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DEPRESSION, HAPPINESS AND THE ILLUSION OF CONTROL

by

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The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies for acceptance, a thesis entitled, "Depression, Happiness and the Illusion of Control" submitted by Geoffrey S. Powter in partial fulfillment of the requirements for the degree of Master of Science.

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ABSTRACT

Recent research on depression is characterized by a focus on complex cognitive dysfunctions and distortions as the source of disorders of mood. Several treatments have attempted to follow suit by directing efforts at identifying and altering these maladaptive cognitive processes. Despite the popularity of cognitive approaches, several authors have begun to question the accuracy of some of these proposals. They believe a fuller understanding of mood-related differences in basic cognitive processes is necessary before attempting to infer higher-order cognitions as the source of depression. The present research was designed in order to provide a better understanding of differences in one of the most basic cognitive evaluations: the judgement of response-outcome contingencies.

In a first experiment, 30 depressed, neutral, and dispositionally happy female college students were selected from a longitudinal study of responses to stress on the basis of their scores on the Beck Depression Inventory and the Costello-Comrey Depression Scale. Subjects participated individually in a laboratory task on which they were asked to quantify their perceived percentage of control over the onset of a light. Subjects were informed that the onset of the light would have some degree of contingency with the pressing or not pressing of a button. In actuality, subjects objectively had no control over the onset of the light. Analysis of perceived control estimates revealed that, contrary to suggestions in the literature that depressives would distort

their control, depressives were highly accurate judges of response-outcome contingency. In a high-reinforcement no-control situation however, non-depressives evidenced a robust distortion of control in the direction of overestimation.

It was further suggested that depressives might evidence the predicted distortion of personal efficacy if they were to unrealistically over-evaluate the performance of others on the task. To investigate this possibility, 60 depressed, neutral and dispositionally happy female college students participated in the non-contingent control estimation task in pairs. One member of each pair actively participated in the button-press task while the other passively observed. Both members were then asked to quantify the control achieved by the participant. Contrary to predictions, reliable participant/observer differences in estimate accuracy were not found. Depressives remained relatively accurate judges of control, whereas neutral and happy subjects continued to display inflated control, although to a lesser degree than found in the first study.

Two crucial points were brought up which have relevance to future investigations. Firstly, the character of contingency judgements, particularly the illusory control of non-depressives, may be to a degree task-dependent, and thus perhaps difficult to generalize to real-life situations. Secondly, when numerical estimates of control in both experiments were compared with verbal labels of control provided by the subject, the two were surprisingly unrelated. It is clearly important for future research then, to determine whether verbal, accurate labels, or inaccurate numerical ones are the basis for future activity.

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INTRODUCTION

The current literature on depression is echoing a general trend in psychology by emphasizing the importance of cognitive processes in determining the nature of disorders of mood. Whereas theorists of the past conceived of depression as being the result of exposure to environmental events such as loss (Bibring, 1957), current theorists focus instead on the causal role of misperceptions of, and resulting maladaptive interactions with the environment by the depressive. Cognitive theories have been offered for the etiology and maintainance of depression, and have formed the basis for the development of several therapeutic approaches currently in vogue. Research in the area typically involves attempts to identify maladaptive thought patterns characteristic of depressives, for example, attributional styles. Cognitively oriented therapy follows logically by identifying for the client these typical maladaptive cognitive styles, and providing for him/her more profitable ways of viewing situations (see Beck, Rush, Shaw & Emery, 1980, for an elaboration of a therapeutic approach which typifies this view).

Despite the current popularity and apparent therapeutic efficacy of the cognitive approaches to depression, several authors (notably Coyne & Lazarus, Note 1; Wortman & Dintzer, 1978) have questioned their perspicuity. These authors contend that numerous gaps exist in the current understanding of depression as a result of the growing tendency to infer cognitive dysfunctions as the root of depression. Their criticism is indirectly butressed by a maze of inconsistent results

found in the literature on the cognitions of the depressed. It seems that this inconsistency is primarily attributable to the complexity of the cognitve dysfunctions proposed as the source of depression and the absence of data on depressed/non-depressed differences in more basic cognitive functions. The earlier cognitive literature (e.g. Heider, 1958; Kelley, 1967) proposed that the foundation of all higher order cognitions is the evaluation of environmental contingencies by an organism. The present research was designed to examine depressed/non-depressed differences in these evaluations of contingencies.

The Role of Environmental Contingencies in Directing Behavior

It is intuitively clear that contingencies, both between environmental events, and between organismic responses and their outcomes, play a role in determining the nature and direction of behavior. A more intriguing question is whether or not an organism forms an internal, subjective representation of the operative environmental contingency which is isomorphic with the objective evidence. That is, is the environment represented accurately by the organism, or is it altered for some reason? According to several theorists (e.g. Bindra, 1976; Bolles, 1972; Dawson and Furedy, 1976; Mackintosh, 1975) organisms must accurately represent their environment internally in order to adaptively interact with their surroundings. Seligman, Maier and Solomon (1971) have proposed a simple model to represent this interaction in which environmental events and personal responses are evaluated by the organism according to past experience, and are then internally

symbolized to form the basis for future behavior. Seligman et al (1971) and Bindra (1976) have suggested that two components are essential for the adequate assessment of the efficacy of a particular response. The organism must first represent the conditional probability of an outcome given the response (p(O/R)), then subtract from this value the probability of the outcome in the absence of that same response ($p(O/\overline{R})$). The efficacy of the response then is defined as the magnitude of this difference ($p(O/R)-p(O/\overline{R})$) (see also Alloy and Abramson, 1979; Abramson and Sackheim, 1977; Jenkins and Ward, 1965, for an elaboration of this calculation). The adaptiveness of combining these two components in calculating the efficacy of behaviors is clear. Responses which are fruitful will be facilitated, and less productive ones will be suppressed (Baker, 1976; Mackintosh, 1975).

Evidence exists however to suggest that subjective representations may not be isomorphic with the objective. Based on the logic of the model described above (Seligman et al, 1971) Martin Seligman and several of his collegues have demonstrated that animals exposed to response-outcome non-contingency develop a condition which is termed "learned helplessness", and is characterized by a lack of such isomorphism between internal representations of contingency and actual contingencies when response -outcome contingencies are reintroduced into the situation This condition has been widely demonstrated in dogs (Seligman & Maier, 1967); rats (Baker, 1976; Seligman & Beagley, 1975); cats (Seward & Humphrey, 1967); and humans (Glass & Singer, 1972; Krantz, Glass & Snyder, 1974; Langer & Rodin, 1976; Maier & Seligman,

1976; Miller & Seligman, 1973,1975; Pennebaker, Burnham, Schaeffer & Harper, 1977; Rodin, 1976; Sherrod, Hage, Halpern & Moore, 1977).

The role of subjective representations of contingencies in producing learned helplessness is a fundamental one. According to Seligman (1975; Seligman & Maier, 1976) when an organism is placed in a situation in which desired outcomes are independent of any behavior in its repertoire, the organism forms a cognitive representation of non-contingency. Successive experiences with this non-contingency will produce a strong expectation on the part of the organism that responses and outcomes will also be independent in the future. This expectation is directly reponsible for the production of three deficits. An immediate consequence is impairment of the representation of future contingencies. The expectation of non-contingency is said to be sufficiently robust that non-contingency will be represented even in the presence of objective contingency in future situations. This interference with the recognition of contingency has been termed an associative or cognitive deficit. The second deficit, termed a motivational one, is defined by a lack of incentive to initiate voluntary responses following exposure to non-contingency. Because of the expectation that responding will not effect desired outcomes, responding is felt to be futile by the helpless animal. The third deficit, essentially a by-product of the other two is an emotional one, and is characterized by heightened autonomic arousal and behavioral passivity.

The associative, or cognitive deficit is of primary importance in the context of contingency jugdements, particularly given its pivotal

importance in producing learned helplessness. As several authors have noted , the recognition and subsequent expectation of non-contingency is the seed for the development of such helplessness (Abramson, Garber, Edwards & Seligman, 1978; Abramson, Seligman & Teasdale, 1978; Huesmann, 1978; Klein, Fencil-Morse & Seligman, 1976, Seligman, 1975). Despite the centrality of the concept in the learned helplessness model, evidence for the associative deficit in both animals and man is at best indirect. In all early experiments, animal and human, the associative deficit is confounded with the motivational deficit, in that it is inferred from the passivity of the organism when given an opportunity for efficacious responding (For a more lucid discussion of this confounding, see Alloy and Abramson, 1979, and Costello, 1978).

