At the other end of the funnel the experimenter retrieved the die and recorded the outcome, then returned the die to the tumbler; at no time did the subjects handle the dice. Subjects are able to see the die being thrown, but not the result of the toss. Thus, the experimenter can, if desired, manipulate the feedback given to subjects, such that various rates of success are experienced; the experimenter can also give the subjects no feedback at all.

In the past, all actual outcomes of the die-tossing task have, of course, been purely random.

Under various instructional sets and other experimental conditions, Benassi et al. (1979) successfully manipulated subjects' reported estimates of control. Subjects who had received instructional sets which indicated that there is little or no experimental evidence for psychokinesis reported fewer confident trials, felt less likelihood of future success at the task, and perceived that they were correct on fewer trials than did subjects who had been exposed to instructional sets either claiming that good experimental evidence exists for psychokinesis or making no positive or negative claim. Subjects who were given ten practice trials reported more confident trials and greater self-influence than did subjects given only one practice trial. Subjects with high belief in the paranormal reported more confident trials and greater self-influence than those with low belief. Subjects in a two-person condition, in which one member of the dyad tossed the die in and the other observed (both tried to psychokinetically influence the outcome of the toss) felt more self-influence over the die toss if they were the active member of the pair than if they were the passive member. Active members also reported that they felt they had more influence than their partner; passive members felt both partners had equal influence (Benassi et al., 1979).

The nature of the task makes it ideal for the present question, because in it: 1) people do report feeling some degree of control over the outcome of the die toss; 2) this reported control varies with different experimental situations, and 3) groups can be run just as readily as individual subjects. Benassi et al. (1979) also used a portion of the Belief in the Paranormal Scale (Jones et al., 1977) to assess prior belief in psychokinesis; I will also use this

scale, to separate believers (in psychokinesis) and nonbelievers in order to test the social facilitation hypothesis.

Previous research using a similar paradigm has shown that men and women do not differ in their judgments of control over the outcome of the die toss (Benassi et al., 1979). There is, however, research that suggests that in some paranormal tasks men and women behave differently (Layton & Turnbull, 1975). In the present experiments subjects are all women, to avoid potential variance due to subject sex.

CHAPTER II

EXPERIMENT 1

Method

The first experiment examined the effects of group size on judgments of control over a die toss in a situation that should engender social loafing; that is, a situation in which there was no possibility for individual subjects' outcomes to be examined. Subjects, in groups of two, three, and six, were asked to try psychokinetically to influence the outcome of the roll of a single die. This task is analogous to many people working individually to accomplish a single, common task, where the individuals cannot discriminate the results of their own effort from that of others. In this experiment, it is hypothesized that prior belief will predict judged control. It is also hypothesized that as group size increases, individual judged control will decrease. A third hypothesis is that own and other effort will be judged to decrease as group size increases.

Subjects

Subjects were 360 women who participated in partial fulfillment of their introductory psychology course requirements. Subjects were all women, to avoid any potential variance due to subject sex and because of the relative unavailability of men in the subject pool.

Design

The independent variables were group size (three levels--subjects were randomly assigned to group sizes of two, three, or six individuals) and prior

belief in psi abilities (two levels--high and low--based on a median split of belief measures).

Dependent measures were obtained on two handouts. The first (appendix A) was a running record of the experiment, on which subjects indicated, on a trial-by-trial basis, whether or not they believed that the desired outcome did indeed occur. The second handout (appendix B) contained questions about the subject's perceived success in the completed task. Questions included "How much control do you feel you had over the outcome of the die toss?"; "How much control do you feel your group had over the outcome of the die toss?"; "Given another 20 trials, on how many do you think you would influence the outcome of the toss?". Each of these questions was accompanied by a Likert-type scale (1 = no influence, 7 = much influence) on which subjects reported their answers.

Apparatus

The experimental apparatus consisted of an inclined plywood chute measuring 36" X 12" X 12" (.91 m X .30 m X .30 m) with a collector box at the lower end. At the upper end, the die was tossed into the funnel by an electric die tumbler controlled by the experimenter. The apparatus was constructed such that subjects had no contact with the die, and were unable to see the outcome of any die toss.

Procedure

As subjects entered the experiment room, they were seated next to the experiment apparatus. Subjects were told that the procedure was a simple experiment in psychokinesis, in which they were to attempt to influence the outcome of a series of tosses of a two-colored die. The experimental apparatus was described to them, as was a slightly more detailed version of the cover story. After signing a consent form, but before participating in the actual

experimental procedure, subjects were given two questionnaires to complete. The first, the Jones et al. (1977) Belief in the Paranormal scale (BIP scale), measures belief in various paranormal phenomena, including the occult, extrasensory perception, and the Loch Ness monster. The second scale (BPAI scale, appendix C) contained questions designed to assess belief in one's personal psi abilities, such as extrasensory perception and psychokinesis. Results of these two scales were used to determine "believers" and "non-believers," although this distinction was not utilized in this experiment.

After completing the questionnaires, subjects were told the following:

> The experiment, as I have already told you, is an investigation of psychokinesis, the ability to mentally manipulate or influence physical objects. Your specific task in this experiment will be to influence whether this die [indicating a die colored red on three sides and green on the remaining three sides] comes up red or green. As you can see, it has three sides red and three sides green, so the probability is 50-50 that either color will come up, unless you are able to influence the die to one or the other color. For each trial--we will be doing 20 trials altogether--you will be asked to concentrate on trying to get the die to come up the specified color, either red or green. How you choose to do that is up to you. You may wish to concentrate on the color, or on a die coming up that color, or perhaps on something else that is that color--whatever you are most comfortable with. You will be given ten seconds during which you should do whatever you have chosen to do. At the end of the ten seconds, I will release

the die [the release is demonstrated] and record the outcome down here. You will not be able to see the outcome, but I will let you know how you did after the experiment is completed. Now, you will not be able to know the outcome for certain, but you may get a feeling about whether you were successful or not. That is where this sheet [indicating the running record] comes into play. For each trial, I want you to record--after that trial--whether you feel you were successful or not. There are twenty spaces there, so just check one or the other column after each trial. Any questions?

When the experimenter was assured that all subjects understood the procedure, trials began. Following the instructions outlined above, for each of twenty trials, the subject(s) indicated on a data sheet whether they felt that the outcome they were trying to achieve actually occurred. Following the twenty trials, subjects completed the dependent measure questionnaire asking how successful they were overall, how well they would do on additional trials, and how hard they had tried. Subjects were asked these questions both with regard to their personal influence and the influence of the group as a whole.

After subjects had completed all dependent measures, they were debriefed on the actual nature of the experiment. Handouts were provided outlining the need for deception, and the exact extent of the deception. It was noted that the data could indeed have been used to answer the questions asked in the cover story, but that I was not interested in the actual amount of control, but rather the amount of control subjects believed themselves to have had.

Results

Of the 360 subjects, the data from one subject per group were randomly chosen for inclusion in the analysis, to avoid violating the assumption of independence of observations necessary for an analysis of variance. After selection, 103 subjects were included in the analysis.

One of the dependent measures of judged control proved problematic. Responses to the question "Oh how many of the 20 trials you completed do you think you correctly predicted what colors came up?" indicated that subjects did not understand the question. Several subjects expressed their confusion with the question during the debriefing period, and the responses of all subjects ($\underline{M}$ = 4.83, where a score of 10 would be chance performance) indicates that they did not understand what chance performance would be for that question. Responses to the other questions showed that subjects typically believe they scored at or above chance levels; on this question they typically reported scoring at levels below that which would be predicted by chance. Because this measure was misunderstood by most subjects it was not included in the analyses.

A regression analysis examined the two measures of prior belief and their relation to the judged control and efficacy measures (Table 1). The BPAI was a significant (at $p = .001$) predictor of all control and efficacy measures, and the BIP (Jones et al., 1977) was a significant predictor (at $p = .005$) of all but one measure (the running record of confident trials, $p < .10$). The squared correlations between each of the two prior belief measures and the judged control measures are also shown in Table 1. With one exception, the two prior belief measures explain roughly the same amount of variability in the dependent measures. The exception is the running record of confident trials, for which the BPAI explained more variability. It seems reasonable, then, to

use the BPAI as the measure of personal psi efficacy, rather than the BIP. The BIP, as a measure of general paranormal belief, could be considered a measure of group psi efficacy, albeit an imperfect one. Although it does not measure a subject's judged efficacy for the group she now is in, it does give an idea of whether she believes that others in the group may possess paranormal abilities even if the subject herself does not. High and low prior belief, then, were defined by a median split of the belief scale for personal psi ability (BPAI).

The two measures of judged individual control (the running record of confident trials and the retrospective report of judged influence) were examined in separate 3 (group size) X 2 (high versus low prior belief) univariate analyses of variance. Although both variables measured judged control in the experimental task, they were examined separately (rather than in a MANOVA) because one measured judged control during the procedure, the other after the procedure was completed. Benassi et al. (1979) have shown that the effect of the situation (group size, in this case) is stronger on dependent measures taken while the subject is involved in the situation. The effect of prior belief (BPAI score, in this case) is stronger when a retrospective report is taken. Separating the two measures of judged control allows for an examination of this effect.

The running record of confident trials (Runrec) was examined in a 3 (group size) X 2 (high versus low prior belief) analysis of variance. The results of this analysis are presented in Table 2. Judged control (as measured by Runrec) was significantly related to prior belief in psi abilities, $F(1, 97) = 14.59$, $p > .001$, $\omega^2 = .12$, but not to group size, $F(2, 97) = .10$, $p = .91$, or to the interaction of prior belief and group size, $F(2, 97) = .29$, $p = .75$. Figure 1 shows the effects of prior belief and group size on the running record of

confident trials. Subjects with high prior belief in psi ability (psi efficacy) reported a greater number of confident trials in all group sizes.

The retrospective report of judged personal control (own) was examined in a 3 (group size) X 2 (high versus low prior belief) analysis of variance. The results of this analysis are presented in Table 2. Judged control (as measured by OWN) was significantly related to prior belief, $F(1, 97) = 26.30$, $p > .001$, $\omega^2 = .20$, but not to group size, $F(2, 97) = .21$, $p = .81$, or to the interaction of prior belief and group size, $F(2, 97) = .27$, $p = .76$. Figure 2 shows the effects of prior belief and group size on the retrospective report to personal control. Subjects with high prior belief in psi ability (psi efficacy) reported a greater number of confident trials in all group sizes.

Judged group control was measured using the question "How much do you feel that the concentration of the group influenced what color came up. That is, how much PK influence do you feel all of you together exerted?" The retrospective report of judged group control (GROUP) was examined in a 3 (group size) X 2 (high versus low prior belief) analysis of variance. The results of this analysis are presented in Table 2. Judged group control was significantly related to prior belief, $F(1, 97) = 18.13$, $p > .001$, $\omega^2 = .15$, but not to group size, $F(2, 97) = 1.09$, $p = .34$, or to the interaction of prior belief and group size, $F(2, 97) = .32$, $p = .73$. Figure 3 illustrates the effects of prior belief and group size on judged group control; high believers reported greater levels of group control at all group sizes.

Judged own control and judged group control were also examined in a within-subjects analysis of variance, to see whether subjects reported any difference between their own control and the control exercised by the entire group, and to see if this difference varied as a function of prior belief or group size. Results of the within-subjects analysis are reported in Table 3 and

illustrated in Figure 4. Within subjects, group control was judged higher than own control, but not significantly, $F(1, 97) = 2.49$, $p = .12$. There were no significant interactions between the within-subject measure and prior belief, $F(1, 97) = 1.49$, $p = .22$, between the within-subject measure and group size, $F(2, 97) = 1.13$, $p = .33$, and no three-way interaction among the within-subject measure, prior belief and group size, $F(2, 97) = .733$, $p = .48$.

Two dependent variables measured subjects' judgments of how well they would do in future trials: "Do you think you would get better on the task if you were given another 20 trials?" (GETBET, scored 1-7) and "If you were given another 20 trials, on how many do you think you would influence what colors came up?"(ANO20, scored 0-20). Although these are arguably measures of judged control, they seem more fitting as measures of judged self-efficacy, because they indicate the subjects' belief that they have the capability to influence the die toss, rather than their belief that they have already successfully done so. These two dependent variables were examined in a 3 (group size) X 2 (high versus low prior belief) multivariate analysis of variance. Results of the analysis are presented in Table 4. With the use of Wilks' criterion, the combined measures of judged efficacy were significantly related to prior belief in psi abilities, $F(2, 96) = 6.96$, $p = .002$, but not to group size, $F(4, 192) = 1.58$, $p = .18$, or to the interaction of prior belief and group size, $F(4, 192) = .78$, $p = .54$. Figure 5 shows the effects of prior belief and group size on the judged likelihood of improvement (GETBET); Figure 6 shows the effects of prior belief and group size on the predicted performance on another 20 trials (ANO20). Judged efficacy, using either measure, was higher for subjects with higher prior belief; judged efficacy, again using either measure, was (nonsignificantly) higher for subjects in groups of three than for subjects in groups of either two or six. When the two measures are looked at

individually, the group size effect for GETBET reaches significance, $F(2, 97) = 6.42$, $p = .05$.

Two questions asked subjects to report their perceptions of how hard they were trying to influence the die toss (OWNEFF) and how hard they thought the others in the group were trying to influence the die toss (OTHEFF). These were examined in univariate analyses of variance and in a within-subjects repeated measure design testing the relation between own and other effort as a function of prior belief and group size. Results of the univariate analyses of variance are presented in Table 5. For subjects' own judged effort, there was no effect of prior belief, $F(1, 97) = 1.40$, $p = .24$. There was an effect of group size which approached significance, $F(2, 97) = 2.82$, $p = .06$. Post-hoc analysis (Fisher's Protected LSD) showed a significant difference between groups sized 6 and 3 ($p = .03$) and a difference between groups sized 6 and 2 which approached significance ($p = .06$), but no difference between groups sized 2 and 3 ($p = .84$). There was no interaction between group size and prior belief, $F(2, 97) = .39$, $p = .68$. Figure 7 illustrates the effects of prior belief and group size on judged own effort. For subjects' judgments of others' effort there were no significant effects of prior belief, $F(1, 97) = .62$, $p = .43$, of group size, $F(2, 97) = 1.48$, $p = .23$, or of the interaction of prior belief and group size, $F(2, 97) = 1.54$, $p = .22$. Fisher's Protected LSD showed that the difference in judged others' effort between groups sized 6 and 3 approached significance ($p = .06$). Figure 8 illustrates the effects of prior belief and group size on judged effort of other group members. Within-subjects analysis of own and other effort (Table 6) revealed that subjects viewed themselves as trying harder at the task than the other group members did, $F(1, 97) = 7.86$, $p < .01$. This own-other effort variable did not interact with prior belief, $F(1, 97) = .20$, $p = .66$, or group size, $F(2, 97) = .61$, $p = .55$, and there was no significant

three-way interaction among the within-subject measure, prior belief and group size, $F(2, 97) = 2.55$, $p = .08$. Within subject effort effects are illustrated in Figure 9.

Discussion.

For the two measures of individual judged control, results were identical. Prior belief in psi abilities was predictive of judged control, but belief was the only significant predictor. There were no effects of group size or of the interaction of group size and prior belief. Prior belief has been shown previously (Benassi et al., 1979) to predict higher judgments of control at a very similar task using the same equipment; the present experiment replicates the previous finding. More generally, this result indicates that judged control, like success in coping with phobias (Bandura et al., 1982) or persistence at a competitive motor task (Weinberg et al., 1981), is influenced by self-efficacy. The higher the perceived self-efficacy--that is, the more subjects feel they have the capacity to perform the needed task--the higher the level of performance (or in this case, the higher the judged level of performance).

The lack of group size effect is a failure to find any social loafing at this task. Neither high believers nor low believers reported any difference in judged control as a function of group size. There may be several possible reasons for a failure to find loafing effects for judged control in this experiment. It may be that judged control, unlike the measures (both behavioral and cognitive) used in previous studies which did find loafing effects, is somehow isolated from and immune to social motivation processes. This appears unlikely, since Benassi et al. (1979) were able to find differences in judged control between active and passive members of a dyad (both tried to psychokinetically influence the die toss, but only the "active"

member physically tossed the die into the funnel) based on the social characteristics of their role on the experiment. Another interpretation of the lack of loafing effects is suggested by the work of Brickner et al. (1986); the experimental task may have been interesting enough to be intrinsically motivating or involving enough to keep the subjects from loafing. A psychokinesis task may hold a special appeal for undergraduate students; many subjects expressed interest in psychokinesis during the debriefing. The difference in control found by Benassi et al. (1979) may reflect not only the difference in physical involvement between active and passive subjects, but also the difference in motivational involvement; passive subjects, comparing themselves to the active subjects, may have been less interested in the task and thus motivated to loaf. A third, perhaps related, interpretation is that paranormal tasks in general may be seen as being enhanced by the presence of others. Seances involve a medium and several "sitters"; many "psychic phenomena" such as psychokinesis, channeling, or mind reading are seen as sensitive to the presence of others--especially skeptical others (Hines, 1988). If the specific psychokinesis task used was seen by a sizable portion of the subjects as facilitated by the presence of others, they may have exerted less effort and still felt the same amount of control (rather like power steering).