Demonstration of the associative deficit in humans suffers from special problems. To show that humans exposed to uncontrollable aversive events expected outcomes to be independent of their responses, Seligman and collegues (Abramson, Garber, Edwards and Seligman, 1978; Klein & Seligman, 1976; Miller & Seligman, 1973, 1975, 1976) employed a method developed by Rotter and his associates (Rotter, Liverant & Crowne, 1961). Rotter et al (1961) demonstrated that subjects who participate in tasks which are response-dependent (skill determined) rather than response-independent (chance determined) will show greater changes in expectancy for success on future trials. Accordingly, Miller and Seligman (1976), and Klein and Seligman (1976) examined expectancy changes of helpless and non-helpless students in an ostensibly skill task. As predicted by the helplessness model, helpless students showed less expectancy change following the skill task than did the control

group though both groups had performed equally well. No differences were found on the chance task. The authors concluded that subjects made helpless demonstrated little change after the skill-based success because they attributed the success they achieved to chance rather than skill. It should be noted that in the experiments proportions of success trials on both skill and chance tasks were predetermined by the experimenter.

The signal problem with the use of Rotter's method in the context of learned helplessness was, once again, the confounding of the motivational and associative deficits (Alloy and Abramson, 1979; Costello, 1978). A second, equally important problem is that quantitative inferences drawn from this method concerning the associative deficit rest on rather indirect grounds (Alloy and Abramson, 1979). It is a large jump to infer inaccuracy of contingency representation from changes in task expectancies.

The connection between helplessness and this discussion of cognitive deficits in depression is made clear by Seligman (1975). Noticing a similarity between the behaviors of helpless subjects and those of reactive depressives, he proposed that learned helplessness serve as a model of naturally occurring depression in humans. The learned helplessness model proposed by Seligman, perhaps one of the most influential cognitive models of depression, clearly states that depressed/non-depressed differences in contingency representations lie at the heart of mood differences. However, the early work of Seligman and his collegues provides no conclusive experimental evidence for the presence of an associative deficit in depressives. The eminence of the

learned helplessness model is clearly threatened by an inability to demonstrate the existence of one of its fundamental components. Therefore, concentrated efforts have recently been directed at developing more direct ways of assessing depressed/non-depressed differences in subjective representation of contingency.

Recent Research On Contingency Judgements Of Depressives

Independent of the work with learned helplessness, Jenkins and Ward (1965) designed a series of experiments which seem to involve a more appropriately direct means of assessing subjective representations of contingency. In these experiments, subjects participated in five contingency judgement problems consisting of 60 trials each. All subjects encountered problems which had both contingency and non-contingency between responses (pressing one of two buttons) and outcomes (onset of a light panel which signalled either 'score', or 'no score'). As in the model of Seligman et al (1971), the objective degree of control held by the subject was defined as the conditional probability of the desired outcome given one response (p(O/R1)) minus the probability of the outcome given the other response (p(O/R2)). Subjects on each trial completed a response, then observed the outcome of their response choice. Following completion of each 60-trial problem, subjects directly quantified their perceived personal control [1] over the outcome by placing a contol estimate on a scale with values which ranged from 0-100% control. These subjective representations were then compared with objective contingency to determine the relationship between the two measures.

Contrary to learning theories which predicted that the internal representations must mirror the objective, subjects in the Jenkins and Ward studies were found to be poor judges of responses-outcome relationship, particularly when this relationship was one of non-contingency. Rather than relying on the relative efficacy of responses in determining the amount of control they had over outcomes, subjects generally based their judgements on the frequency of successful (onset of "score") outcomes. This disregard of failure feedback clearly constitutes a logically maladaptive strategy for determination of contingency. Jenkins and Ward concluded that it is in fact unlikely that the typical experimental subject has an operative concept of contingency.

The applicability and value of the Jenkins and Ward paradigm within the context of learned helplessness is clear. Firstly, subjective representations of contingency are far more directly tapped than was the case with the use of Rotter et al's (1961) method. Secondly, control judgements made by subjects within the Jenkins and Ward paradigm are quantified as some value between 0 and 100%, whereas the Rotter method permits only the inference of relative differences in representation of either gross contingency or non-contingency.

Two students of Seligman, Lyn Abramson and Lauren Alloy have recently published a series of papers which involve the resurrection of the Jenkins and Ward method as it applies to predictions derived from the learned helplessness model. Their first publication (1979) examined several facets of depressed/non-depressed differences in contingency judgements in four separate experiments. The method they employed is

essentially the same as that used by Jenkins and Ward, except for a few procedural alterations. According to Alloy and Abramson, the apparent reliance of subjects in the Jenkins and Ward study on the percentage of successes as the heuristic in determining control may have been artifactual. Because subjects were forced to choose between pressing one of the two responses buttons, the probability given no button press response was in fact disregarded in the original experimental analysis. Alloy and Abramson(1979, p. 456) suggested that the data of Jenkins and Ward imply that subjects actually did take into account this additional probability in making their control judgements. Because not pressing was an invalid responses in these experiments, subjects of Jenkins and Ward would be forced to estimate the probability of a score outcome given no button press as 0%. Thus percentage of successes would in fact be a realistic means of assessing control, albeit logically poor from an ecological perspective.

Jenkins in a recent study (Allan & Jenkins, 1980) redesigned the early method so as to permit only a press - no press response option, and found that subjects used far more logical heuristics in determining control. Taking these factors into account, Alloy and Abramson (1979) similarly decided to permit only pressing or not pressing as valid responses.

The first experiment in the Alloy and Abramson series investigated whether systematic differences existed in judgements of various degrees of contingency between depressed and non-depressed college students. According to the predictions of the learned helplessness model, depressives, because of the expectation of

non-contingency should underestimate their control over potentially controllable outcomes. These authors had subjects participate in only one problem, which consisted of 40 trials. Following completion of the 40 trials, which involved pressing or not pressing a button in order to achieve the onset of a light, subjects completed several scales designed to provide information about perceived control, as well as the sorts of information used by the subjects in forming their estimates. Contrary to the predictions of the learned helplessness model, as well as the findings of Jenkins and Ward (1965), both depressed and non-depressed subjects were relatively accurate judges of contingency, and appeared to logically base their judgements on the relative efficacy of responses.

A logical second step was to compare differences in the judgements of non-contingency as a function of mood. In a second experiment, Alloy and Abramson (1979) placed subjects in a situation in which they objectively had no control over the onset of the light, that is, where both pressing and not pressing were associated with an equal frequency of light onset. In order to determine if subjects relied on the number of successes in defining perceived control, subjects received either 25% or 75% reinforcement (onset of the light). Clearly, if subjects relied on the invalid heuristic of percentage of successes, they would report greater control in the high reinforcement condition. According to Alloy and Abramson, the learned helplessness model indirectly predicts that non-depressives, because of an experience with control will have an expectation of response-outcome contingency. As a result of interference effects due to this expectation, non-depressives should overestimate control when they objectively have none, while

depressives should be relatively accurate judges.

The prediction was clearly confirmed. As shown in Figure 1 (from Alloy and Abramson, 1979, p. 459), depressives were accurate judges in both problems, whereas non-depressives were accurate only in the low reinforcement condition.

When asked to judge control in the 75% reinforcement problem, non- depressives overestimated their control by a mean of approximately 40%. In this high density reinforcement problem, non-depressives relied on invalid heuristics in determining their control, such as percentage of successes. However, investigation into the data available to subjects indicated that the non-depressives were in fact aware of the conditional probabilities associated with the outcomes, but that they organized this information inappropriately. The finding that non-depressed subjects overestimate control associated with high reinforcement corresponds to a growing literature on a phenomenon termed the "illusion of control". The relationship of this phenomenon to contingency judgement will be discussed shortly.

Two final experiments in the first Alloy and Abramson paper add an interesting dimension to the demonstration of the non-depressive's inaccuracy in the judgement of non-contingency. In these experiments, subjects judged their control when it was associated with either desired or aversive outcomes. Experiment 3 (Alloy and Abramson, 1979) examined judgements in the non-contingent case and demonstrated that the illusory control of non-depressives was even more robust when associated with monetary gain. Non-depressives however, demonstrated no such illusion when perceived control was associated with monetary loss. The final

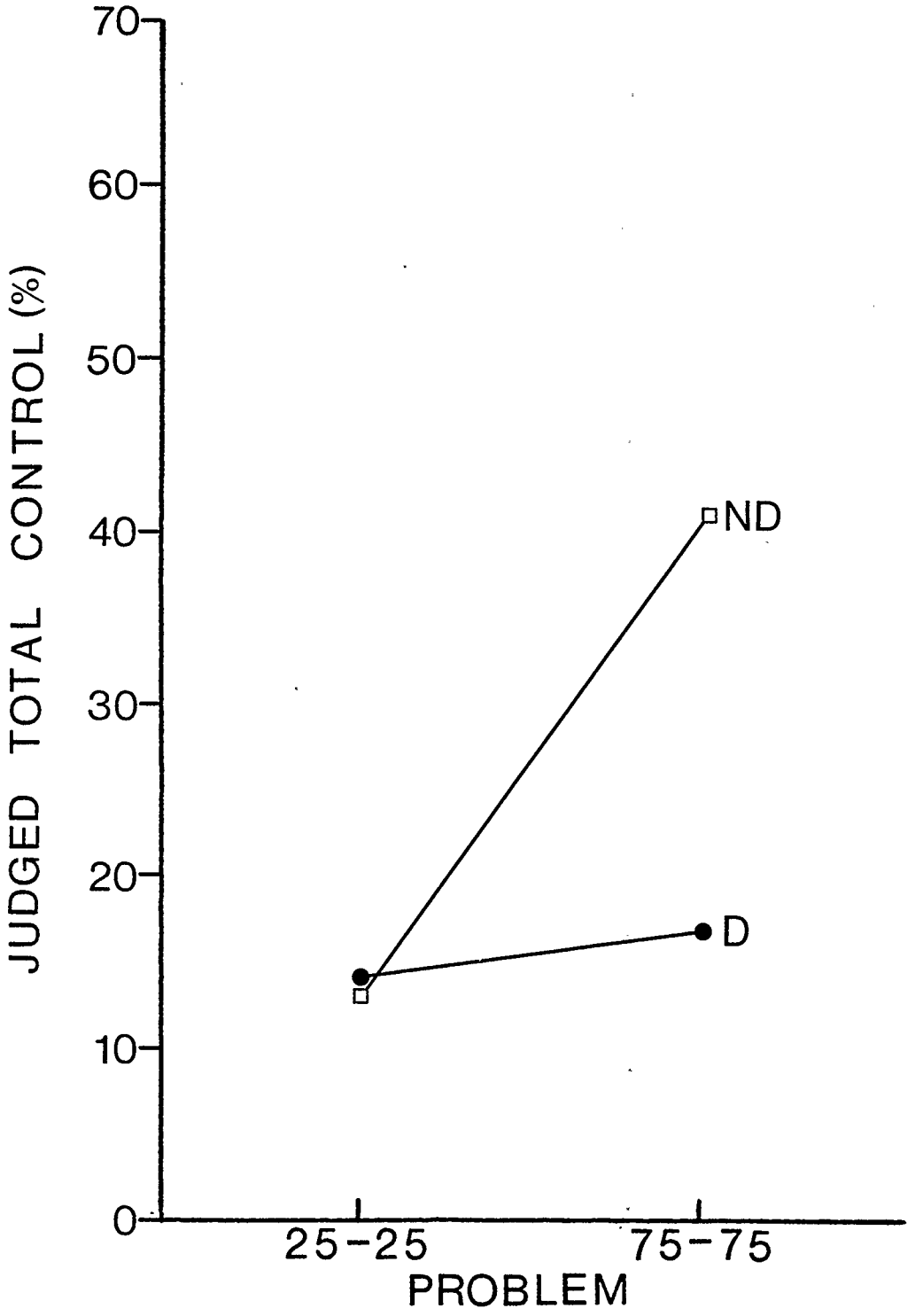

FIGURE 1 : Judged Total Control for Depressed (D), and Non-depressed (ND) Students. (From Alloy and Abramson, 1979, p. 459).

experiment in this series (Alloy and Abramson, 1979, experiment 4), had subjects judge contingent situations associated with loss or gain of money. It was predicted that in such a hedonically valent situation the underestimation of control on the part of the depressives predicted by the learned helplessness model would be more likely to emerge. Subjects exposed to a 50% control situation in which their responses were associated with either the loss or gain of money were asked to judge their degree of control over outcomes. Contrary to predictions, depressives were not affected by the loss/gain manipulations. Non-depressives however claimed very little control when this control was associated with monetary loss, while remaining relatively accurate in the win situation.

To briefly recapitulate then, this first report of Alloy and Abramson runs contrary to the predictions of learned helplessness. Depressives' judgements were accurate in all experiments. Non-depressives on the other hand, overestimated control when associated with high-reinforcement success, but did not lay claim to control when it was associated with failure.

Further research by these authors has delimited additional facets of depressed/non-depressed differences in perceived control. Alloy (personal communication, 1980) has replicated the differences using depressed and non-depressed psychiatric patients, rather than mildly depressed college students. Alloy and Abramson (Note 2) investigated whether exposure to uncontrollable noise (the typical learned helplessness paradigm) would reduce the illusory control of non-depressives. Clearly, the helplessness model would predict that

experience with uncontrollability should proactively inhibit expectations of control. Once again, the predictions of the learned helplessness model were not confirmed. The illusory control judgements of non-depressives exposed to uncontrollable noise remained robust. Depressed subjects remained accurate judges irrespective of experience with noise.

Abramson, Alloy and Rosoff (Note 3) examined the possibility that the simplistic responses-outcome relationships involved in the Jenkins and Ward method may have obscured actual depressed/non-depressed differences in the recognition of more complex patterns of contingency. To assess whether depressives had in fact an associative deficit in the recognition of more complex responses-outcome relationships, or whether they merely have a lesser tendency to generate complex hypotheses (analogous to a purely motivational deficit), subjects were placed in one of two experimental conditions. Half the subjects in each mood group attempted to generate the potential outcome controlling response themselves, while the other half had a small pool of hypotheses provided by the experimenter. It was found that depressed subjects in the "self-generated" condition were less adept at performing the controlling response, and judged that they had less control than subjects in any other group. When complex hypotheses were provided for them however, depressed subjects had no difficulty in accurately assessing contingency. These results were felt to support the presence of a motivational, but not an associative deficit.

From the perspective of the learning theories which dictate that subjective representations of contingency should mirror the objective to

adaptively direct behavior, and from the position of cognitive approaches to psychopathology which suggest that depressives will be characterized by unrealistic appraisals of situations, these clear demonstrations of depressive accuracy are entirely unexpected. Equally puzzling is the relative inaccuracy and illogic of the non-depressives. Though puzzling, this non-depressive inaccuracy was not entirely unexpected.In fact, the illusory control reported in the present context of learned helplesness is butressed by an extensive literature dealing with the parameters of these "illusions of control". This literature has been introduced in this roundabout fashion here with the purpose of emphasizing directly the relationship of contingency judgements to cognitive theories of depression, in particular learned helplessness. The nature of the illusions of control will now be discussed.

Evidence For The Illusion Of Control

The pivotal research on the illusion of control has been carried out by Ellen Langer and several co-workers (Langer, 1975; Langer & Roth, 1975; Langer & Benevento, 1978). In her earliest efforts, Langer (1975; Langer & Roth, 1975) demonstrated that when normal subjects are placed in objectively chance situations which include several elements characteristic of skill situations (e.g. competition, apparent choice, active involvement) they behave as if they actually have control over the outcomes. In all her research, Langer has attempted to demonstrate that this behavior represents a lack of isomorphism between subjective and objective contingencies. Unfortunately, Langer's choice of method

only indirectly taps the subjective representations of contingency. In her typical experiment (Langer, 1975), she examines how the experimental addition of an element of skill affects the betting behavior of a subject. The relationship between such gaming behavior and the perception of odds by a subject is a relatively unexplored one, and therefore may be a poor choice of method for the assessment of contingency representations. Nevertheless, the results obtained by Langer are of interest to this discussion, even at a superficial level. According to Langer, the fact that normals act as if non-contingent situations were indeed contingent is evidence that normals have an expectation of controllability over outcomes. Thus, Langer suggests that the illusion of control is in effect the inverse of learned helplessness (Langer, 1975, p. 325). According to this position then, depressives should not evidence the illusion. This of course has been directly demonstrated in the work of Alloy and Abramson. Additional support for this supposition comes from research which has employed a method identical to that used by Langer. Golin, Terrell & Johnson (1977) tested differences in gaming behavior between depressed and non-depressed college students, and found, as expected, that depressives were more pessimistic about potential for success on a dice game than were non-depressives. Golin, Terrell, Weitz & Drost (1979) have since replicated this finding with a clinically depressed population.