Individual judgment of group control followed the same pattern as judged individual control; prior belief influenced judged group control, but group size and the interaction of prior belief with group size did not. It is not surprising that personal belief influenced this measure, because the phrasing of the question (". . .how much PK influence do you feel all of you together exerted?") included the subject's own effort in the group's effort. Thus, individual control (and, by extension, individual efficacy) was explicitly involved in group control to some extent, as it must be for any action of a

group one holds membership in. This result implies that individual efficacy is predictive of group performance; its relation to group efficacy cannot be determined in this experiment.

The failure to find group size effects for judged group control may be explained by the same reasons as with individual judged control above, to some extent. It does seem odd, however to see the same pattern (or lack thereof) for judged own and judged group control. If individuals do not feel that their contributions are decreased as group size grows (as evidenced by the reports of judged own control), their contributions and those of other group members combined should grow as group size grows. Conversely, if judged group control remains the same with increasing group size (as evidenced by the reports of judged group control), then their own contributions should be perceived as shrinking (cf. Kerr, 1989).[4] Although there was a tendency in the present experiment for subjects to report that the group had exercised more control (at least in groups of 3 and 6) than had individuals, this tendency did not reach significance, and it did not grow with increasing group size.

The measures of predicted success in future trials (GETBET and ANO20) showed identical patterns; those with higher prior belief believed that they would perform better on future trials than did people with lower prior belief. No effects of group size or interaction of prior belief and group size were found. It is not surprising that prior belief was predictive of these measures. The belief measure (BPAI) assessed perceived psi ability, and was used as a measure of psi efficacy; predictions of future performance are

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[4]The idea that additional workers would necessarily increase the output of the group assumes an additive task, and assumes that the contribution of additional individuals will overcome any coordination losses which might arise from their addition to the group. These may not be valid assumptions in the present experiment.

another measure of efficacy. Because the procedure offered no feedback which might cause subjects to alter their efficacy beliefs (and perhaps because one feature of high efficacy is perseverance), subjects' initial beliefs remained unchanged and were predictive of their confidence on future trials. The lack of any group size effect may also be a factor of the lack of feedback in the procedure. There is no reason for subjects to feel that the presence of others would help or hurt them in future trials any more than in the trials they had just finished. That is, without any feedback to indicate otherwise, subjects may feel that whether one improves with experience or practice is independent of group size. Because the predicted success measures were phrased in terms of the individual and not the group, subjects may not have even considered the possibility that the others in the group might affect their future performance.

The analysis of own effort indicates that high and low believers report trying equally hard to influence the die toss. This is a bit surprising, given the nature of self-efficacy, and the tendency for highly efficacious individuals to exert more effort even on initial trials (Weinberg et al., 1981). It is possible that those individuals who felt more efficacious also felt that they needed to exert less effort, and that those who felt less efficacious tried to compensate with increased effort. The near-significant group size effect may indicate a perception of loafing, of decreased effort with increasing group size, even though judged control did not decline with increasing group size.

The analysis of other effort once again showed no difference in judged effort due to prior belief. This is less surprising than the same finding with own effort, because there need not be any relation between self-efficacy and the judged effort of one's companions (unless the assumption is made that your companions share your efficacy beliefs). A similar explanation may be

tendered for the lack of a group size effect for judged effort of other group members; without assuming that your companions feel as you do, there is no reason to think that their effort has declined in the same manner your own has.

There is a simpler explanation for the lack of group size effect for judged other effort, which is suggested by the within subjects analysis of effort. Judged other effort was significantly lower than judged own effort across all group sizes; there may be a "floor effect" influencing other effort. Other effort scores were not that close to the minimum scale values, but there may be a de facto minimum value judged by subjects. The between subjects effect suggests an egocentric bias on the part of subjects, such that the individual always perceives herself as trying harder than others in the group. This effect may reflect the difficulty in determining what represents extreme effort in other group members; one's own efforts are much more available (cf. Ross & Sicoly, 1982). That other group members' effort was not seen as waning with increased group size may simply reflect the subject's view that the other subjects were not trying very hard even in the smaller groups.

CHAPTER III

EXPERIMENT 2

Method

When the social facilitation literature is applied to judged control, an interesting pattern of results can be predicted. Those persons who do not believe they have much control over a situation as individuals should feel less individual control as members of a group with the same task, given that individual output is perceived as identifiable. Persons who believe they have a good deal of control as individuals in the situation will feel even more control as members of a group. These two predictions would be the result of social facilitation of the individual's dominant responses.

To examine the possible effects of social facilitation of disbelief, the second experiment employed groups, the efforts of whose members were individually identifiable, to engender social facilitation of group members' dominant responses. Several hypotheses are advanced: It is hypothesized that in this "individually identifiable" condition, the effect of group size will depend on the individual's judged psi efficacy (measured by the BPAI). For highly efficacious individuals, increased group size should facilitate dominant feelings of efficacy, leading to increased judgments of individual control. For inefficacious individuals, increased group size should facilitate dominant feelings of inefficacy, leading to decreased judgments of individual control.

Individual judgments of group control may also depend upon group size and individual judged efficacy. Judged group control is examined in this

experiment, in relation to group size and individual efficacy. Judged own and other effort are also examined; it is expected that effort will decline as group size increases.

Subjects

Subjects were 360 women who participated in partial fulfillment of their introductory psychology course requirements. After drawing one subject from each group to avoid violating the assumption of independence of data in analysis of variance, 102 subjects were included in the analysis.

Design

The independent variables in the second study were group size (three levels: two versus three versus six subjects per group) and prior belief (high belief and low belief, based on a median split of the BPAI scores). Rather than having all subjects concentrate on one die (as in experiment one), subjects in Experiment 2 tried psychokinetically to influence the same number of dice as the number of subjects in the group. This change was made because the "identifiable" condition requires one die per person. The dice were individually identifiable, with each subject concentrating on a particular die. The same judged success and judged control scales as above were administered in these experiments as well. In addition, subjects in the second study were asked to rate how hard they were trying, and how hard the others in their group were trying. These self-reports of own and other effort may prove sensitive to possible motivational effects of group size (facilitation or loafing effects), and were included as a dependent measure even though they are not measures of judged control. Although data were collected for all subjects, data from only one subject per group was included for analysis, to avoid violating the assumption of independence of observations required for an analysis of variance.

Materials

Materials for the second experiment were identical to those in the first experiment, with the following exceptions: Two, three or six dice were used, rather than one die. The dice were individually identifiable. A color card corresponded to each die, labeling the colors "1" and "2" for the sake of convenience.

Procedure

The procedure for the second experiment was identical to those of the first experiment, with one obvious exception: Different colored dice--one die for each subject--were given for this "individually identifiable" condition, and each subject was given a guide card with the two colors shown on it corresponding to their die. The explanation of the cover story was slightly modified, as follows:

> The experiment, as I have already told you, is an investigation of psychokinesis, the ability to mentally manipulate or influence physical objects. Your specific task in this experiment will be to influence whether these dice [indicating several dice colored one color on three sides and a different color on the remaining three sides, each die unique in its color combination, with no color used more than once] come up one or the other color. As you can see, they have three sides one color and three sides the other, so the probability is 50-50 that either color will come up, unless you are able to influence the die to one or the other color. For each trial--we will be doing 20 trials altogether--you will be asked to concentrate on trying to get the die to come up the specified color, either

color number one or color number two on your card. I will choose the color each time, so that I know which are right and wrong outcomes. How you choose to influence your die is up to you. You may wish to concentrate on the color, or on a die coming up that color, or perhaps on something else that is that color--whatever you are most comfortable with. You will be given ten seconds during which you should do whatever you have chosen to do. At the end of the ten seconds, I will release the die [the release is demonstrated] and record the outcome down here. You will not be able to see the outcome, but I will let you know how you did after the experiment is completed. Now, you will not be able to know the outcome for certain, but you may get a feeling about whether you were successful or not. That is where this sheet [indicating the running record] comes into play. For each trial, I want you to record--after that trial--whether you feel you were successful or not. There are twenty spaces there, so just check one or the other column after each trial. Any questions?

After the experimenter was assured that all subjects understood the procedure, the experiment began and twenty trials were run. Dependent measures were obtained on questionnaires following the trials, and subjects were partially debriefed. A full debriefing handout was distributed after all subjects had been run.

Results

The first two dependent variables, measuring individual judged control, were the same as described in Experiment 1. They were examined in separate 3 (group size) X 2 (high versus low prior belief) univariate analyses of variance.

The results of the first analysis, on the running record of confident trials (RUNREC), are presented in Table 7. Judged control (as measured by RUNREC) was significantly related to prior belief, $F(1, 96) = 7.98$, $p < .01$, $\omega^2 = .06$, but only marginally to group size, $F(2, 96) = 2.62$, $p = .08$, $\omega^2 = .01$, and not to the interaction of group size and prior belief, $F(2, 96) = .93$, $p = .40$. Figure 10 shows the effects of prior belief and group size on judged control. Subjects with higher prior belief reported more judged control than did subjects with lower prior belief; judged control was greatest in the largest size group (6 people). Fisher's protected LSD test shows that group size 6 is significantly different from group size 2, $p = .01$, and from group size 3, $p = .02$, but that group sizes 2 and 3 are not significantly different.

The results of the analysis of the retrospective report of judged personal control (OWN) are also presented in Table 7. Judged control (as measured by OWN) was significantly related to prior belief, $F(1, 96) = 16.75$, $p < .001$, but not to group size, $F(2, 96) = .87$, $p = .42$, or to the interaction of group size and prior belief, $F(2, 96) = .06$, $p = .94$. Figure 11 shows the effect of prior belief and group size on judged control. Subjects with higher prior belief reported more judged control than did subjects with lower prior belief. Although the effect of group size did not prove significant, the trend is the same as the significant group size effect for RUNREC above, with highest judged control in the largest group.

The retrospective report of judged group control (GROUP) was examined in a 3 (group size) X 2 (high versus low prior belief) analysis of variance. The results of this analysis are also presented in Table 7. Judged group control was significantly related to prior belief, $F(1, 96) = 5.35$, $p < .01$, $\omega^2 = .07$, and to group size, $F(2, 96) = 8.65$, $p < .005$, $\omega^2 = .06$, but not to the interaction of group size and prior belief, $F(2, 96) = 1.42$, $p = .25$. Figure 12 shows the effects of group size and prior belief on judged control; judged control was greatest in the largest size group (6 people). Fisher's protected LSD test shows that group size 6 is significantly different from group size 2, $p > .01$, and from group size 3, $p = .01$, but that group size 2 and 3 are not significantly different. Subjects with higher prior belief reported more judged control than did subjects with lower prior belief.

Judged own control and judged group control were also examined in a within-subjects analysis of variance, to see whether subjects reported any difference between their own control and the control exercised by the entire group, and to see if this difference varied as a function of prior belief or group size. Results of the within-subjects analysis are reported in Table 8. Within subjects, group control was not reliably different from individual control, $F(1, 96) = .495$, $p = .48$. There were, however several significant interaction effects. Relative control (own versus other) interacted significantly with prior belief, $F(1, 96) = 4.29$, $p = .04$, such that high believers judged themselves as having more control than the group, while low believers reported less control than the group. Relative control also interacted with group size, $F(2, 96) = 3.37$, $p = .04$, such that subjects in groups of 2 or 3 felt they had more control than the group did, while subjects in the groups of 6 felt less control than the group. The three-way interaction (within subjects by prior belief by group size) also reached significance, $F(2, 96) = 3.58$, $p = .03$, due to the unexpectedly high

reports of other control for subjects with high prior belief in the groups of 6 people. Figure 13 shows the within-subjects effects of prior belief and group size; the three-way interaction may explain the within subjects group size effect, but the within-subjects prior belief effect appears to be genuine.

Two dependent variables measured subjects' judgments of how well they would do in future trials: "Do you think you would get better on the task if you were given another 20 trials?" (GETBET, scored 1-7) and "If you were given another 20 trials, on how many do you think you would influence what colors came up?"(ANO20, scored 0-20). These two dependent variables were examined in a 3 (group size) X 2 (high versus low prior belief) multivariate analysis of variance. Results of the analysis are presented in table 9. With the use of Wilks' criterion, the combined measures of judged efficacy were significantly related to prior belief in psi abilities, $F(2, 93) = 10.91$, $p<.001$, not significantly related to group size, $F(4, 186) = 1.55$, $p = .19$, and marginally related to the interaction of prior belief and group size, $F(4, 186) = 2.22$, $p=.07$. Further examining the interaction at the univariate level, GETBET was marginally related to the interaction of prior belief and group size, $F(2, 94) = 2.71$, $p = .07$, and ANO20 was significantly related to the interaction of prior belief and group size, $F(2, 94) = 4.05$, $p = .02$. Figure 14 shows the effect of prior belief and group size on GETBET; Figure 15 shows the effects of prior belief and group size on ANO20. For both measures, judged efficacy was higher for subjects with higher prior belief. The interaction effect was the same for both measures. Efficacy scores for the low-belief subjects increased as group size increased; scores for the high-belief subjects were highest in the 2-person groups, then dropped to parallel the low-belief subjects for 3- and 6-person groups.

Two questions asked subjects to report their perceptions of how hard they were trying to influence the die toss (OWNEFF) and how hard they thought the others in the group were trying to influence the die toss (OTHEFF). These were examined in univariate analyses of variance and in a within-subjects repeated measure design testing the relation between own and other effort as a function of prior belief and group size. Results of the univariate analyses of variance are presented in Table 10. OWNEFF was not significantly related to prior belief, $F(1, 96) = 2.15$, $p = .15$, to group size, $F(2, 96) = .92$, $p = .40$, or to the interaction of prior belief and group size, $F(2, 96) = .96$, $p = .39$. OTHEFF was not related to prior belief, $F(1,96) = .07$, $p = .79$, marginally related to group size, $F(2, 96) = 2.58$, $p = .08$, and not related to the interaction of prior belief and group size, $F(2, 94) = 1.63$, $p = .20$. Within-subjects analysis of own and other effort (Table 11) revealed that subjects viewed themselves as trying harder at the task than the other group members did, $F(1, 96) = 4.12$, $p < .05$. This within-subject effect did not interact with prior belief, $F(1, 96) = 2.77$, $p = .10$, or group size, $F(2, 96) = .42$, $p = .66$, and there was no significant three-way interaction among the within-subject measure, prior belief and group size, $F(2, 96) = .04$, $p = .96$. Relations of belief and group size to effort measures is illustrated in Figures 16 (Own Effort), 17 (Other effort) and 18 (Within Subjects).

Discussion

As was the case in Experiment 1, prior belief in psi abilities was predictive of both measures of individual judged control. This finding further reinforces the connection between self-efficacy and performance, and between self-efficacy and judged control. Once again, the higher the perceived self-efficacy--the more one feels she has the capacity to perform the task--the higher the judged level of performance.

The effect of group size, which approached significance, indicates a general social facilitation effect. Subjects in this experiment felt more individual control as group size increased; this effect is somewhat surprising, especially in light of Kerr's (1989) observation that subjects felt their contributions diminish as group size increased. Subjects in the present experiment did not feel any less control as group size increased, but rather felt greater individual control in the largest (6 person) groups. This increase in judged control with increased group size is, by definition, social facilitation. Although social facilitation effects have been seen in several different tasks, increases in judged individual control with increasing group size have not been found, perhaps due to the perception that one individual makes a proportionally smaller contribution with increasing group size (Kerr, 1989) or simply because discrimination of events in the environment which lead to the perception of control is more difficult in a large group situation (Fuld & Nevin, 1988).

Two possible explanations for the presence of a facilitation effect in this experiment are found in the sizes of the groups and the nature of the task. Group sizes in the present experiment (groups of 2, 3 and 6) are considerably smaller than the pseudogroups of 9, 54, and 324 used by Kerr (1989) or the nationwide (or global) analysis of Fuld and Nevin (1988). It is certainly possible that the effect of increased control with increased group size found in the present experiment would eventually, with still larger groups, be reversed and would reflect the decreases in control found previously. Alternately (or additionally), the nature of the task may be enough different from Kerr's that different results are not unexpected. As discussed in Experiment 1, there is some reason to believe that performance on paranormal tasks is viewed as especially sensitive to the presence of others.