Theoretical Perspectives On Contingency Judgement Inaccuracy

A consistent trend is documented in the literature to suggest that judgements of non-contingent situations by normals are inaccurate. Two divergent explanations for this finding have been offered. A position taken by several "information-processing" theorists implies that this judgemental inaccuracy can be attributed solely to the fact that man in general is a terribly ineffective judge of correlation. A distinctly different approach is taken by a group perhaps best described as self-theorists. This latter group suggests that a variety of ego-protecting mechanisms operate to prevent the mentally healthy individual from admitting to complete lack of control. Both of these explanations will now be examined in some detail.

Man as a Poor Judge of Correlation.

Two statements concisely summarize the results of most studies carried out on contingency judgements by mathematical psychologists: "...normal adults do not have a cognitive structure isomorphic with the concept of contingency."(Smedslund, 1963); and "...(man)has a disinclination towards forming contingency judgements."(Shweder, 1977). Many studies do exist which note inaccurate contingency judgements, and which serve to prompt such strong statements. Smedslund (1963) had nurses attempt to judge the contingency between symptoms and diagnoses which the provided data made objectively non-contingent. Judgements obtained showed no relation to actual contingency, but rather were related only to frequency of confirming correlations (conjoint occurrence of symptom and diagnosis with no attention to disjoint). A similar pattern of symptom - diagnosis relationship inaccuracy has been

documented by Golding and Rorer (1978).

Much of the support for the position that man is a poor judge comes from studies investigating the influence of objectively non-contingent reward on task performance. For example, Wright (1962) demonstrated that high levels of non-contingent reinforcement greatly disrupted fruitful response patterns. Bruner and Revusky (1961) found that subjects generated highly complex task response hypotheses when given non-contingent feedback even though such patterns were entirely unnecessary in a non-contingent task. Levine (1971) has provided evidence to suggest that subjects are extremely poor at establishing the circumstances under which feedback is in actuality non-contingent upon response.

In a similar vein, studies have shown that subjects have great difficulty dealing with the concept of randomness, indicating problems with statistical representations of events. Hake and Hyman (1957) found that subjects generated highly inaccurate prediction hypotheses when observing random series of binary digits. Jones (1971) likewise noted an inability of subjects to grasp randomness inherent in a task.

Hogarth(1975) has published a lengthy essay which serves a summary and proposed explanation for the faults of subjects as efficient calculators of probabilities. He quite succinctly states that as man "does not have the capacity to make what one might call 'optimal' calculations, man makes use of heuristics and cognitive simplification mechanisms" (Hogarth, 1975, p. 273). These heuristics include several described by Tversky and Kahneman (1971,1974; Kahneman & Tversky, 1973). According to these authors, these limitaions of man as an information

processor force him into accepting a selective range of information, as well as relying on invalid heuristcs in determining contingency.

In general, Hogarth concludes "...man is a selective. stepwise information processing system with a limited capacity, and... is ill-equipped for assessing subjective probability distributions." (1975, p. 273.) The results of these studies clearly should be considered when any conclusions are drawn concerning the validity of contingency judgements. An important question arises however when the accuracy of depressives described above is taken into account. If non-depressive inaccuracy is the product solely of information processing deficits, as Hogarth indicates, there is little apparent logic in suggesting that depressives would be any less likely to suffer from the same deficits.

A more parsimonius explanation for the depressed/non-depressed differences in such judgements is provided in the alternative view of the self-theorists.

Emotional-Motivational Explanations for the Illusion of Control

The most eloquent summary of the position that illusory control is a product of self-protective mechanisms is developed in an essay by Greenwald (1980). According to this author, the normal human adult is characterized by a number of egocentric biases designed to serve the purpose of preserving the organization and integrity of existing cognitive structures. In the context of contingency judgements, considerable evidence is offered to support the contention that judgemental inaccuracies may result from a variety of biases, at the

levels of information search, organization, and recall. Three primary categories of bias indirectly account for non-depressive inaccuracy, as well as depressive accuracy in estimates of contingency.

Greenwald suggests the first bias to be one of egocentricity. It is proposed that organization and recall of information is biased by a propensity to view the self as a central and causative agent in events and outcomes. Research on the self (for example, Brenner, 1973, 1976; Kuiper & Rogers, 1979; Markus, 1977; Rogers, Kuiper & Kirker, 1977) has demonstrated that information is remembered with greater facility if subjects consider the information as self-relevant. In a recent paper (Alloy, Crocker & Tabachnik-Kayne, Note 4) it was demonstrated that information search and recall of depressives was particularly affected by negative hedonic valence of outcomes when the outcomes were self relevant. In a sense, these findings confirm Hogarth's (1975) statement that man is a selective information processor, although within a different framework. The research above indicates primarily that self-relevant, emotionally laden information will be more readily recalled than will affectively neutral material. A second dimension of egocentricity, that of self as a causative agent is supported by studies by Jervis (1967) and Ross and Siccoly (1979). These authors both provide data supporting the observation that individuals will take credit for a far greater proportion of successes in a group effort than the objective evidence warrants.

The second and third biases described by Greenwald may be considered as one for the purposes of the present discussion. He uses the terms "beneffectance" and "cognitive conservativism" to describe the

selective perception and recall of information so as to preserve existing cognitive structures. Evidence for the tendency to maintain positive self perceptions by normals is legion. For example, Mischel, Ebbesen and Zeiss (1973, 1976) demonstrated that subjects selectively recall successes, even at the expense of disregard of failure. The relationship of beneffectance to depression is an intriguing and well investigated one. Buchwald (1977), Nelson and Craighead (1977) DeMonbreun & Craighead (1977), and Wener and Rehm (1975) have all provided data to support the contention that depressives underestimate the number of successes in a performance task whereas non-depressives underestimate failure feedback. In a similar vein, Rozensky, Rehm, Pry and Roth (1977) have shown that non-depressives are far more self-applauding than are depressives for similar performance. In one of the most ecologically representative studies, Lewinsohn, Mischel, Barton and Chaplin (1980) proposed the notion that non-depressives are characterized by a "warm glow" about their own efficacy. In a social setting, these authors showed that non-depressives greatly overestimated the degree to which others saw them positively, whereas depressives were essentially accurate judges of their own competencies. Finally, Fischoff, Slovic and Lichtenstein (1979), and Koriat, Lichtenstein and Fischoff (1980) emphasize that this bias in selectivity is likely a function of a motivation to confirm expectancies of outcome.

This evidence prompts several considerations. It has been argued that the non-depressive will differ from the depressive in his judgements of response-outcome relationships. Surprisingly, it appears

that the depressive rather than the non-depressive is the accurate judge of non-contingency. Through a variety of direct and indirect methods, it has been demonstrated that non-depressives are subject to distortions of non-contingency, perhaps as a product of self-defensive mechanisms. The healthy ego is felt to protect its own integrity through perception of controllability - even at the expense of distortion of reality. A number of authors, for example Gergen (1971), Greenwald, (1980) and Lefcourt (1973) have argued persuasively that the motivational value of perceived control far outweighs the drawbacks of contingency misjudgement.

From the perspective of several theorists (e.g. Freud, 1917/1957; Bibring, 1957; Becker, 1979) it is hardly surprising that depressives do not succumb to these biases and illusions of control. They describe the depressive as suffering from impoverishment of the ego. In their view, when the depressive is forced to judge his/her own efficacy, a characteristic lack of self confidence will greatly restrict the evocation of beneffectance and the like. Mood, they feel, is a relatively linear function of ego strength. Thus, it might be suggested, as one's mood increases, so will the robustness of the cognitive illusions described above.

An equally interesting prediction concerns the judgements made of the performance of others by groups differing in mood. If, as the data suggests, biases occur primarily as a function of self-reference, the nature of judgements made of the performance of others will likely have a very different character than those made of the performance of self.

These and other concerns form the rationale for the present research.

PURPOSE OF THE PRESENT RESEARCH

Several fundamental questions will be addressed in this research. First, as mentioned above, implicit in the explanation of the illusion of control offered by the self theorists is the prediction that the illusion is a function of mood. While this has been demonstrated in part by previous research, the present study extends the analysis of this relationship by including a dispositionally 'happy' group along with a depressed and a neutral group.