Although the group size main effect indicates social facilitation of judged control, the main effect was not the predicted manifestation of social facilitation for this experiment. It was expected that prior belief in psi abilities, or psi efficacy, would be the dominant response that was facilitated, and that the polarization of initial attitudes--believers feeling more efficacious, nonbelievers feeling less efficacious--would be seen in an interaction between prior belief and group size. No such interaction was found; subject with low prior belief did not feel any less control in the larger group than the smaller groups. Apparently, although judged control is enhanced somewhat by increased group size, this effect does not occur through the enhancement of prior belief. Once again, the possibility also exists that a lack of interaction effect is due to the arbitrary nature of the high belief--low belief split, which may not have produced true high belief and low belief groups. If the low belief group was in reality composed of mostly moderate believers, no effect of enhancement of disbelief would be found. There is independent evidence that suggests that the low belief group might not be composed of true nonbelievers; Jones et al. (1977) report that college students do hold relatively high belief in the paranormal.

Individual judgment of group control followed the same pattern as judged individual control, but in this case both prior belief and group size were significantly related to judged group control. As in Experiment 1, the influence of prior belief on judged control is not surprising; group control was defined as including the subject's own control, and so was subject to the same influences as was individual control.

Judged group control increased with increasing group size, indicating either a social facilitation effect or a recognition of the contributions of a larger number of people. The latter possibility (that the increase in judged

control is simply due to the increase in the number of people working at the task) is less likely, since each subject was working on her own die, such that the work load increased with increasing group size. If each condition saw each subject working on one die, then the increase in judged control should not reflect a sharing of the workload. Because the increased judgment of control is unlikely to be due to the physical change (the addition of more group members) of the task, it appears likely that it is social facilitation. As was the case with individual judged control, the facilitation effect did not interact with prior belief in the manner expected. Once again, although judged control is subject to group size effects--social facilitation, in this case--facilitation does not come about through the enhancement of dominant prior efficacy beliefs.

Looking at judgments of own and group control within subjects, the most interesting effect is the three-way interaction of the within-subjects term with prior belief and group size. (Of course, the within-subject by prior belief and within-subject by group size interactions can only be properly understood after examining the three-way interaction.) Subjects with low prior belief judged themselves as having less control than the group as a whole did, for all group sizes (both group and own control were judged highest for the 6-person groups); subjects with high prior belief judged themselves as having more control than the group as a whole did for the 2 and 3-person groups, but less than the group for the 6-person groups. Subjects with lower prior belief apparently felt more confidence in the group than in themselves, or perhaps simply assumed that group control--since it was defined in the question as including one's own control--must have been greater than personal control. Subjects with high prior belief felt more control than did the group (paradoxically, the group explicitly including themselves) in groups of 2 and

3 people. Highly self-efficacious subjects in these groups apparently have enough confidence in their own abilities to overshadow the logic of the situation (unless they thought that the presence of others might somehow diminish their own contribution--not unheard of in explanations of paranormal phenomena). In the largest groups (6 individuals), high-efficacy subjects did report that the group had exercised more control than they had individually. If this is not a fluke (in which case the same explanation for the 2 and 3 person groups would hold true for the 6 person group), then apparently a group of this size is large enough that the self-serving bias is not strong enough to overpower the effect of the situation.

The measures of predicted success in future trials (GETBET and ANO20) showed identical patterns, though very different patterns from the same measures in Experiment 1. Those with higher prior belief believed that they would perform better on future trials than did people with lower prior belief. This effect interacted with group size, significantly for ANO20, approaching significance for GETBET and in the MANOVA. The main effect for belief is, once again, not surprising. These measures may be interpreted as efficacy measures (for the hypothetical 20 future trials), and so should correspond to prior belief measures which also measure efficacy (such as the BPAI). The interaction effect reflects the different effects of high and low prior belief in the 2-person groups; high and low believers paralleled each other in the 3 and 6-person groups. In the 2-person groups, scores on these measures reflect the prior belief of the subjects; in the larger groups, it may be the case that the effects of group size are more apparent. As was possibly the case in the within-subjects control effects, it seems that prior belief is more influential in the smaller group, and that the effects of group size become more important as group membership becomes more salient.

For own and other effort measures there were no effects of prior belief, and no facilitation effects of group size; in fact, the only group size effect that approached significance indicated that "other effort" was lower for groups of 6 than for groups of 3. This contrasts with the group size effects for judged own and other control; judged control increased, but judged effort decreased (though not significantly for judged own effort), with increasing group size. It seems that subjects assume (or perhaps recognize) that social loafing occurs, and report "loafing," at least in terms of effort given to the task, even as they claim that the fruits of their reduced labor increase. Within subjects, individuals reported exerting more effort than they felt the others in the group did. This effect was also found in Experiment 1, and reflects an egocentric bias on the part of subjects.

CHAPTER 4

General Discussion

In both experiments, prior belief in psi abilities was significantly related to all measures of control (RUNREC, OWN, and GROUP). These results appear to be consistent with previous research on efficacy and with the previous use of this experiment procedure (Benassi et al., 1979). If we conceive of efficacy as "the conviction that one can successfully execute the behavior required to produce the outcomes" (Bandura, 1977), then belief in one's own psi abilities must be a general, if not a specific, measure of efficacy at this experimental task. Highly efficacious individuals, in situations as diverse as phobia management, smoking cessation, and athletic performance, have been shown to experience higher performance levels. They do so, in theory, because high self-efficacy is associated with perseverance, increased effort, and interest in the task at hand (Bandura, 1984), and these variables lead to success. The present experiments have no objective measure of control with which to measure the relative success of self-efficacious individuals, but the subjective measures of success show a strong relation between self-efficacy and judged control. The strength of the relation is more evident when one considers the arbitrary nature of the high and low prior belief categories, created by a median split of the prior belief scores. If the groups were composed of only "true believers" and "true disbelievers," the difference in judged control might well be even more marked.

That prior belief was predictive of judged own control is consistent with a large number of prior studies; there is no such history with which to

compare the relation of prior belief to judged group control. In both experiments judgments of group control followed the same pattern as judgments of own control. This could be the result of experimental demands; the group control question was phrased similarly to the own control question and followed it immediately in the questionnaire. On the other hand, it could be the case that individual efficacy is a good predictor of judged group control. Individual efficacy certainly should be at least somewhat predictive of group control, given that the subject is a member of the group and thus at least partially responsible for its performance. Feltz et al. (1989, in Feltz, 1992) found individual efficacy almost as predictive of group performance as was team efficacy. The notion that self-efficacy is predictive of group performance (and predictive of the judged group control) supports Fuld and Nevin's (1988) contention that enhancing personal efficacy will increase an individual's likelihood of working at a large collective task (e.g., anti-war activism), and may be at odds with Oliver's (1990) finding of group efficacy without self-efficacy. A possible reconciliation with Oliver might be that her subjects were asked about personal efficacy and about "citizen and national efficacy"; because a nation's organization is for the most part independent of any one particular task (about which individuals would feel varying degrees of efficacy), subjects may not feel the same connection with the larger collective that they do when the collective is organized for the sole purpose of the task at hand. In the present experiments, the phrasing of the questions explicitly connected the performance of the group to the contribution of the individual.

Prior belief in psi abilities was also significantly related, in both experiments, to subjects' predictions about future performance. Again, this relation is consistent with previous efficacy research (Bandura, 1982) and with prior use of this procedure (Benassi et al., 1979). One characteristic of high

self-efficacy is perseverance in the face of obstacles. The present experiments had no outright obstacles--that is, challenges to subjects' pre-existing efficacy beliefs--but did not offer any feedback which might confirm or reinforce subjects' efficacy beliefs. Since both prior belief (as operationalized in the present experiments) and predictions of future performance may be thought of as measures of self-efficacy for the psychokinesis task, the relation between prior belief in psi abilities and predictions about future performance may be thought of as simply an example of perseverance of efficacy beliefs in the face of an ambiguous situation.

Prior belief in psi abilities was not related to own or other judged effort. This result is surprising, both in terms of previous efficacy research and theory and in comparison with the judged own and other control results. One mechanism by which self-efficacy influences performance, in theory, is through differential expenditure of effort corresponding to level of self-efficacy (Bandura, 1984). In the present experiment, even though judged control was affected by prior belief, judged effort--in theory, causally prior to performance--was not. There are several possible explanations for the failure to find any influence of prior belief on effort. One explanation hinges on the arbitrary nature of the high and low belief groups. Since the groups were created by median split of belief scores, most members of each group might be considered "average" believers, with only the extreme high and low scorers reflecting a real difference in belief. To examine further this possibility, the highest and lowest scoring (on the BPAI) thirds of the subjects in both experiments were examined in the same analyses that were run on the complete samples. With the strength of the difference in belief scores thus enhanced, one effort measure was significantly related to prior belief (Experiment 2, own effort, $F(1, 62) = 4.82$, $p = .03$). The other three effort

measures (Experiment 1, own and other effort; Experiment 2, other effort) remained unrelated to prior belief.

A second possible explanation for the lack of observed relation between prior belief and effort is that increased effort is but one of several processes by which performance is enhanced due to high self-efficacy, any of which may be situation-specific. High self-efficacy may manifest itself as increased confidence, decreased anxiety, ability to approach threatening situations, perseverance in the face of initial defeat, the willingness to set challenges for oneself, or any of a number of intermediaries between prior efficacy beliefs and performance (Bandura, 1984). In the present experiments, effort may be difficult to perceive, even in oneself, let alone other group members. Because these experiments gave no feedback to signal subjects that they were or were not trying hard enough, subjects in all conditions may have felt that they were exerting some appropriate amount, more determined by the wording of the question than by the ambiguous task at hand.

Group size was unrelated to judged own control (for either measure of judged own control) in Experiment 1, but the relation approached significance for the running record of confident trials in Experiment 2. This combination of results is for the most part consistent with recent literature in social loafing and social facilitation (e.g., Sanna, 1992). Experiment 1, in which subjects' contributions were not individually identifiable, was intended to replicate the social loafing paradigm with a new dependent measure: judged control. The typical social loafing study has found a decrease in performance with increasing group size. Experiment 1 found no decrease, nor any reliable change, as a function of increasing group size. Although this result is not the typical decline in performance seen in classic social loafing, it is not unprecedented in the social loafing literature. Sanna (1992) found no

difference, in two different tasks, between subjects working at a collective task (Experiment 1; or at a task for which no individual evaluation was available, Experiment 2) and subjects working at the same tasks alone. Still, because there was a difference between "collective" subjects and subjects in a "coacting" (but still individually identifiable and subject to evaluation) group, Sanna claimed to have found social loafing. His "loafing" appears to be defined by a comparison with facilitation conditions, rather than by a comparison with "alone" conditions.

The present experiments could not be compared with each other, because of methodological differences beyond simple identifiability versus anonymity. As part of the manipulation of identifiability, the present experiments differed in the number of dice in a given session; Experiment 1 had one die for the group, no matter what size group, and Experiment 2 had one die per person in the group. Still, it may be instructive to examine the two experiments together. When the slopes of the judged control figures are examined it is evident that subjects are behaving differently in the identifiable and unidentifiable conditions; this difference is similar, though not identical, to the difference seen due to the same manipulation in Sanna's (1992) experiment. The present experiments differ from his results in that the present experiments showed no interaction between efficacy and group size. The facilitation effect found by Sanna was a strengthening, due to the presence of others, of a dominant response; whether subjects performed better or worse in the group situation was predicted by their initial efficacy. The facilitation effect found in Experiment 2 was a main effect of group size in the individually identifiable condition; both high-efficacy and low-efficacy subjects reported more confident trials with increased group size.

The present results are not incompatible with Sanna's results if one considers the arbitrary nature of the high and low belief categories in the present experiments. Sanna's results suggest that initial efficacy will be enhanced, that high efficacy will be made higher, low efficacy lower, by the presence of a group and the possibility of evaluation. If subjects in the present experiment--even the "low belief" subjects--were of the opinion that they would be able to influence the outcome of the die toss to some extent, that is the belief that will be enhanced, rather than the artificial "low belief."

Judged group control was unrelated to group size in Experiment 1, but group control increased with increasing group size in Experiment 2. For much the same reasons as related above, these results may be seen as consistent with previous literature in social loafing and social facilitation. Loafing studies have generally found group performance to have increased with increasing group size; only individual performance declined. Experiment 1 found subjects' judgments of group control did not increase with increasing group size, even though there were more people working at the same task (the reason for the increased performance, despite individual declines, in typical loafing studies). This result, while not an actual decline in performance, does reflect a judgment of declining influence on the part of other group members which compensates for the addition of extra workers. The increase in judged group control found in Experiment 2 parallels facilitation effects in previous experiments. It may be impossible to judge whether the increase in judged group control reflects facilitation or simply a recognition of the increased output of a larger group, but the slope of the increase is decidedly different from that in Experiment 1. Again, the comparison between experiments is not statistically viable, due to the

difference in experimental methodologies, but may be instructive nonetheless.

The effect of group size on predicted future performance did not reach significance in either experiment. However, the pattern of results in the two experiments was quite different; groups of 3 were the highest scorers on these measures in the loafing condition and the lowest scorers on these measures in the facilitation condition. If the two experiments were comparable, this discrepancy in pattern might be addressable; as is, there is no way to anchor the values with one another and thus identify differences.

Although differences in effort due to group size were not significant, subjects did consistently report having exerted less effort in the largest sized group, and did consistently report judging other group members as having exerted less effort in the largest sized group. The two results that approached significance appear to make sense in terms of social loafing and social facilitation. In the individually unidentifiable (loafing) condition of Experiment 1, judged own effort tended to be less as group size increased, perhaps in recognition of decreased effort as an anonymous member of a group. In the individually identifiable (facilitation) condition of Experiment 2, judged other effort tended to be less as group size increased; own effort was kept (relatively) high due to the possibility of evaluation, but other effort may have been seen as under the influence of increasing group size.

In both experiments and for all group sizes, subjects reported exerting more effort than they felt other group members were exerting. This difference in judged effort was greater, in some instances, for subjects with higher prior belief. This egotistic bias is easily explainable in the context of the experiments; effort at a psychokinesis task might entail intense concentration rather than some more easily observable (externally) form of

effort. Any subject will have personal knowledge of her own effort, but may be unable to judge accurately the amount of effort exerted by other group members in their attempts at intense concentration. A task for which exertion of effort is more easily observable might not produce the same differences in judged effort as these experiments. If, however, a different, more observable task were employed and the discrepancy in effort remained a significant finding, a true egotistic bias would be indicated.

Suggestions for Future Research

Suggestions for further research include a change in the methodology of the "loafing" experiment (Experiment 1) to make it directly comparable to the "facilitation" experiment (Experiment 2). One change which might make comparison possible is to use one die per person in both experiments, with the difference in identifiability being made evident through instructions to subjects. If subjects are told that the performance of the group as a whole is being observed, and that their individual performance is not subject to evaluation, this should engender social loafing (cf. Sanna, 1992). It is possible that the lack of individual identifiability under these conditions will not be as salient as in the present Experiment 1, in which the experimenter had no way of evaluating individual performance even if he wanted to, due to the single die for all subjects together. Proper wording of the instructions given to subjects should eliminate any such lack of salience; at any rate, even an imperfect loafing condition with one die per person will be more valuable because it will be directly comparable to the facilitation condition.

Another desirable addition to the experiment would be a measure of group efficacy. The Jones et al. (1977) BIP scale is too general a measure to be an effective measure of group efficacy at one particular task; it may be necessary to ask directly for a prediction of group performance. Perhaps the

questions asked about future trials in the present experiments could be asked before the trials in future experiments. The direct measures of predicted own performance would be joined by measures of predicted group performance, administered before and after the 20 trials are run.

Another change allowing a more detailed examination of results would be to run subjects in multiple trials, in all three group sizes. With order in which group sizes are run randomly assigned, a within-subjects analysis of group size and practice effects will be possible. Measures of predicted own and group performance could be taken before each set of trials, and after the final set as though for a hypothetical additional set. Judged own and other control and effort measures would be taken after each set of trials, as in the present experiments.

The Big Picture

The present experiments have shown a consistent effect of prior belief, such that high believers report higher judgments of control and of predicted future performance. Some evidence for a group size effect, such that increasing group size caused an increase in judged own and group control under conditions of individual identifiability (social facilitation) was found, but no reliable differences in control due to group size were found when subjects were individually unidentifiable. Perhaps most interestingly, judged own and other effort decreased as group size increased (though this effect only approached significance), even as (in Experiment 2) judged own and group control increased.

The juxtaposition, as group size increased, of judged control measures with judged effort measures, suggests that in this situation judgments of control do not come from the perception of one's actions in the situation, but instead depend on initial belief. This initial belief may be subject to social

motivation--that is, loafing or facilitation--but not through the intermediate step of changes in effort. Effort, in the present experiments, was difficult to judge in others, perhaps even in oneself. In larger social situations (such as discussed in the introduction of this dissertation), the effectiveness of effort may likewise be difficult to ascertain, though more because of lack of feedback than because of ambiguity of what constitutes extreme effort.

A letter written to an authority may receive no reply, or may receive merely a form-letter acknowledgement. Either of these outcomes is likely to leave the letter-writer in the dark as to the effect of his or her message. Without some sort of feedback, actual exercise of control cannot be perceived (cf. Crocker, 1981), and the individual who is trying to judge how much control he or she can exercise (that is, his or her level of self-efficacy for the situation) is left without any objective evidence with which to make that judgment. Of course, the problem of assessing extreme effort still exists at this scale as well. Signing a petition, for example, may be seen as a small effort (only a few seconds of one's time) or a large effort (making a public statement about very private views).

That subjects in the present experiments relied on initial beliefs more than judged effort in making judgments about judged control supports Fuld and Nevin's (1988) decision theory analysis, and echoes their conclusion that "the critical determiner of involvement [may be] one's belief in the possibility of effective personal action" (p. 62). To this the present experiments would add that the critical determiner of whether one feels control (and judges that others also have control) over an event is whether one believes--before the event even begins--that one has the ability to control it. The effects of group size, at least across the group sizes utilized in these experiments, cannot account for the decline in judged control (or decline in action) that is noted by

Fuld and Nevin and others (e.g., Oliver, 1990). Indeed, in Experiment 2, when subjects were individually identifiable, judged control--both own and group--actually increased with increasing group size. The present experiments suggest that focusing on the efficacy beliefs of the individual, rather than on the social characteristics of the situation, is the best way to affect a change in judged control.

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Table 1

Simple Regressions of Judged Control and Efficacy Dependent Variables on Measures of Prior Belief (BIP and BPAI)

	BIP		BPAI	
Dependent Variable	Beta	Adj. r^2	Beta	Adj. r^2
Running Record				
Loafing	-.08	.07*	.12	.20**
Facilitation	-.05	.03†	.10	.12**
Judged Own Control				
Loafing	-.07	.30**	.07	.28**
Facilitation	-.06	.27**	.07	.33**
Judged Group Control				
Loafing	-.07	.28**	.06	.23**
Facilitation	-.05	.24**	.05	.22**
Likelihood of Improvement				
Loafing	-.05	.14**	.04	.11**
Facilitation	-.06	.21**	.07	.28**
Prediction, Another 20				
Loafing	-.21	.23**	.19	.23**
Facilitation	-.14	.20**	.19	.33**

*$p < .005$ **$p < .001$ †$p < .10$

Table 2

Analysis of Variance of Control Measures in the Loafing (Individually Unidentifiable) Condition

Measure	Prior Belief (B)	Group Size (S)	B X S	Within
Running Record				
MS	146.69	.98	2.95	10.06
df	1	2	2	97
F	14.59*	.10	.29	
Judged Own Control				
MS	47.94	.37	.49	1.82
df	1	2	2	97
F	26.30*	.21	.27	
Judged Group Control				
MS	33.61	2.02	.60	1.85
df	1	2	2	97
F	18.13*	1.09	.32	

*$p < .001$

Table 3

Within-Subjects Analysis of Variance of Control Measures in the Loafing (Individually Unidentifiable) Condition

Source	SS	df	MS	F
Between Ss				
Prior Belief (A)	80.92	1	80.92	24.88*
Group Size (B)	3.84	2	1.92	.59
A X B	1.55	2	.78	.24
Ss within groups (C)	315.44	97	3.25	
Within Ss				
Relative Control (D)	1.06	1	1.06	2.49
A X D	.64	1	.64	1.49
B X D	.96	2	.48	1.13
A X B X D	.62	2	.31	.73
D X C	41.19	97	.43	

*$p < .001$

Table 4

Multivariate Analysis of Variance of Efficacy Measures in the Loafing (Individually Unidentifiable) Condition

Measure	Prior Belief (B)	Group Size (S)	B X S	Within
		Univariate Tests		
Likelihood of Improvement				
MS	10.88	6.42	1.07	2.06
df	1	2	2	97
F	5.28*	3.11*	.52	
Predicted Performance on Another 20 trials				
MS	294.65	28.70	15.64	21.24
df	1	2	2	97
F	13.87***	1.35	.74	
		MANOVA		
Combined Measure				
Wilks' Lambda	.87	.94	.97	
Num df	2	4	4	
Den df	96	192	192	
F	6.96**	1.58	.783	

*$p < .05$ **$p < .005$ *** $p < .001$

Table 5

Between-Subjects Analysis of Variance of Effort Measures in the Loafing (Individually Unidentifiable) Condition

Measure	Prior Belief (B)	Group Size (S)	B X S	Within
Own Effort				
MS	1.98	4.00	.55	1.42
df	1	2	2	97
F	1.40	2.82†	.39	
Other Effort				
MS	.809	1.92	2.01	1.30
df	1	2	2	97
F	.622	1.48	1.54	

†$p < .10$

Table 6

Within-Subjects Analysis of Variance of Effort Measures in the Loafing (Individually Unidentifiable) Condition

Source	SS	df	MS	F
Between Ss				
Prior Belief (A)	2.66	1	2.66	1.29
Group Size (B)	11.04	2	5.52	2.68†
A X B	1.75	2	.87	.424
Ss within groups (C)	199.83	97	2.06	
Within Ss				
Relative Effort (D)	5.17	1	5.17	7.86*
A X D	.13	1	.13	.20
B X D	.80	2	.40	.61
A X B X D	3.36	2	1.68	2.55†
D X C	63.84	97	.69	

*$p < .01$ †$p < .10$

Table 7

Analysis of Variance of Control Measures in the Facilitation (Individually Identifiable) Condition

Measure	Prior Belief (B)	Group Size (S)	B X S	Within
Running Record				
MS	83.14	27.29	9.66	10.43
df	1	2	2	96
F	7.98*	2.62†	.93	
Judged Own Control				
MS	30.49	1.59	.11	1.82
df	1	2	2	96
F	16.75*	.87	.06	
Judged Group Control				
MS	13.53	8.37	2.21	1.57
df	1	2	2	96
F	8.65**	5.35**	1.41	

*$p < .001$ **$p < .01$ †$p < .10$

Table 8

Within-Subjects Analysis of Variance of Control Measures in the Facilitation (Individually Identifiable) Condition

Source	SS	df	MS	F
Between Ss				
Prior Belief (A)	42.32	1	42.32	14.16**
Group Size (B)	17.25	2	8.62	2.89†
A X B	1.81	2	.90	.30
Ss within groups (C)	286.95	96	2.99	
Within Ss				
Relative Control (D)	.20	1	.20	.50
A X D	1.70	1	1.70	4.29*
B X D	2.67	2	1.34	3.37*
A X B X D	2.84	2	1.42	3.58*
D X C	38.04	96	.40	

*$p < .05$ **$p < .001$ †$p < .10$

Table 9

Multivariate Analysis of Variance of Efficacy Measures in the Facilitation (Individually Identifiable) Condition

Measure	Prior Belief (B)	Group Size (S)	B X S	Within
		Univariate Tests		
Likelihood of Improvement				
MS	25.53	3.50	5.72	2.11
df	1	2	2	94
F	12.10**	1.66	2.71†	
Predicted Performance on Another 20 trials				
MS	258.19	25.75	51.99	12.83
df	1	2	2	94
F	20.13**	2.01	4.05*	
		MANOVA		
Combined Measure				
Wilks' Lambda	.81	.94	.91	
Num df	2	4	4	
Den df	93	186	186	
F	10.91**	1.54	2.22†	

*$p < .05$ **$p < .001$ †$p < .10$

Table 10

Between-Subjects Analysis of Variance of Effort Measures in the Facilitation (Individually Identifiable) Condition

Measure	Prior Belief (B)	Group Size (S)	B X S	Within
Own Effort				
MS	2.40	1.03	1.07	1.12
df	1	2	2	96
F	2.15	.92	.96	
Other Effort				
MS	.07	2.49	1.58	.97
df	1	2	2	96
F	.07	2.58†	1.63	

†$p < .10$

Table 11

Within-Subjects Analysis of Variance of Effort Measures in the Facilitation (Individually Identifiable) Condition

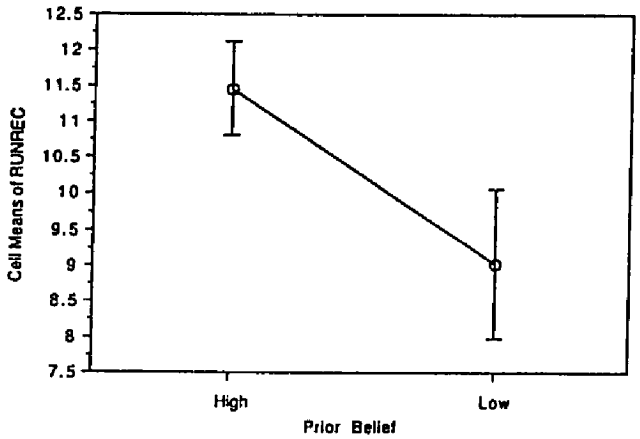
Source	SS	df	MS	F
Between Ss				
Prior Belief (A)	.83	1	.83	.56
Group Size (B)	6.54	2	3.27	2.19
A X B	5.25	2	2.63	1.76
Ss within groups (C)	143.13	96	1.49	
Within Ss				
Relative Effort (D)	2.43	1	2.43	4.12*
A X D	1.64	1	1.64	2.77†
B X D	.50	2	.25	.66
A X B X D	.05	2	.02	.96
D X C	56.74	96	.59	

*$p < .05$ †$p < .10$

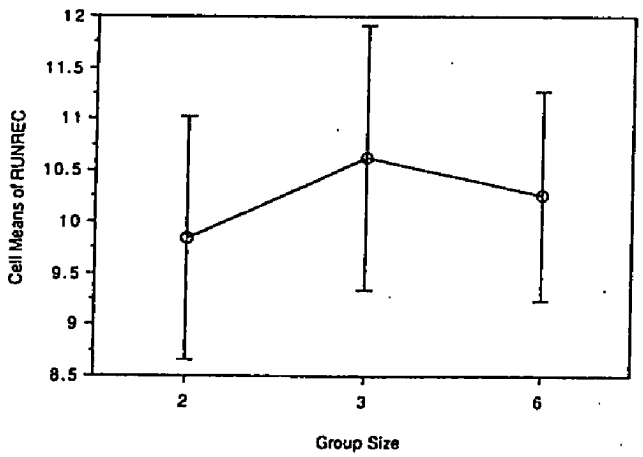
Figure Caption

Figure 1. Effects of Prior Belief and Group Size on the Running Record of Confident Trials, for Subjects in the Loafing (Individually Unidentifiable) Condition.

Running Record of Confident Trials (Runrec)
By Prior Belief in Psi Ability
With 95% Confidence error bars.



Running Record of Confident Trials (RunRec)
By Group Size
With 95% Confidence error bars.



Running Record of Confident Trials (Runrec)
By Group Size and Prior Belief in Psi Abilities
With 95% Confidence error bars.

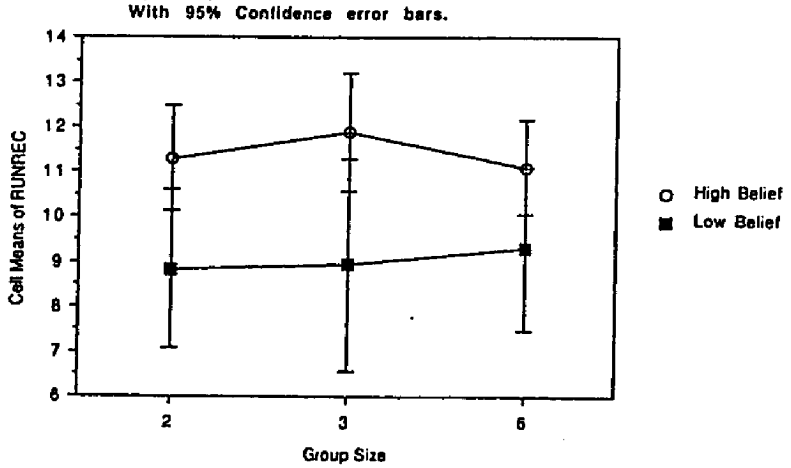
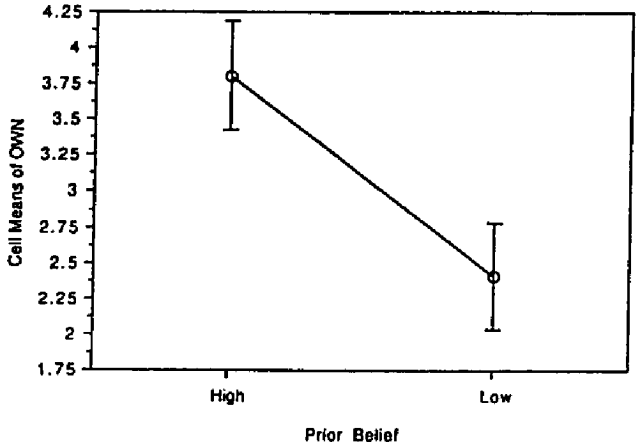


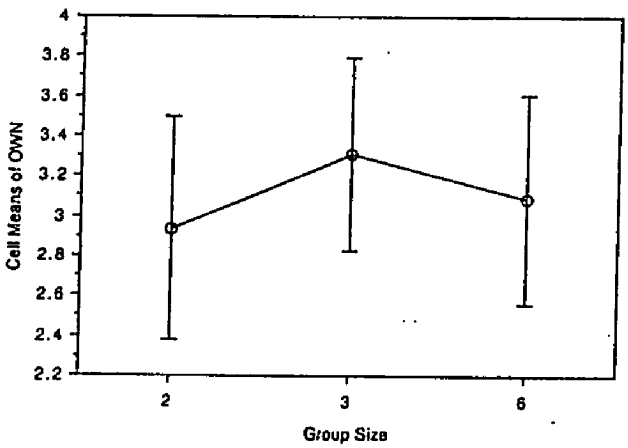
Figure Caption

Figure 2. Effects of Prior Belief and Group Size on the Retrospective Report of Judged Own Control, for Subjects in the Loafing (Individually Unidentifiable) Condition.

Retrospective Report of Own Judged Control
By High versus Low Prior Belief (Psi Efficacy)
With 95% Confidence error bars.



Retrospective Report of Judged Control
By Group Size
With 95% Confidence error bars.



Retrospective Report of Judged Control by
Group Size and High versus Low Prior Belief
With 95% Confidence error bars.

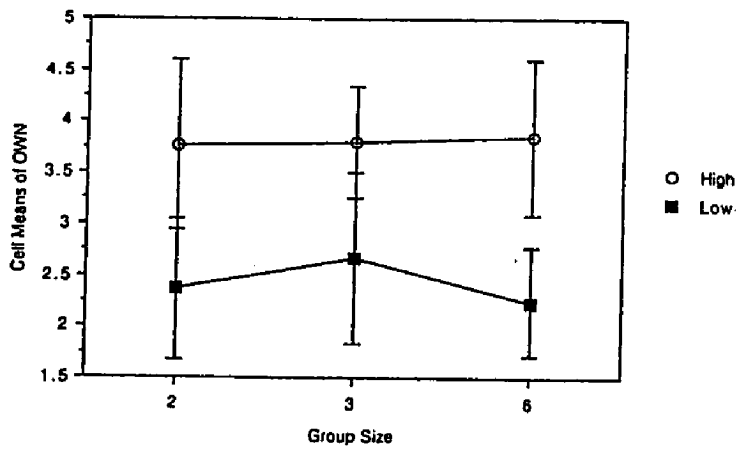
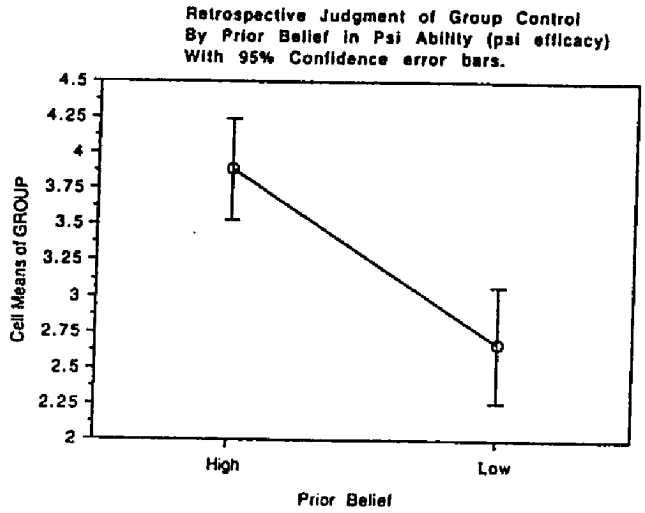
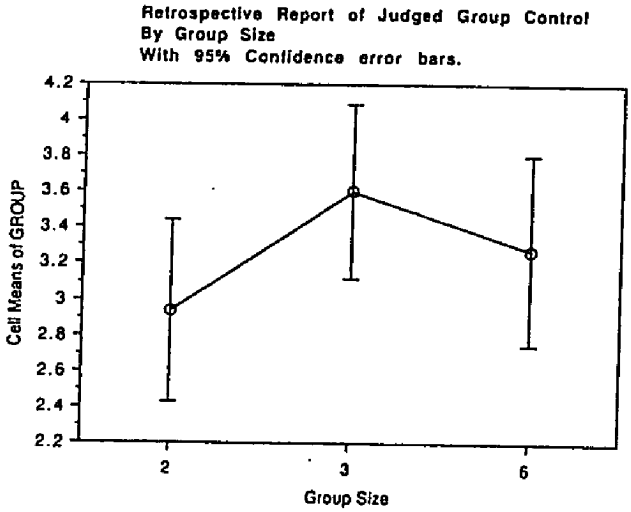


Figure Caption

Figure 3. Effects of Prior Belief and Group Size on the Retrospective Report of Judged Group Control, for Subjects in the Loafing (Individually Unidentifiable) Condition.