A second issue, of particular relevance to the learned helplessness model, is the examination made of the performance of others as a function of dispositional status. In a reformulated version of the learned helplessness model, Abramson, Seligman & Teasdale (1978) stated that any feelings of personal helplessness will be determined to a considerable degree by the performance of self relative to the performance of others on a task. If a subject performs objectively well, yet subjectively perceives others to be performing significantly better, helplessness might still ensue. Clearly then, it is of interest to examine depressed/non-depressed differences in the judgements of the control held by others. Depressives may be accurate about their own performance, but inaccurate about the performance of others which may affect the evaluation of their own performance. Evidence does exists to suggest that depressives and non-depressives differ in the way they evaluate others. In a study comparing college students identified as having either high or low self-esteem, Schrauger and Terbovic (1976)

found that high self-esteem subjects evaluated their performance on a task far more favorably than did the low, although both groups performed equally well objectively. When the groups evaluated the performance of others however, they were equally accurate. A criticism of this study within the present context however is the potential difficulty in equating measures of self-esteem with those of depression, despite the theoretical relationship of the concepts. A more relevant study is reported by Lobitz and DeePost (1979) in which depressed and non-depressed psychiatric patients were compared on the basis of reinforcement given to self and others for task performance. This study demonstrated that depressives were far more generous when rewarding others than when rewarding themselves, even though performances were identical in all respects. Non-depressives demonstrated exactly the opposite tendency. These results suggest that a more contextual evaluation of contingency judgements is essential to a full understanding of their role in motivating behavior.

A final issue central to the present research is the importance of independently replicating the finding of Alloy and Abramson that depressives differ from non-depressives in their judgements of contingency. As mention above, learned helplessness has proven an extremely influential model of depression, thus any evidence as damaging to it as is that provided by Alloy and Abramson should be subject to replication.

EXPERIMENT I

Overview

This study was designed partly as a replication of the one by Alloy and Abramson (1979; Experiment 2) in which it was found that depressives are relatively accurate in judgements of non-contingency, whereas normals significantly overestimate control in non-contingent situations with high reinforcement. A group of subjects, designated as dispositionally 'happy' were added to the present study in order to more clearly establish the relationship between illusory control and dispostion. All subjects were placed in an experimental situation in which they objectively had no control. Half the subjects received a high-reinforcement, no control problem, and the other half a low-reinforcement, no control problem. The experimental situation consisted of having subjects make one of two possible responses (pressing or not pressing a button) in order to achieve one of two possible outcomes (onset or absence of a red light). Following completion of 40 such trials subjects were asked to rate, through the completion of several scales, their judgements of control over these outcomes.

In line with the findings of Alloy and Abramson (1979), the following predictions were made;

1)no significant differences would exist between the three dispositional groups (Happy, Neutral and Depressed) in the low reinforcement problem;

2)in the high-reinforcement condition, depressives would remain accurate in reporting non-contingency. Neutral and happy subjects, on the other hand, would subjectively inflate reported control;

3)the influence of dispositional status would be such that happy individuals should inflate control judgements to a significantly greater degree than would the neutrals;

4)given that the inflated control judgements of both the neutrals and happy subjects would not be supported by objective data, they would be shown to use invalid, illogical heuristics in making these judgements, whereas depressives will utilize more appropriate information (i.e. the relative efficacy of responses).

METHOD

Subjects

30 female undergraduates, between the ages of 17 and 26, from the University of Calgary served as paid volunteers. Females alone were chosen for two reasons. Firstly, females represent a majority of depressives (Beck, 1967). Secondly, all subjects in this study had previously participated in a longitudinal study of responses to stress. Access to dispositional scores from this previous study allowed for a consistency check on scores from a first testing to a second testing. In order to attempt to replicate the Alloy and Abramson (1979) study, subjects were classified into depressed and non-depressed groups on the

basis of their scores on the Beck Depression Inventory (BDI)(Beck, Ward, Mendelson, Mock and Erbaugh, 1961). In accordance with the criterion established by Miller and Seligman (1973) subjects who scored 9 and above on the BDI were classified as depressed, and those with scores of 8 or below, non-depressed. To further subdivide subjects into happy, neutral and depressed dispositional groups, they were also given the Costello-Comrey Depression Scale (CCDS)(Costello & Comrey, 1967). This bipolar scale was used because it is reportedly less affected than are other measures by extraneous variables such as age, sex, anxiety level and social demand (Costello and Comrey, 1967; Costello, Christiansen & Rogers, 1974); as well as being more specifically intended for applications which involve non-clinical populations (Costello & Comrey, 1967). This 14-item questionnaire was designed to measure intensity of disposition, or tendency to experience a depressed or happy mood(Costello & Comrey, 1967). Boundaries for the three dispositional groups on the CCDS were Depressed= CCDS<28; Neutral= 35>CCDS<40; Happy= CCDS>44. Only those subjects whose scores on the CCDS at first and second testing placed them in the same dispositional category were included. Three subjects whose CCDS scores placed them in discrepant categories were dropped. The final sample consisted of 10 happy, 10 neutral and 10 depressed subjects.

Experimental Design

The experiment involved a 3 (Mood - Happy, Neutral, Depressed) X 2(Problem Type - 25-25, 75-75) factorial design. The two problem types

differed only in the amount of reinforcement obtained - either 25% or 75%. In each case, the first number of the problem designation represents the percentage of trials on which the onset of a red light occurred when the subject completed a button-press response. The second number represents the percentage of red light onsets when the subject did not press. As previously mentioned, the actual degree of control was defined as the difference between these two numbers. Therefore, despite differences in frequency of reinforcement, subjects in both problem types objectively had no control.

Dependent Measures

The measures on each subject were collected on a form entitled the Subjective Probability Assessment Questionnaire (SPAQ - see Appendix 1). This questionnaire was comprised of 5 scales which were completed by the subject following the experimental task. The first scale, Judgement of Total Control (JTC) asked subjects to judge how much control they had over the onset of the red light by placing an "X" somewhere on a scale with values ranging between 0-100%. The second scale, Judgement of Total Reinforcement (JTR) was similarly marked off, and asked subjects to estimate what the overall percentage of red light onsets was, irrespective of response. The third and fourth scales, Judgement of Reinforcement When Press (JRP) and When Not Press (JRNP) had subjects estimate frequency of onsets dependent on choice of responses. This information was collected to determine if subjects had available the data neccessary to compute the relative efficacy measure described above. The final scale, Statement of Certainty (SC) was included to

determine whether any dispositional group was more certain of their responses to the questionnaire. This final scale was marked off in units from 0 to 10.

Finally, subjects were given an open-ended question which asked for the sorts of information they used in making their decision, as well as for any information which would have convinced them otherwise. This question was included to investigate the bases for judgements made by the subjects.

Apparatus and Materials

Subjects sat at the end of a table with two pieces of equipment placed in front of them. The first unit was a black box 17cm X 25 cm X 7cm with a spring-loaded ELECTROL button-press located in the middle. This unit was connected via switching circuitry to a light display panel 30cm X 47cm, with a yellow light (labelled as "warning") 16cm from the top, and red light of the same intensity (labelled as "signal") 21 cm from the top. Both units were interfaced with a PDP-11 computer. The outcomes of responses (e.g. the onset or absence of a red light when the subject either pressed or did not press the button) were pre-programmed into the computer. This programme included 4 lists of randomly sequenced outcomes, two for the low reinforcement problems, two for the high. Within each problem type, one list was activated when the subject pressed the button, the other when she did not. If the subject in the 75-75 case, for example, decided to press the button, the first list would be activated, and she would either be reinforced or not. If she

decided to press again on the second trial, the second outcome of the "press" list would be effected, and so on. To ensure that actual percentage of reinforcement was as close as possible to either 25% or 75%, the total forty trials were broken down into blocks of four trials, with outcome sequence randomized independently within each block. In this fashion, it was assured that the obtained reinforcement would deviate from the desired by no more than 2.5%.

Procedure

All subjects received the CCDS as part of the longitudinal study. This first administration of the scale took place 9 to 10 months before the subjects were retested for the present experiment. All subjects were contacted by telephone and asked if they would be willing to participate in an additional study which would take only 1/2 hour. Subjects were informed that the present investigator would have access to no information from the longitudinal study, save for the original mood scale scores.

Subjects who agreed to participate came individually to a laboratory at the University of Calgary, and were immediately administered both the Costello-Comrey and the BDI. The experimenter was absent from the testing room during the completion of the scales. If the scores on the three scales (CCDSI, CCDSII, BDI) all placed the subject in the same dispositional group, she was randomly assigned to one of the two problem types, and was then instructed to sit before the experimental apparatus. If any of the scores placed a subject in

discrepant categories, she was paid $2.00, then dismissed. All subjects then listened to a tape-recorded set of task and scale completion instructions. The actual experimental procedure was slightly different from that used by Alloy and Abramson (1979), in order to overcome some methodological problems, as suggested by Alloy (personal communication). Each subject completed a problem which consisted of 40 trials. The beginning of each trial was marked by the onset of the yellow light. This light would remain on for 5 seconds, during which period the subject had the choice of either making a button-press response, or not pressing the button. If she decided not to press the button, the yellow light would be followed either by the onset of the red light, or by nothing. If she did decide to press within 5 seconds of yellow light onset, the yellow light would extinguish immediately, and the red light would either come on or stay off - depending on the pre-programmed sequencing of outcomes. Inter-trial intervals were randomly determined, with a range of 10-15 seconds and a mean of 14.