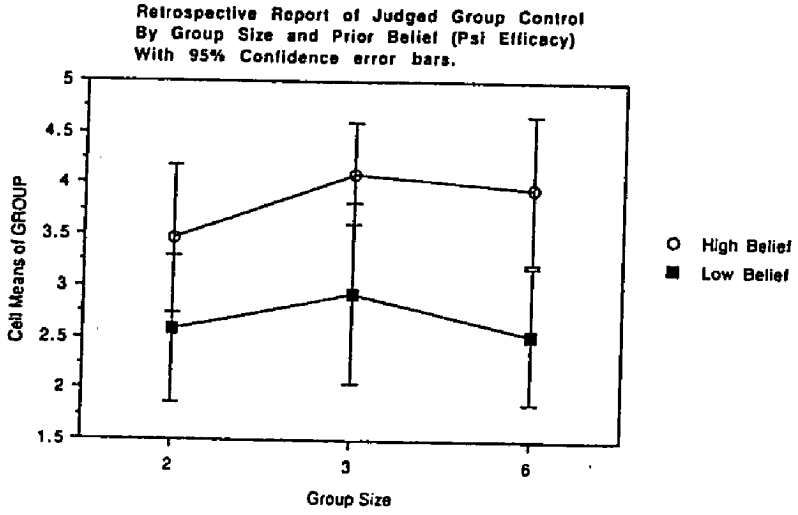
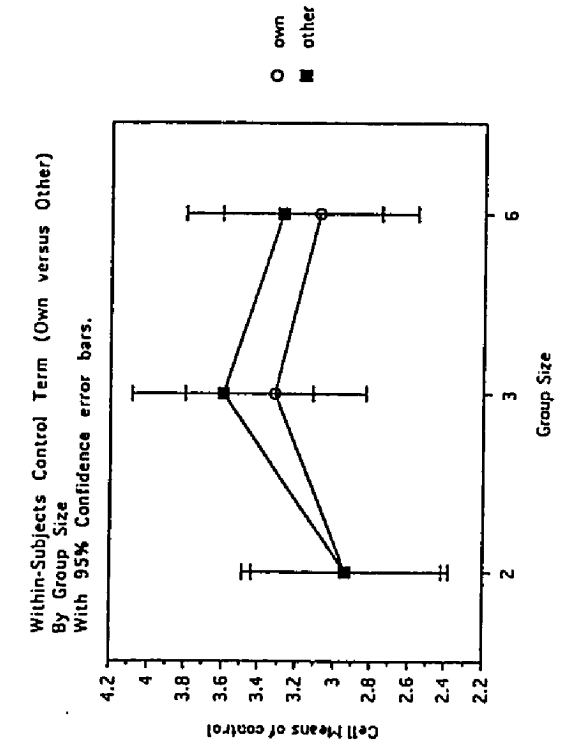
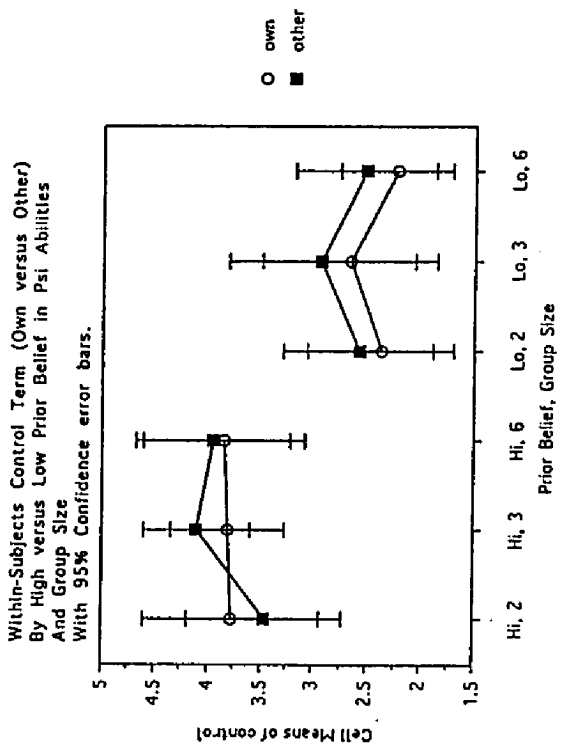
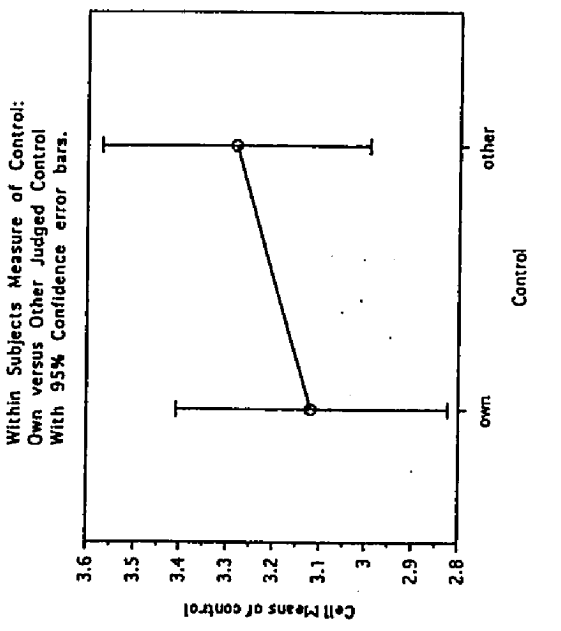


Figure Caption

Figure 4. Within Subject Effects of Own versus Group Control, Prior Belief and Group size for Subjects in the Loafing (Individually Unidentifiable) Condition.







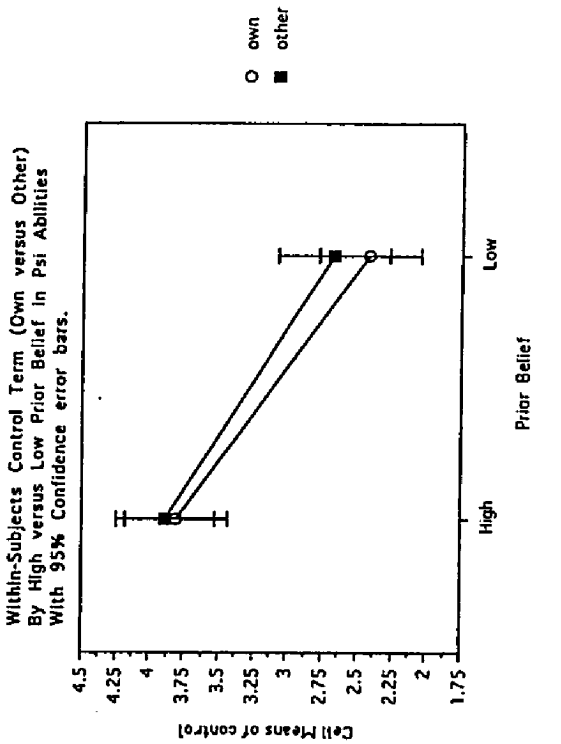
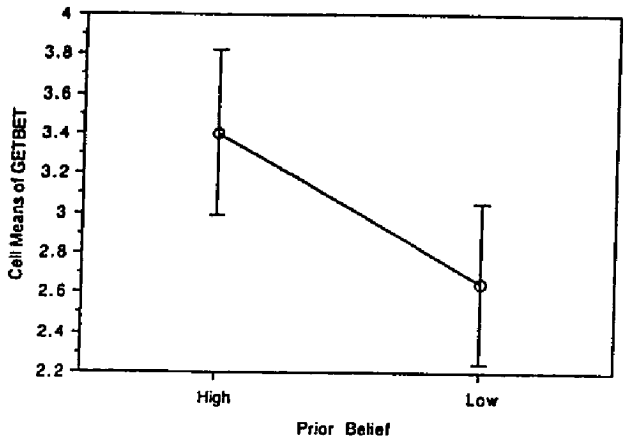


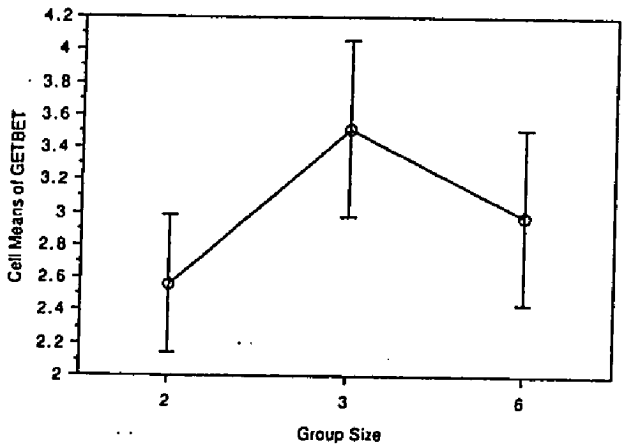
Figure Caption

Figure 5. Effects of Prior Belief and Group Size on Subjects' Judged Likelihood of Improvement on Another 20 Trials, for Subjects in the Loafing (Individually Unidentifiable) Condition.

Judged Likelihood of Improvement (GetBet)
By Prior Belief (Psi Efficacy)
With 95% Confidence error bars.



Judged Likelihood of Improvement (GetBet)
By Group Size
With 95% Confidence error bars.



Judged Likelihood of Improvement (GetBet)
By Group Size and Prior Belief (Psi Efficacy)
With 95% Confidence error bars.

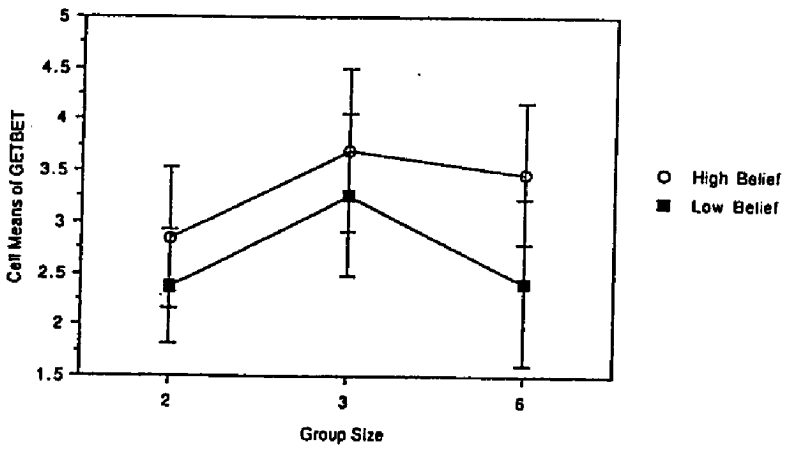
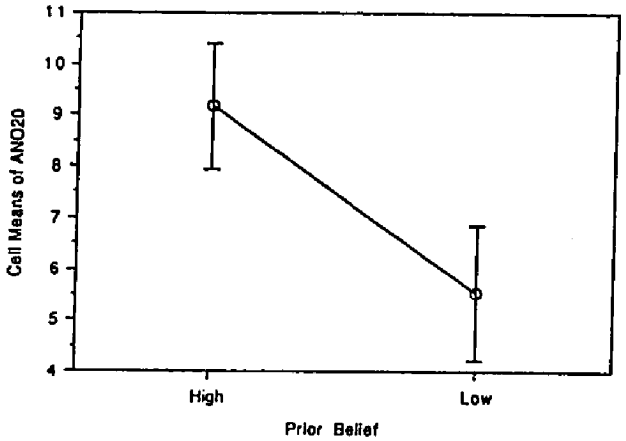


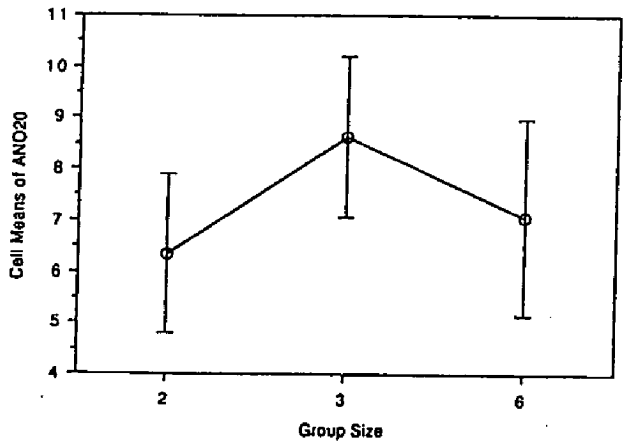
Figure Caption

Figure 6. Effects of Prior Belief and Group Size on Predicted Performance on a Hypothetical Second Set of 20 Trials, for Subjects in the Loafing (Individually Unidentifiable) Condition.

Predicted Performance on Another 20 Trials (ANO20)
By Prior Belief (Psi Efficacy)
With 95% Confidence error bars.



Predicted Performance on Another 20 Trials (Ano20)
By Group Size
With 95% Confidence error bars.



Predicted Performance on Another 20 Trials (Ano20)
By Group Size and Prior Belief (Psi Efficacy)
With 95% Confidence error bars.

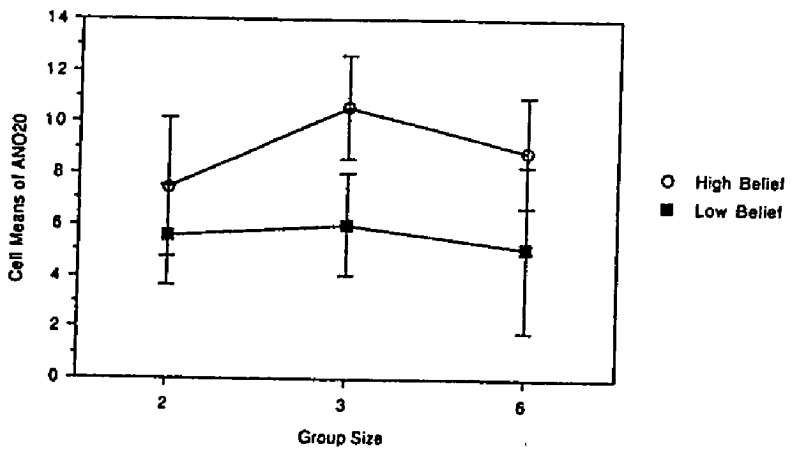
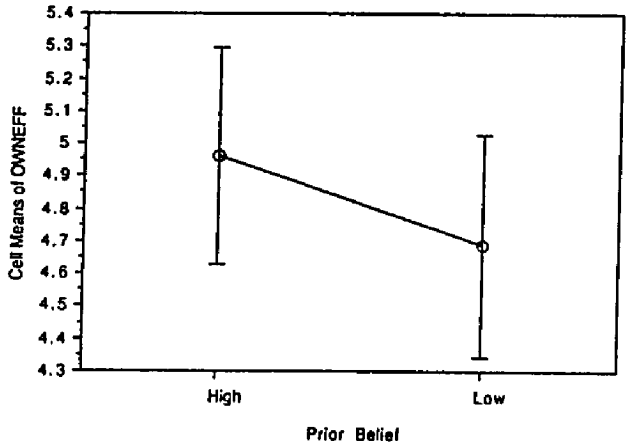


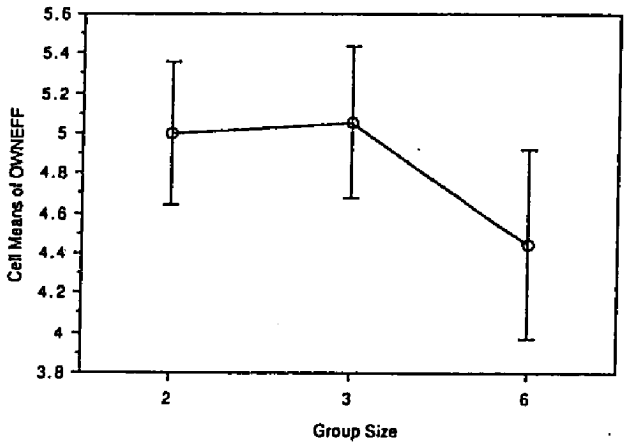
Figure Caption

Figure 7. Effects of Prior Belief and Group Size on Judged Own Effort, for Subjects in the Loafing (Individually Unidentifiable) Condition.

Judged Own Effort (Owneff)
By Prior Belief in Psi Ability (Psi Efficacy)
With 95% Confidence error bars.



Judged Own Effort (Owneff)
By Group Size
With 95% Confidence error bars.