Instructions to subjects involved a complete description of potential responses and outcomes. The instructional set given to subjects is included as Appendix 2.

Following the instructions, if the subject had no questions, the experimenter left the room and the task began. After completion of the 40 trials, the subject was replayed the section of the instruction tape dealing with the concept of control. She then filled out the SPAQ and

was debriefed. All subjects who completed the study received $5.00 for their participation.

RESULTS

Mood Scales

Table 1 presents the means, standard deviations and range of scale scores obtained for the three groups.

Correlations obtained between the measures were as follows: CCDSI with CCDSII $r(30)=.91$ ($p<.001$); CCDSI with BDI $r(30)=-.86$($p<.001$); CCDSII with BDI $r(30)=-.83$,($p<.001$).

SPAQ

All scales on the questionnaire were subjected to a 3(disposition) X 2(problem type) analysis of variance.

The scores on the first scale, Judements of Total Control were analysed in raw form. As was found by Alloy and Abramson (1979), there were significant effects for mood ($F(2,24)= 4.07$, $p<.05$); problem type ($F(1,24) = 22.75$, $p<.01$); and a significant mood X problem interaction ($F(2,24)= 4.07$,$p< .05$). A simple main effects test (Kirk, 1968) revealed a significant effect for mood only in the high reinforcement (75-75) problem ($F(2,24) = 11.51$, $p<.01$), and a significant effect for problem only for the happy ($F(1,24) = 20.21$, $p<.01$) and neutral groups ($F(1,24) = 10.39$,$p<.01$). Thus, as was found by Alloy and Abramson, depressives were relatively accurate judges of control in both the high and low reinforcement problems, whereas non-depressives were accurate in the

TABLE 1

Mood Scale Means, Standard Deviations, and Ranges : Experiment 1

GROUP	SCALE	MEAN	STD. DEV.	RANGE
DEPRESSED	CCDS1	9.6	5.05	03-18
	CCDS2	4.8	13.57	-17-28
	BDI	23.5	4.92	17-31
NEUTRAL	CCDS1	37.5	2.06	35-38
	CCDS2	36.8	2.78	35-40
	BDI	2.5	1.90	01-06
HAPPY	CCDS1	44.2	10.20	44-56
	CCDS2	50.1	1.85	44-52
	BDI	0.7	0.94	00-03

low, but clearly not in the high reinforcement condition. (See figure 2 for a graphic depiction). To determine whether significant differences existed between the happy and neutral groups, a Newman-Keuls multiple comparison test (Kirk, 1968) was carried out on the 75-75 problem. This analysis revealed depressed's subjects judgements to be significantly lower than those of the non-depressed ($p<.05$), while the difference between the happy and neutral non-depressives did not reach significance (see Appendix 4 for full details).

To determine how accurate subjects were in assessing the overall frequency of red light onset, scores on the second scale, Judgement of Total Reinforcement, were transformed into discrepancy values by subtracting judged reinforcement from actual. Analysis of JTR revealed no significant main effects or interactions.

On the third scale, Judgements of Reinforcement When Press, analysis of discrepancy scores revealed a significant effect for problem type ($F(1,24) = 6.11$, $p<.05$). As shown in Figure 3, the non-depressives, both happy and neutrals, overestimated onset frequency in the 25-25 problem and underestimated in the 75-75; whereas depressives were relatively accurate in both problems.

Analysis of JRNP discrepancy scores revealed a significant problem effect ($F(1,24) = 8.87$, $p<.01$) with all groups being less accurate in the high-reinforcement condition where they consistently underestimated onset frequency. As illustrated in figure 4, this was particularly true in the case of the happy group, with a mean underestimation of 25% in the high-reinforcement condition.

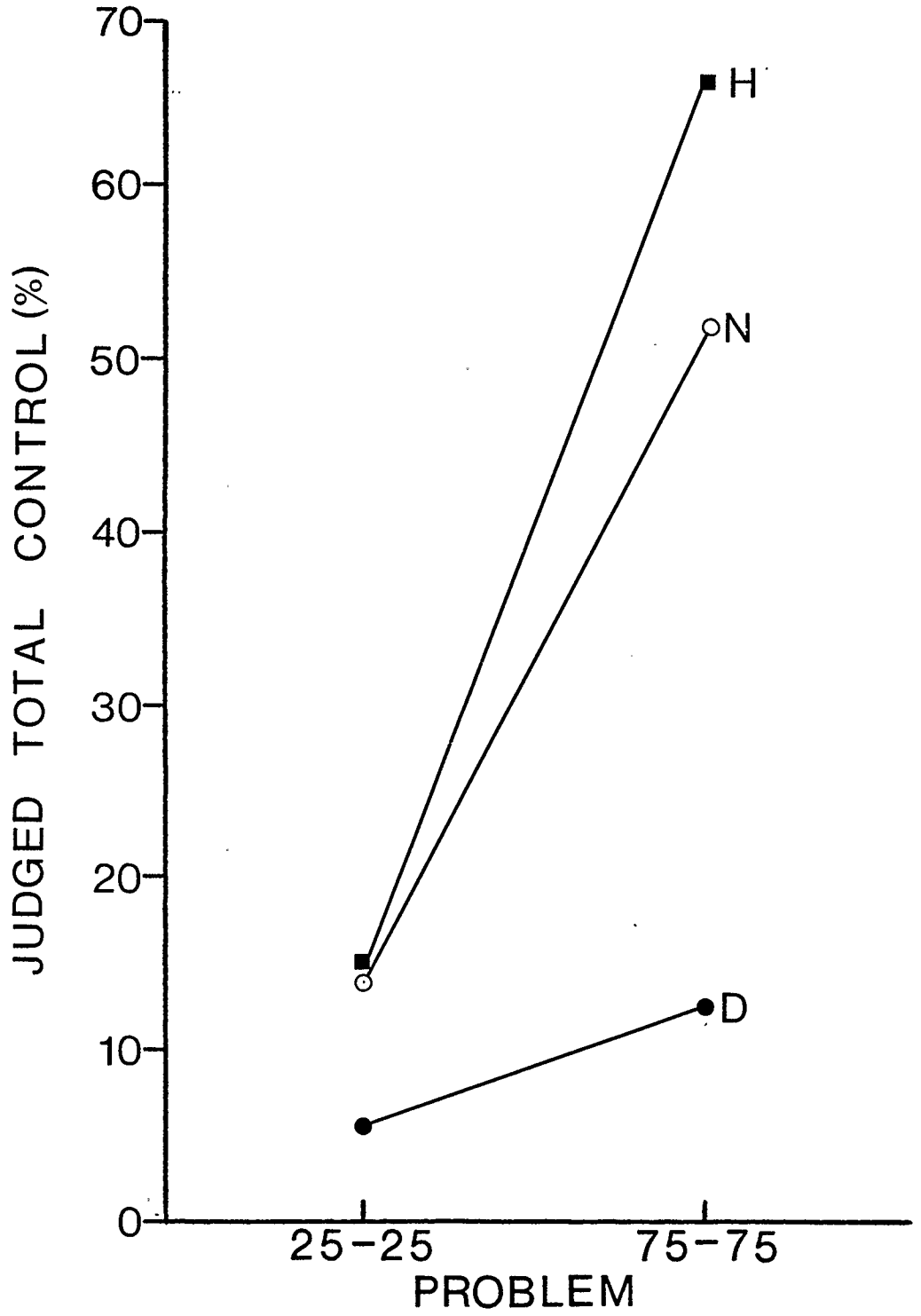

FIGURE 2 : Judged Total Control Scores for Depressed (D), Neutral (N) and Happy (H) Students - Experiment 1

TABLE 3

Mean Judged Total Control Scores

Happy, Neutral and Depressed Students

Experiment 1

		PROBLEM	
		25-25	75-75
	HAPPY	14.0	67.0
DISPOSITION	NEUTRAL	13.0	51.0
	DEPRESSED	5.6	12.0

TABLE 4

Mean Discrepancy Scores Between Judged and Actual Total Reinforcement - Happy, Neutral and Depressed Students Experiment 1

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	-2.0	3.0
	NEUTRAL	-2.0	3.6
	DEPRESSED	1.0	-1.0