Judged Own Effort (Owneff)
By Group Size and Prior Belief in Psi Ability
With 95% Confidence error bars.

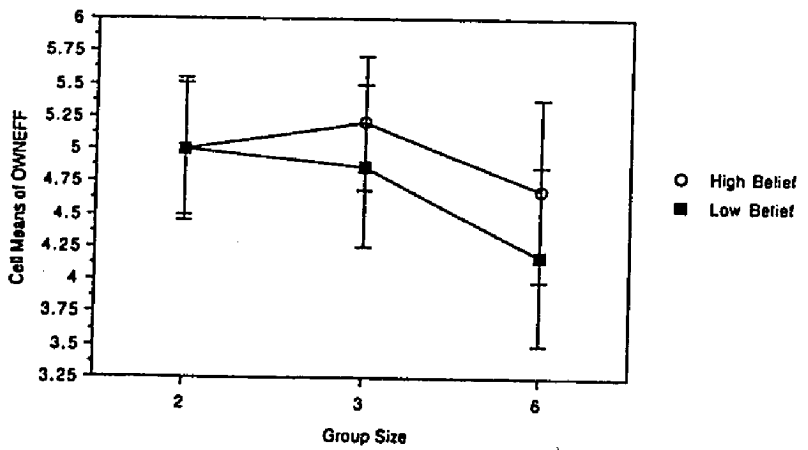
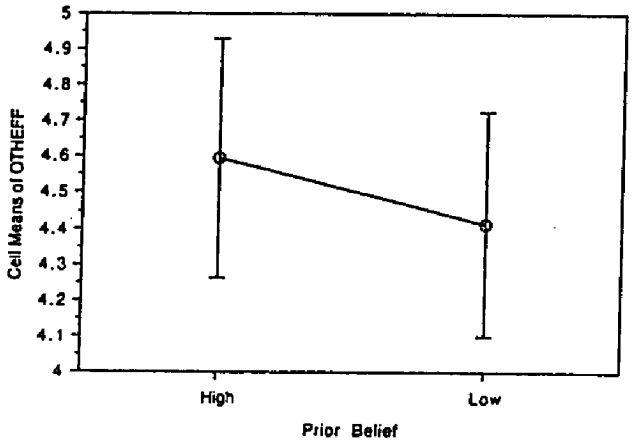


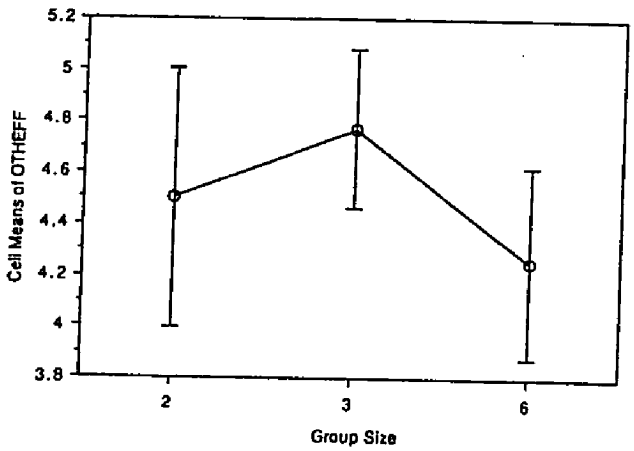
Figure Caption

Figure 8. Effects of Prior Belief and Group Size on Judged Other Effort, for Subjects in the Loafing (Individually Unidentifiable) Condition.

Judged Effort of Other Group Members (Otheff)
By Prior Belief in Psi Ability (Psi Efficacy)
With 95% Confidence error bars.



Judged Effort of Other Group Members (Otheff)
By Group Size
With 95% Confidence error bars.



Judged Effort of Other Group Members (Otheff)
By Group Size and Prior Belief in Psi Ability
With 95% Confidence error bars.

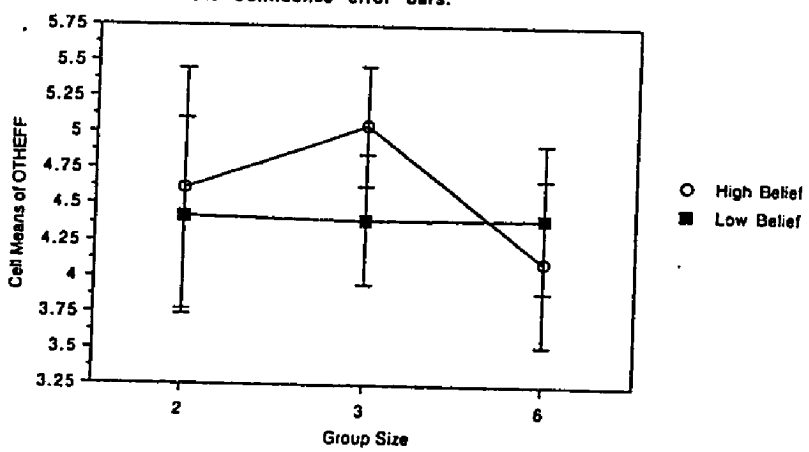
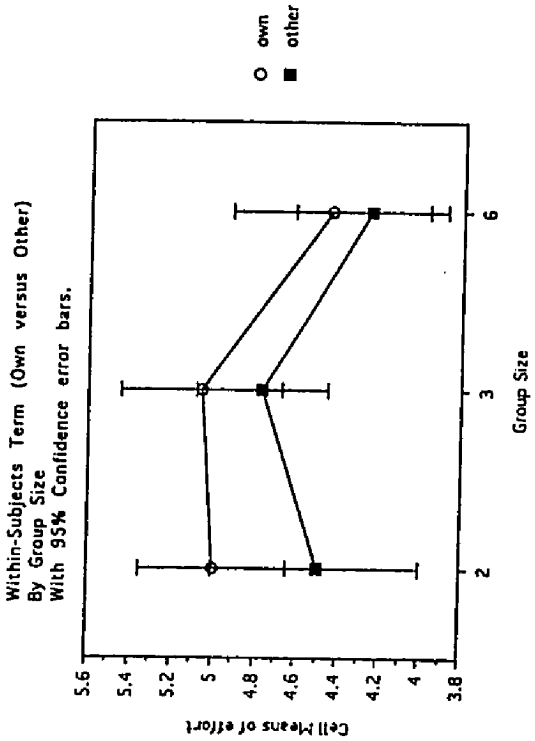
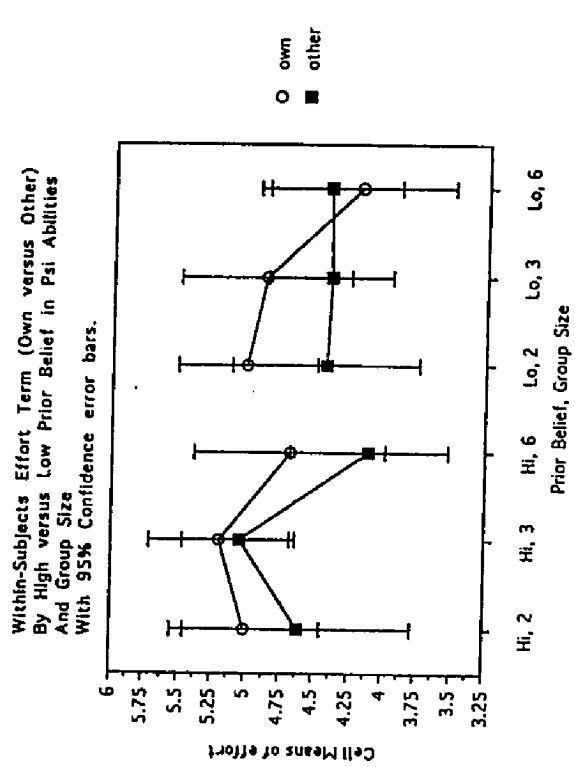
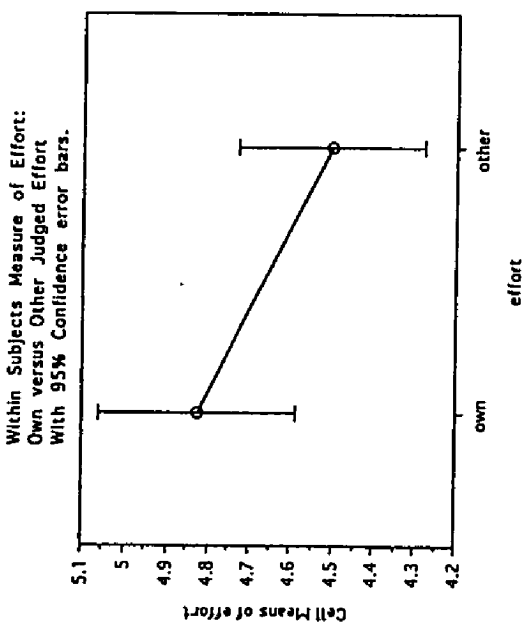


Figure Caption

Figure 9. Within Subject Effects of Own versus Group Effort, Prior Belief and Group size for Subjects in the Loafing (Individually Unidentifiable) Condition.







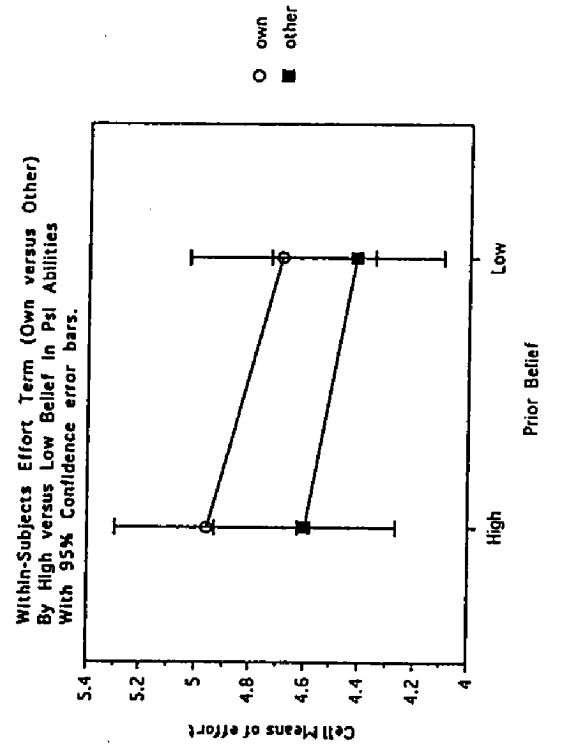
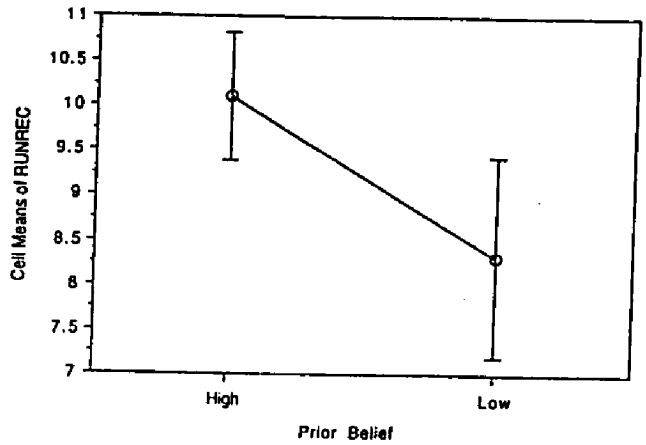


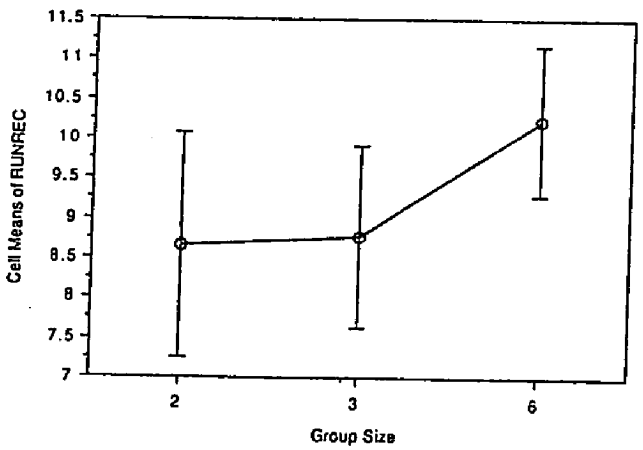
Figure Caption

Figure 10. Effects of Prior Belief and Group Size on the Running Record of Confident Trials, for Subjects in the Facilitation (Individually Identifiable) Condition.

Running Record of Confident Trials (RunRec)
By Prior Belief in Psi Ability (Psi Efficacy)
With 95% Confidence error bars.



Running Record of Confident Trials (RunRec)
By Group Size
With 95% Confidence error bars.



Running Record of Confident Trials (RunRec)
By Group Size and Prior Belief in Psi Abilities
With 95% Confidence error bars.

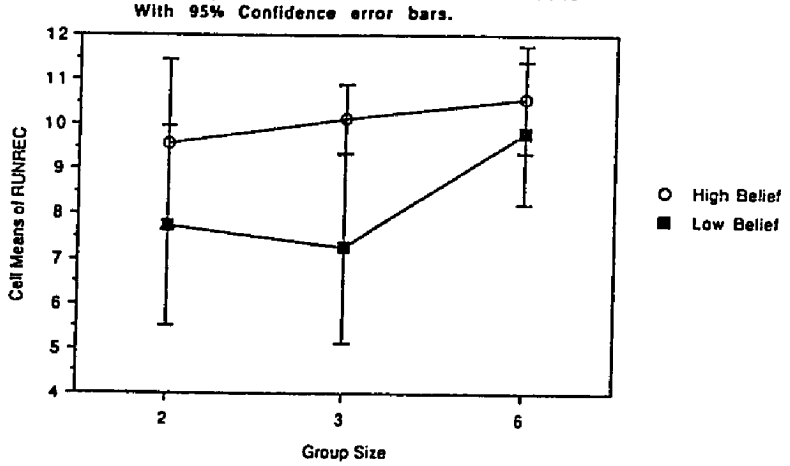
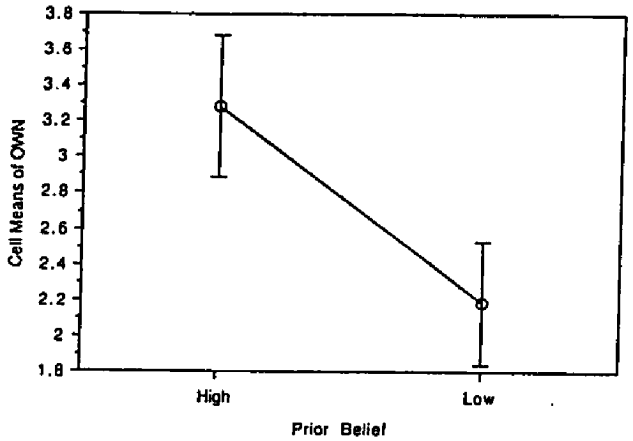


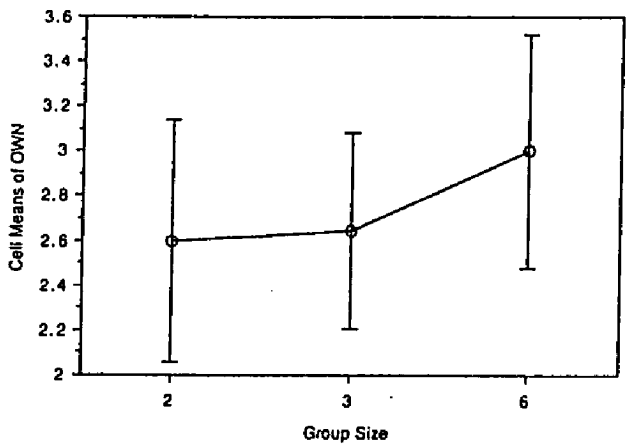
Figure Caption

Figure 11. Effects of Prior Belief and Group Size on the Retrospective Report of Judged Own Control, for Subjects in the Facilitation (Individually Identifiable) Condition.

Retrospective Report of Own Judged Control (own)
By Prior Belief in Psi Abilities (Psi Efficacy)
With 95% Confidence error bars.



Retrospective Report of Own Judged Control (Own)
By Group Size
With 95% Confidence error bars.



Retrospective Report of Own Judged Control (Own)
By Group Size and Prior Belief (Psi Efficacy)
With 95% Confidence error bars.