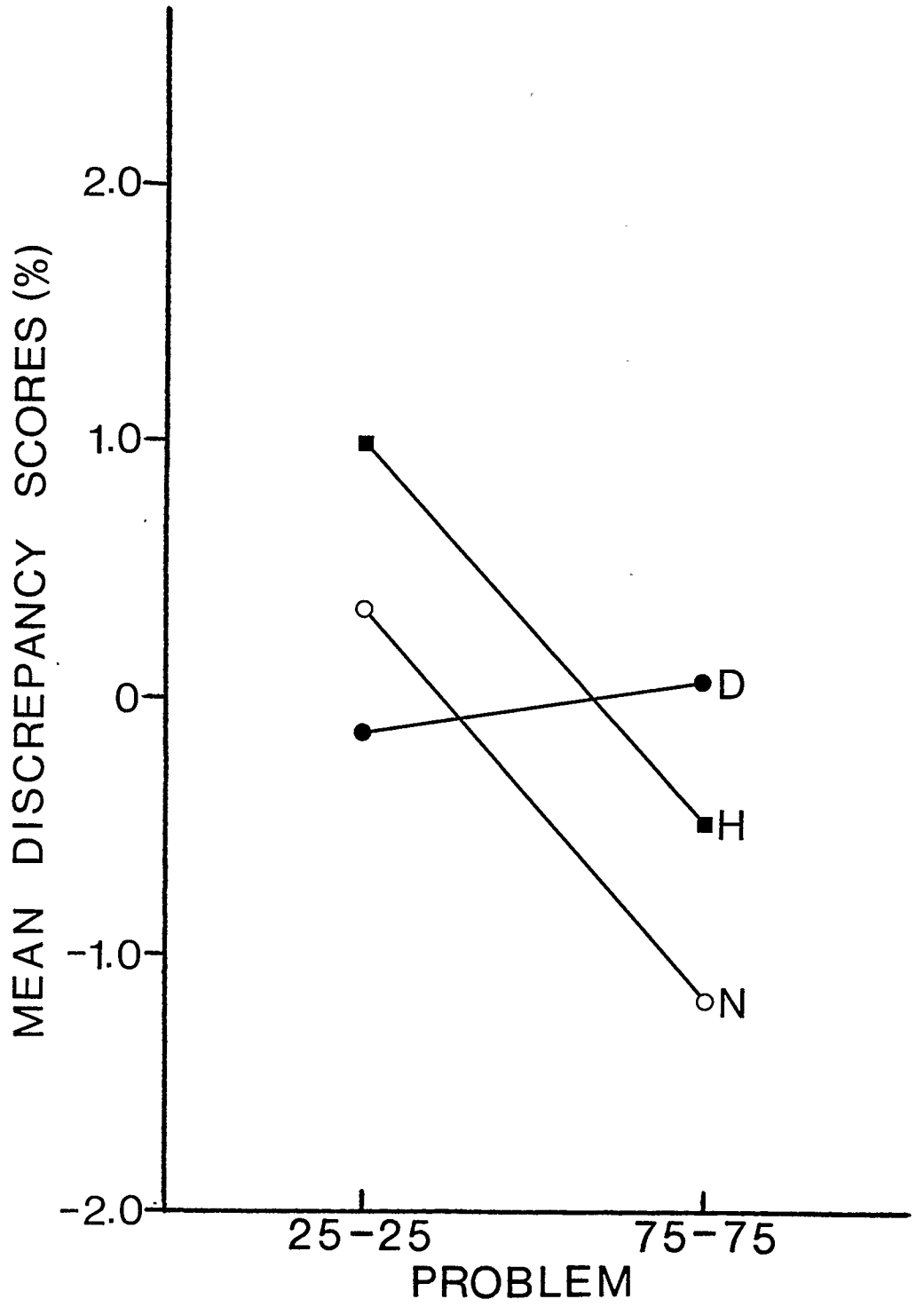

FIGURE 3 : Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Press - Depressed (D), Neutral (N), and Happy (H) Students - Experiment 1

TABLE 5

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Press - Happy, Neutral and Depressed Students Experiment 1

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	-0.4	1.0
	NEUTRAL	-1.1	0.3
	DEPRESSED	0.1	-0.1

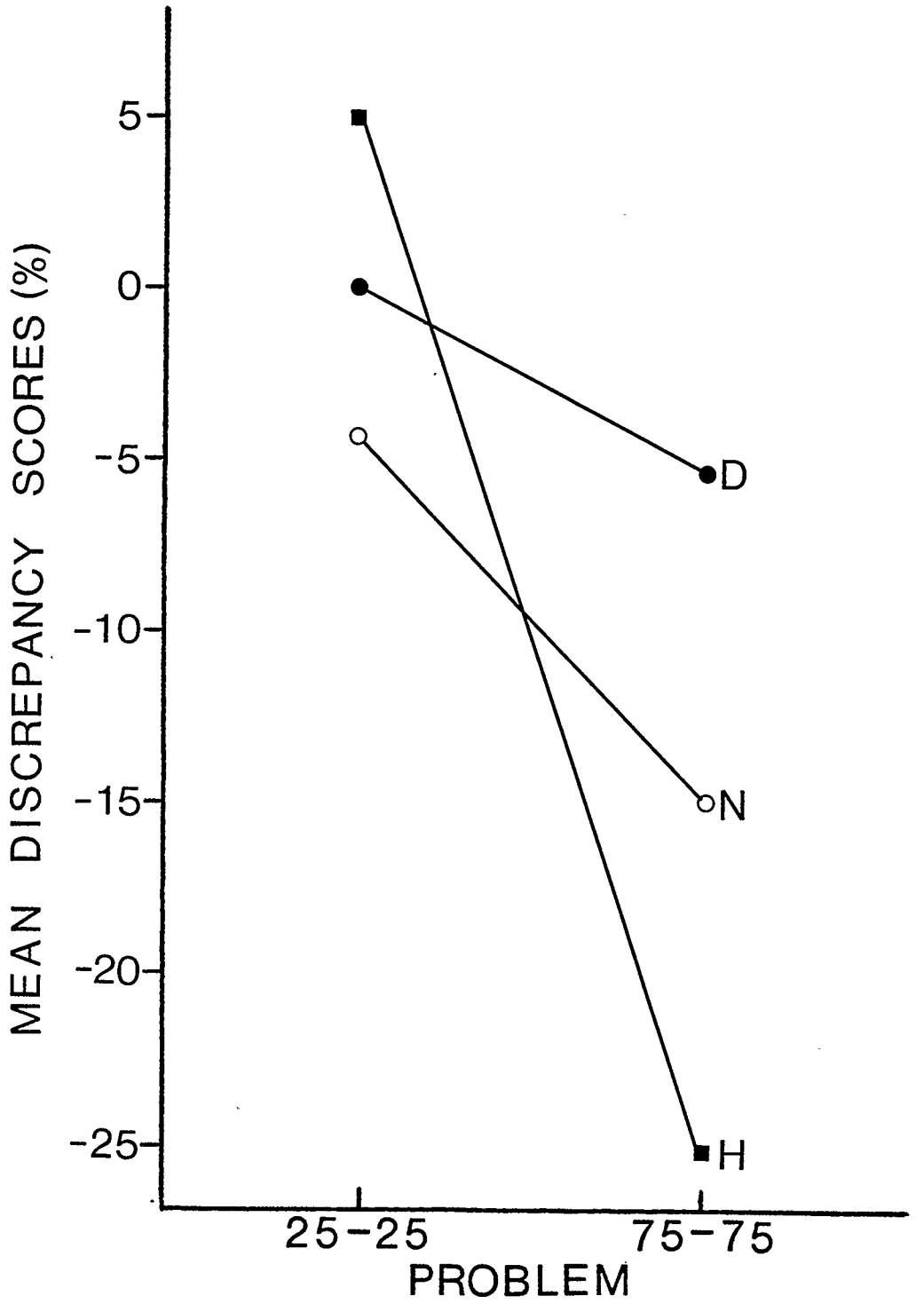

FIGURE 4 : Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Not Press - Depressed (D), Neutral (N), and Happy (H) Students - Experiment 1

TABLE 6

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Not Press - Happy, Neutral and Depressed Students

Experiment 1

		PROBLEM	
		25-25	75-75
	HAPPY	5.0	-25.0
DISPOSITION	NEUTRAL	-4.0	-15.0
	DEPRESSED	0.0	- 5.0

TABLE 7

Mean Statement of Certainty Scores

Happy, Neutral and Depressed Students

Experiment 1

		PROBLEM	
		25-25	75-75
	HAPPY	5.6	6.2
DISPOSITION	NEUTRAL	7.4	5.4
	DEPRESSED	7.8	6.6

The analysis of the final scale, Statement of Certainty revealed no significant main or interaction effects.