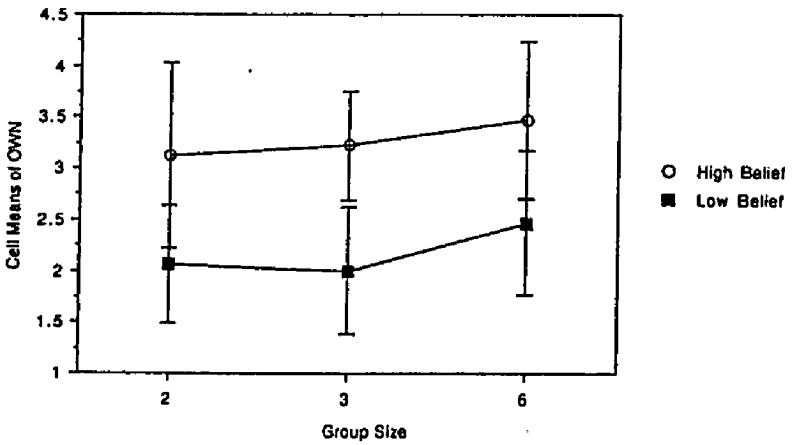
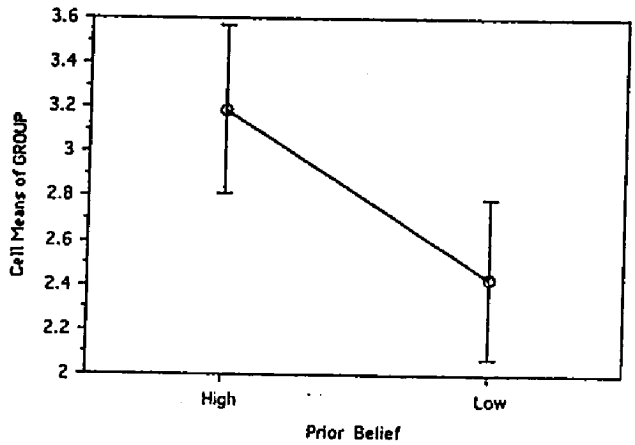


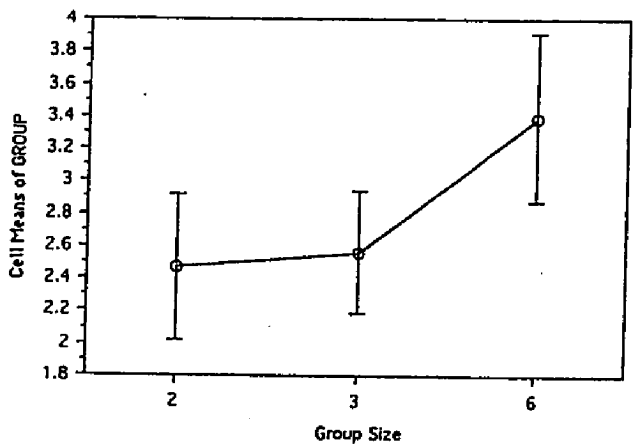
Figure Caption

Figure 12. Effects of Prior Belief and Group Size on the Retrospective Report of Judged Group Control, for Subjects in the Facilitation (Individually Identifiable) Condition.

Retrospective Judgment of Group Control
By Prior Belief in Psi Ability (Psi Efficacy)
With 95% Confidence error bars.



Retrospective Judgment of Group Control
By Group Size
With 95% Confidence error bars.



Retrospective Judgment of Group Control
By Group Size and Prior Belief in Psi Ability
With 95% Confidence error bars.

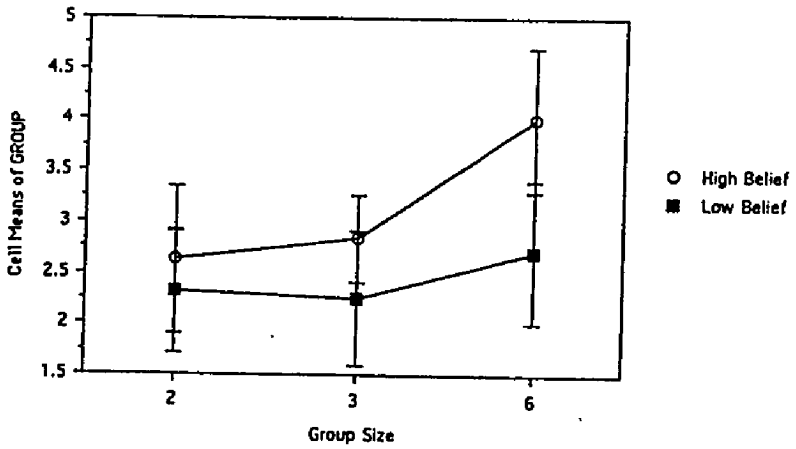
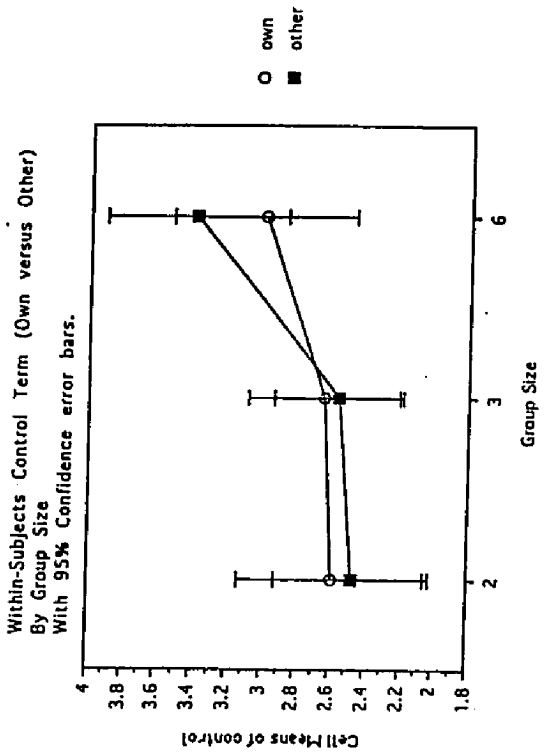
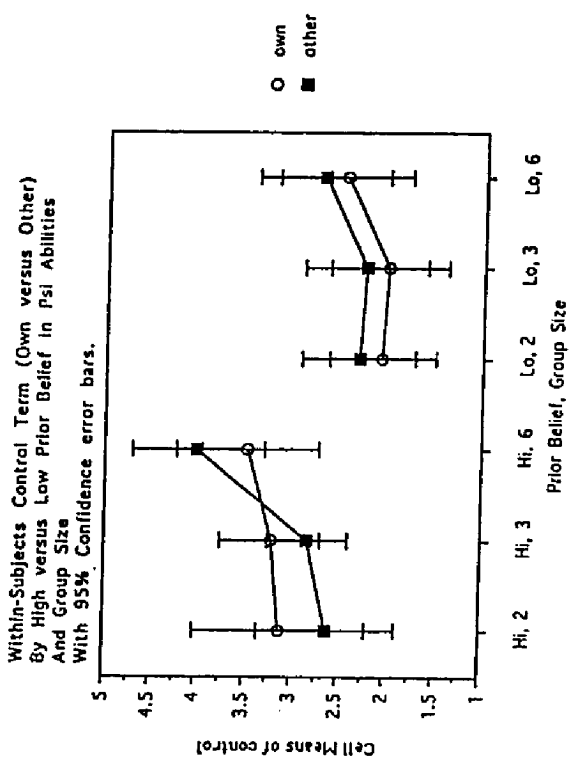
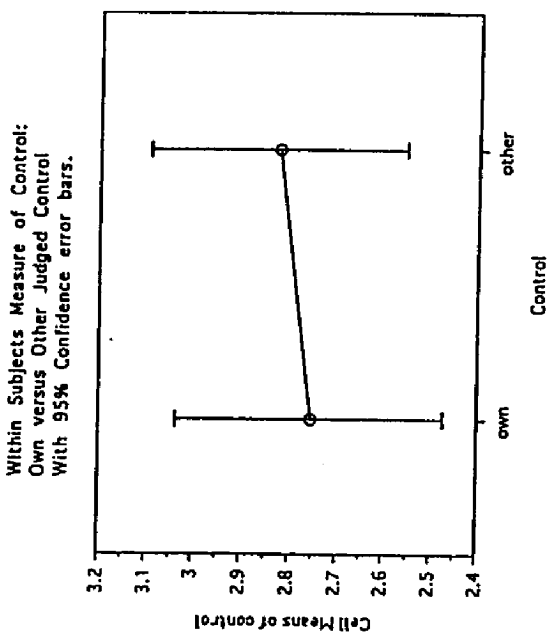


Figure Caption

Figure 13. Within Subject Effects of Own versus Group Control, Prior Belief and Group size for Subjects in the Facilitation (Individually Identifiable) Condition.







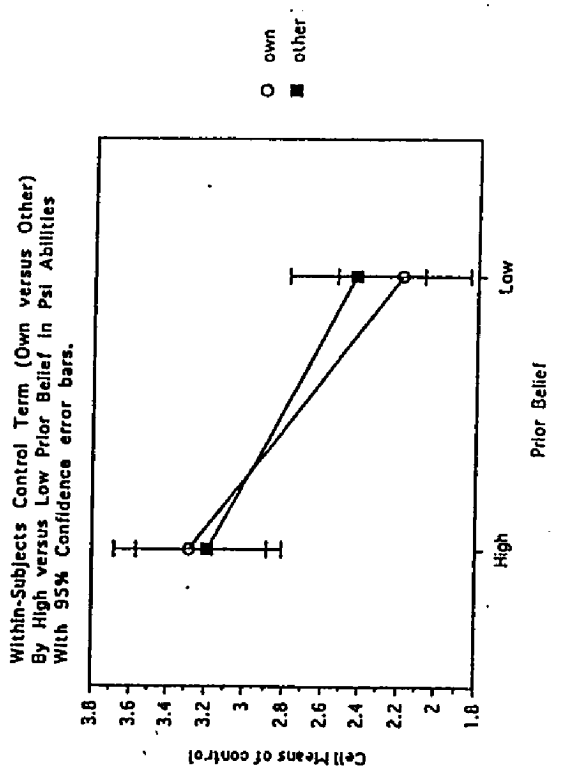
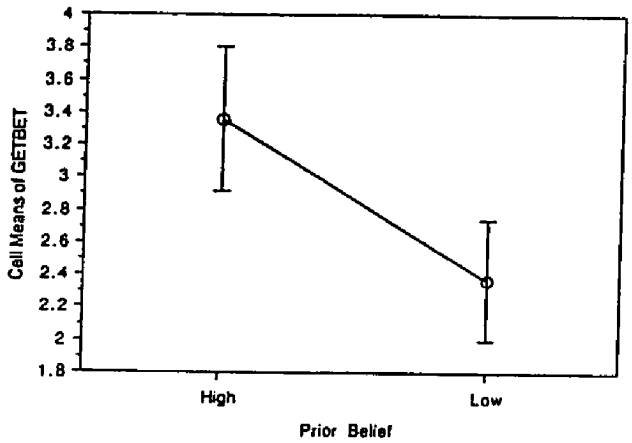


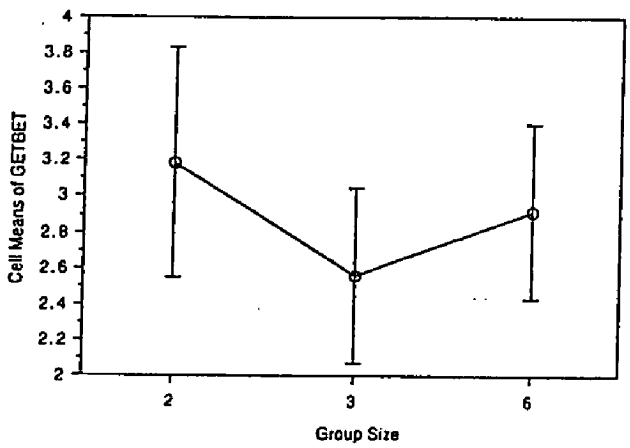
Figure Caption

Figure 14. Effects of Prior Belief and Group Size on Subjects' Judged Likelihood of Improvement on Another 20 Trials, for Subjects in the Facilitation (Individually Identifiable) Condition.

Judged Likelihood of Improvement (GetBet)
By Prior Belief in Psi Ability (Psi Efficacy)
With 95% Confidence error bars.



Judged Likelihood of Improvement (GetBet)
By Group Size
With 95% Confidence error bars.



Judged Likelihood of Improvement (GetBet)
By Group Size and Prior Belief in Psi Ability
With 95% Confidence error bars.

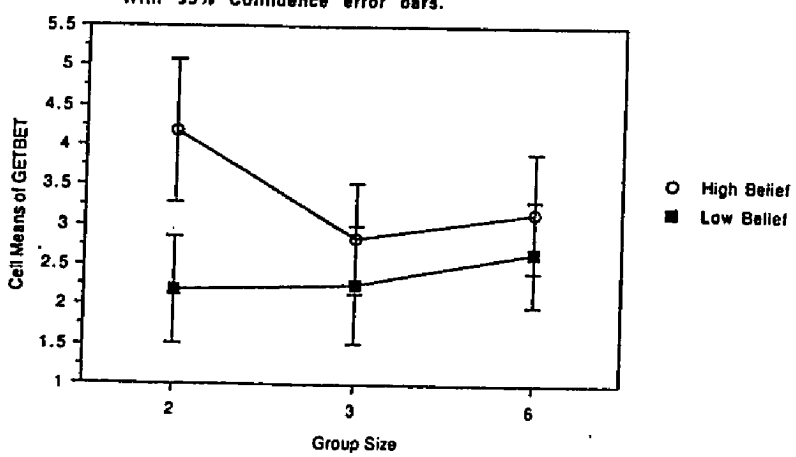
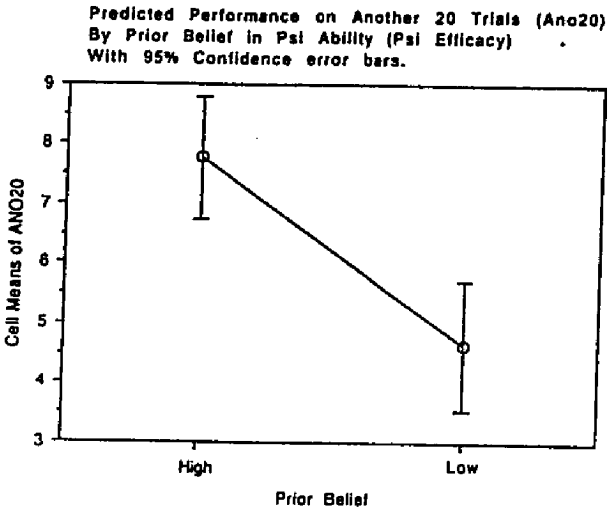
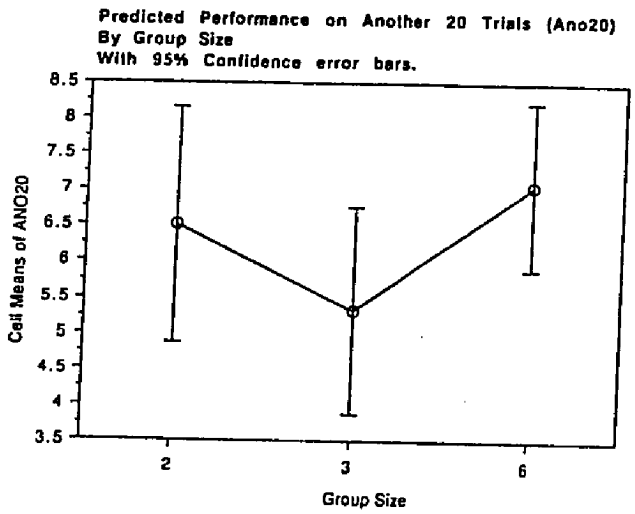


Figure Caption

Figure 15. Effects of Prior Belief and Group Size on Predicted Performance on a Hypothetical Second Set of 20 Trials, for Subjects in the Facilitation (Individually Identifiable) Condition.





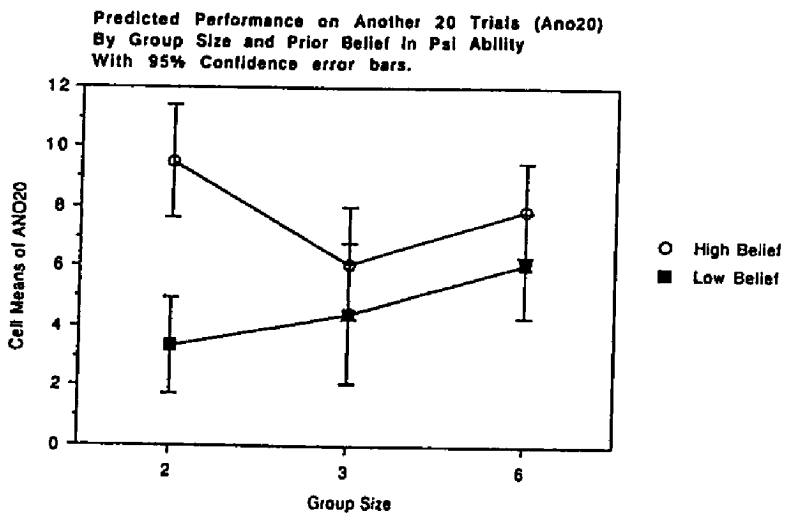
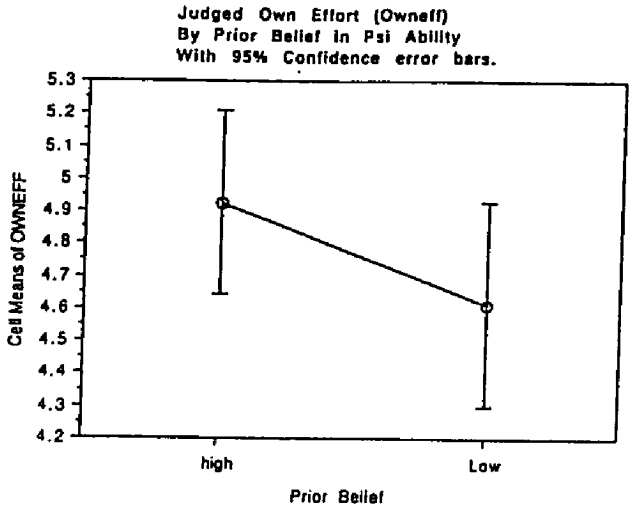
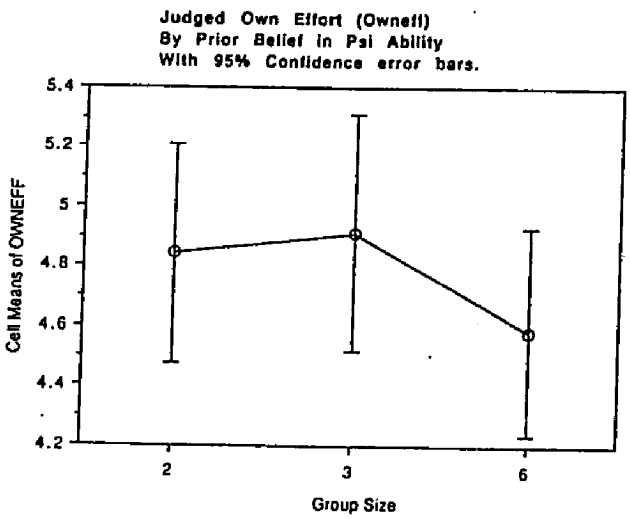


Figure Caption

Figure 16. Effects of Prior Belief and Group Size on Judged Own Effort, for Subjects in the Facilitation (Individually Identifiable) Condition.





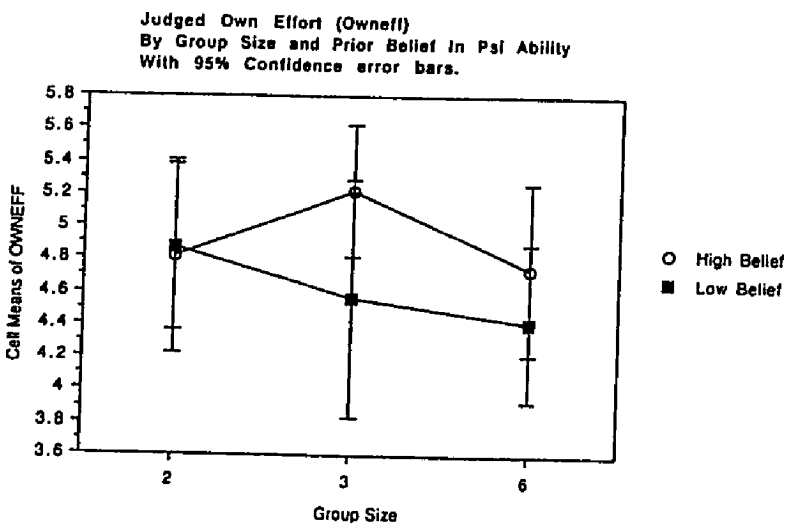
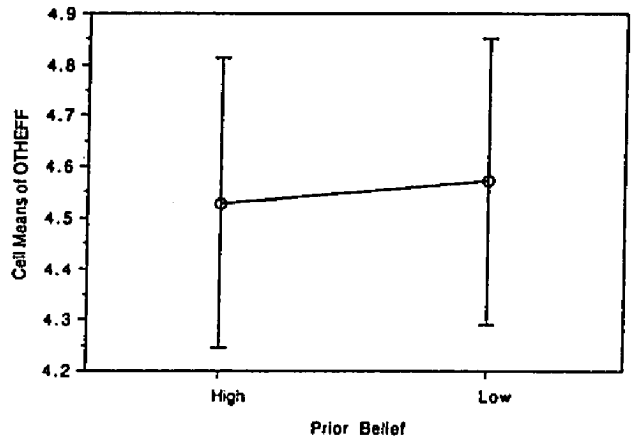


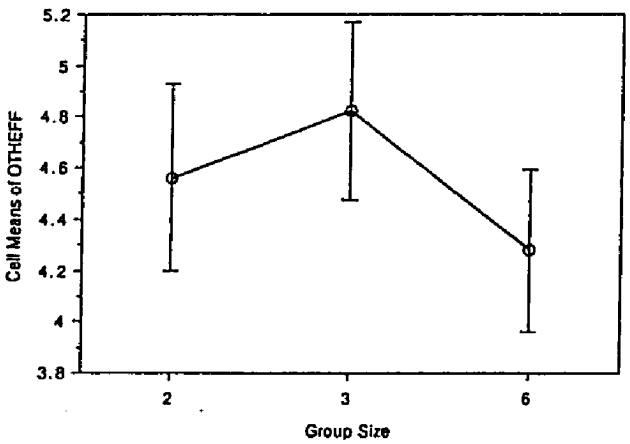
Figure Caption

Figure 17. Effects of Prior Belief and Group Size on Judged Other Effort, for Subjects in the Facilitation (Individually Identifiable) Condition.

Judged Effort of Other Group Members (Otheff)
By Prior Belief in Psi Ability
With 95% Confidence error bars.



Judged Effort of Other Group Members (Otheff)
By Group Size
With 95% Confidence error bars.



Judged Effort of Other Group Members (Otheff)
By Group Size and Prior Belief in Psi Ability
With 95% Confidence error bars.

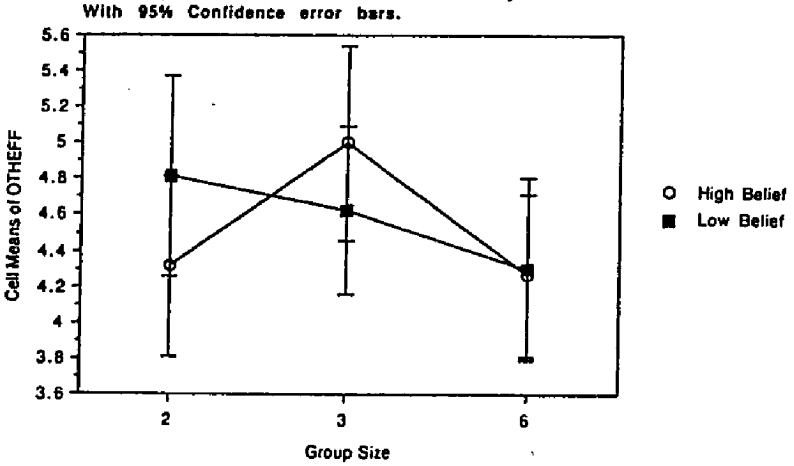
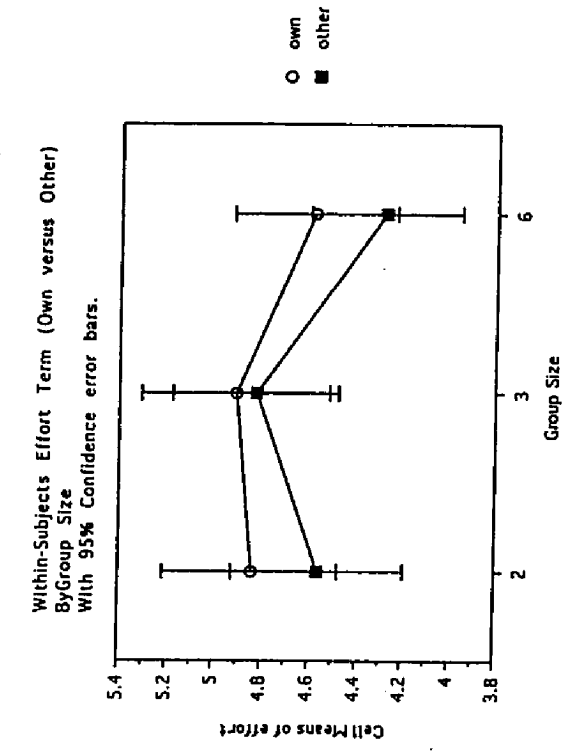
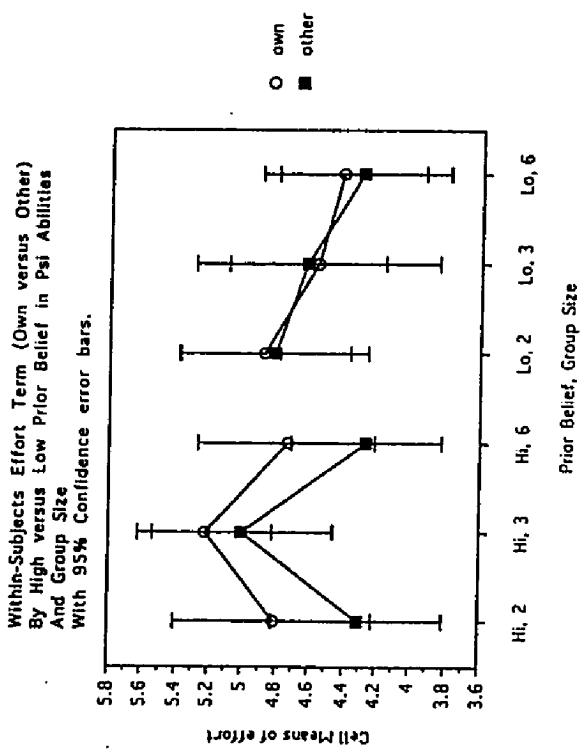
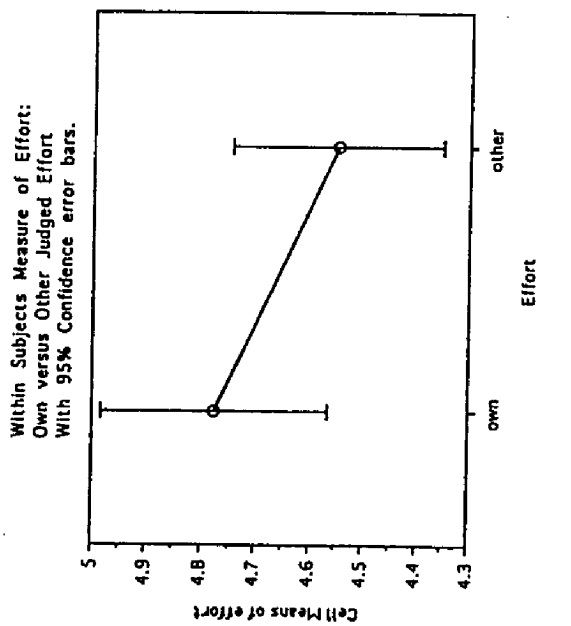


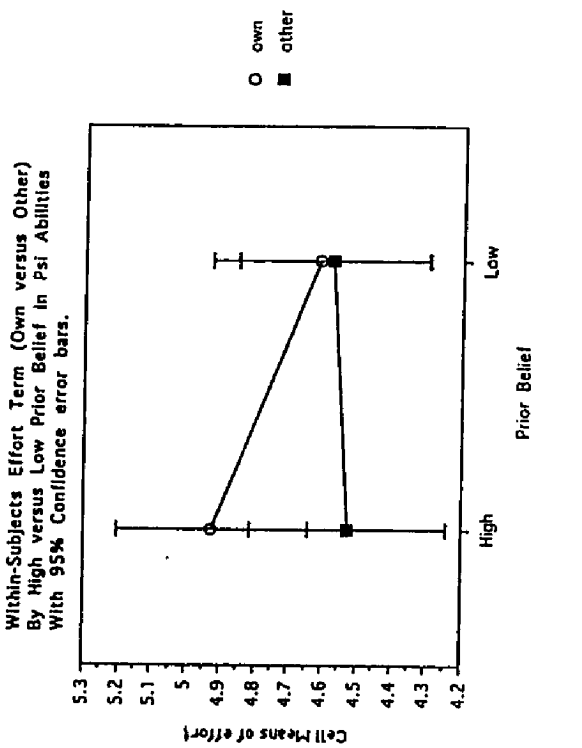
Figure Caption

Figure 18. Within Subject Effects of Own versus Group Effort, Prior Belief and Group size for Subjects in the Facilitation (Individually Identifiable) Condition.









APPENDIX A

EXPERIMENT MATERIALS: RUNNING RECORD OF CONFIDENT TRIALS

Running Record
Psychokinesis Experiment (Exp # 5)

Please indicate on this sheet whether you are confident or not that the color you chose actually does come up.

Trial	Yes--I am confident that my color did come up	No--I am not confident that my color did come up
1		
2		
3		
4		
5		
6		
7		
8		
9		
10		
11		
12		
13		
14		
15		
16		
17		
18		
19		
20		

APPENDIX B

EXPERIMENT MATERIALS: FOLLOW-UP QUESTIONNAIRE

Follow-up Questions
Psychokinesis Experiment (Exp. #5)

1 How much do you feel that your own concentration influenced what colors came up? That is, how much PK influence do you feel you have exerted?

1 2 3 4 5 6 7
Great influence no influence

2 How much do you feel that the concentration of the group influenced what colors came up? That is, how much PK influence do you feel all of you together exerted?

1 2 3 4 5 6 7
Great influence no influence

3 Do you feel you would get better on the task if you were given another 20 trials?

1 2 3 4 5 6 7
confident yes unsure confident no

4 If you were given another 20 trials, on how many do you think you would influence what colors would come up? _____(0-20)

5 On how many of the 20 trials you completed do you think you correctly predicted what colors came up? _____(0-20)

6 How hard were you trying to influence the die toss? Could you have tried harder? (answer honestly)

1 2 3 4 5 6 7
not really trying moderate effort extreme effort

7 How hard do you feel the others in your group were trying? Could they have tried harder?

1 2 3 4 5 6 7
not really trying moderate effort extreme effort

8 How much do you think the others in the group believe in psychokinesis?

1 2 3 4 5 6 7
a great deal somewhat not at all

9 How much do you think the experimenter believes in psychokinesis?

1 2 3 4 5 6 7
a great deal somewhat not at all

10 How well do you think the experimenter could tell your die apart from the other group members' dice?

1 2 3 4 5 6 7
could not tell mine could easily tell mine

11 Do you think that working with others affects one's psi abilities?

1 2 3 4 5 6 7
enhances no effect detracts

APPENDIX C

EXPERIMENT MATERIALS: BELIEF IN PSI ABILITIES INVENTORY (BPAI)

Belief in Psi Abilities Inventory
Mark J. Henn
1989

1. In games using dice, I manage to get just the roll I need:
   1 2 3 4 5 6 7
   never sometimes always

2. I can improve my bowling (or other equivalent) game by concentrating on the ball after it leaves my hand.
   1 2 3 4 5 6 7
   fully agree undecided fully disagree

3. While I may or may not have had "unusual" experiences, nothing has convinced me that I am at all "psychic."
   1 2 3 4 5 6 7
   fully agree undecided fully disagree

4. Occasionally, I will just be thinking of a person and he/she will call me.
   1 2 3 4 5 6 7
   never sometimes often

5. If I "get a feeling" about a coin toss, I have a better than random chance of "guessing" the outcome.
   1 2 3 4 5 6 7
   fully agree undecided fully disagree

6. Things that happen to me that others might call "psychic" I call just plain chance.
   1 2 3 4 5 6 7
   fully agree undecided fully disagree

7. With enough concentration, I could have some effect on a roulette wheel or some other gambling or lottery device.
   1 2 3 4 5 6 7
   fully agree undecided fully disagree

8. It sometimes seems that I can "will" things to happen.
   1 2 3 4 5 6 7
   never sometimes often

9. Belief in "psychic" phenomena is evidence of a weak mind.
   1 2 3 4 5 6 7
   fully agree undecided fully disagree

10. Some people say that everyone is a little bit psychic; I believe that I am psychic:
    1 2 3 4 5 6 7
    not at all a little bit moderately (or more)

11. I sometimes get brilliant or creative thoughts without knowing where they came from, and I feel it is possible that someone sent them to me.

1	2	3	4	5	6	7
never			sometimes			often

12. I can concentrate all I want to, but it still won't help me win any game of chance.

1	2	3	4	5	6	7
fully agree			undecided			fully disagree

13. Unusual personal experiences have convinced me that my "psychic" abilities are better than the average person's.

1	2	3	4	5	6	7
fully agree			undecided			fully disagree

14. I believe that people possess psychic abilities, such as ESP and PK.

1	2	3	4	5	6	7
believe			undecided			do not believe