Supplementary Analyses

The results obtained prompted a search for the source of errors in judgement made by the non-depressed subjects. To determine the basis for judgements of total control, these scores were correlated with a number of heuristics which the subjects might have employed in making their judgements. Jenkins and Ward (1965) for example, found that subject's judgements of contingency were better related to number of successes, and/or confirming cases than they were with actual relative efficacy of responses. For the purposes of the present analyses, successes were defined as the number of red light onsets when the subject pressed the button. Confirming cases were defined as the number of successful trials plus the number of times the subject did not press the button and the light did not come on. Further comparisons were made between the control judgements and the overall percentage of reinforcement (JTR), and a measure described as subjective differential efficacy (SDE). SDE was calculated by taking the judged reinforcement when press (JRP) for each subject, and subtracting from it her judgement of reinforcement when not press (JRNP). This latter value is of course similar to that defined by our measure of objective control ($p(O/R)-p(O/\bar{R})$), with the advantage of being exclusively based on subjective, rather than objective conditional probabilities. From the point of view of the subject then, this calculation would be the best

basis for judgements.

Correlation coefficients were calculated separately for each group between JTC and each of these 4 heuristics. Depressive JTC scores were found to correlate with SDE to a significant degree ($r(10)=.72$, $p<.01$), but not with the other heuristics. This demonstrates a relatively correct selection of basis for judgments on the part of the depressives. Non- depressives on the other hand were shown to rely on invalid heuristics to a significant degree. The JTC scores of the neutral group correlated significantly with both successes ($r(10)=.49$, $p<.04$), and judged total reinforcement ($r(10)=.52$, $p<.03$), but not with the other measures. Happy group JTC scores correlated with number of successful trials ($r(10)=.53$, $p<.04$), judged total reinforcement ($r(10)=.63$, $p<.03$), and confirming cases ($r(10)=.61$, $p<.02$), but not with SDE. Thus, as predicted, the accurate depressed subjects utilized the most appropraite information in forming their judgements, whereas the non-depressed used data which guaranteed inaccuracy.

Post-Experimental Questionnnaire

Depressed subjects generally responded with statements which corresponded logically with their low numerical estimates of control, for example, "I felt I had no control. I tried to look for a pattern or relationship between my presses and the light coming on, but it seemed to work all on its own". Similar statements were made by the non-depressives in the low reinforcement condition. In the 75-75 problem however, the JTC scores of the non-depressives tended to be based on

circumspect criteria. As mentioned above, several invalid heuristics seemed to serve as determinants of judged control. Several non-depressives, particularly those of a happy disposition, reported the use of complex hypotheses, such as the following time-dependent one: "...if the button was pressed within two seconds of the onset of the signal light on even trials, the red light would go on. On odd trials, I had to press within the last two seconds of yellow light. Once I learned this strategy, I had 100% control." This particular happy subject, even though reinforced 75% of the time when she did not press, reported a JRNP value of 5%!

One of the most intriguing findings from the post-experimental questionnaire was the discovery that a full 40% of non-depressives in the 75-75 problem wrote that they had little, or no control. This clearly stands in contrast to their numerical illusions of control. To determine if those subjects in the non-depressed group whose PEQ did not indicate feelings of little control differed in their JTC measures from those who reported little or no control, a t-test was carried out. Only those non-depressed subjects who clearly stated little or no control, versus those who reported "I had control", or some similar phrase were included in the analysis. Because there were no JTC differences found in the low reinforcement problem, only the 75-75 questionnaire was analysed. On this problem, 4 subjects reported little or no control, and 4 reported high control on the PEQ. No significant differences were found between the two groups on JTC scores ($t(6)=-.72$). This indicates, surprisingly, that verbal and numerical estimates of control are not isomorphic.

DISCUSSION

The findings of Alloy and Abramson (1979, exp't 2) were clearly replicated. Three of four original hypotheses were confirmed. Depressed, Neutral and Happy subjects were all relatively accurate judges of control in a non-contingent problem with low reinforcement (25-25). When non-contingency was associated with high reinforcement, depressives remained relatively accurate in judging low control. However, non-depressives, both happy and neutral, were highly inaccurate in their judgements at this reinforcement level, consistently overestimating control. Hypothesis 3, which predicted that happy individuals would show a significantly greater illusion of control than would the other two groups, was not confirmed. Although the mean score of the happy group was higher than for the neutrals (67% vs. 51%), the difference did not prove significant. Analysis of the heuristics used by the groups in forming their judgements demonstrated, as predicted (hypothesis 4), that neutral and happy subjects seemed to base their estimates on invalid sources of information, whereas depressives used none of the tested invalid sources.

Only one effect proved significant in the present study which did not emerge in the work of Alloy and Abramson. All dispositional groups in the present study underestimated the percentage of red light onsets associated with not pressing in the high reinforcement problem, whereas

judgements in the 25-25 case were far more accurate. This pattern is particularly evident in the case of the happy group, who underestimated by a mean of 25% in the high reinforcement problem, while overestimating by 5% in the low. Looking back at both the analysis of invalid heuristics and at the post-experimental questionnaire, reasonable explanations for this observation can be found.Reading through the post-experimental questionnaires, which gave information about data the subjects found relevant, it can be seen that few subjects in the non-depressed groups made reference to control as it related to not pressing. Rather, most estimates seemed to be based on what happened after a button press. A more precise indication of this information-seeking bias comes from the investigation of heuristics used by the subjects. As documented above, both non-depressed groups relied heavily on number of successes as an indicator of control. It is conceivable that a subject who relies on goal-eliciting presses as the basis for control might interpret red light onsets associated with not pressing as an instance of failure. As pointed out in the introduction, several studies exist to support the idea that normals are suceptible to selective memory for success, and disregard of failure (DeMonbreun & Craighead, 1977; Mischel, Ebbesen & Zeiss, 1973,1976; Nelson & Craighead, 1977). Such a process may have been operating in this first study.

Independent replication of the work of Alloy and Abramson was fundamental to continuing on to complete further studies. Clearly, results obtained with the method and population employed in the present study and those of Alloy and Abramson (1979,1980) challenge the position

in the literature that inability to recognize non-contingency is a ubiquitous human characteristic. Rather, numerical expressions of perceived control appear to be a clear function of disposition - becoming less accurate as one becomes happier. Supplementary evidence from the present study however forces evaluation of the relevance of these numerical measures. Subjects in the groups which were the least accurate judges in numerical terms could not be so easily differentiated from the accurate depressives when compared on the basis of their responses to the post-experimental questionnaire. Lack of isomorphism between numerical and verbal reports of contingency brings to bear an important issue of the ecological validity of the present method. When a subject rates her control as 65% on a scale only vaguely anchored in the mid-ranges, then later responds to an open question by writing "I had little control", we must ask which of these representations of events would most likely guide and structure future behavior. Ability to determine contingency does not necessarily imply the use of such judgements in practice. Indeed, several authors (notably Peterson, 1980; Shapira, 1975) have suggested that laboratory elicitation of such judgements may not mirror real-life manifestations. It is possible that in vivo judgements may have quite a different character, perhaps for contextual reasons. One clear way this can be illustrated is to consider the influence of a social setting on control judgements as measures of personal performance. Two influential papers on the evaluation of personal performance (Abramson, Seligman & Teasdale, 1978; Bandura, 1977) explicitly demonstrate that to be truly relevant, such evaluations must be made relative to the performance of others. For example, in a

task in which everyone else achieves 20% control, one who manages 30% may feel she has considerably more control than if everyone else managed 70%. Similarly, if the accurate depressives in experiment 1 universally overestimate the performance of others whom they watch, they may be thereby altering the motivational and emotional effects of their own performance, despite their accuracy.

The object of the second experiment was to gain greater insight into the nature of control judgements in more representative social situations.

EXPERIMENT II

Overview

Experiment II was designed to investigate whether the disposition of a subject determined in any way the judgements she made of the control held by others. The method and procedure were essentially identical to those used in experiment 1, with the addition of 30 subjects who were randomly assigned to an observer condition. Participants and observers attended the experimental sessions in pairs. Both pair members came from the same dispositional group - either Happy, Neutral, or Depressed. While one subject actively participated in a no-control button press task, (once again with either high or low reinforcement frequency), the other subject sat to the side and watched. At the end of the task, both subjects judged how much control the participant managed over the desired outcome (onset of a red light). In accord with findings in the literature, the following predictions were made:

1)in line with the findings of the first study, depressives in the participant condition will demonstrate a capacity to accurately judge the absence of control in the high-reinforcement, non-contingent problem, whereas the non-depressed, both happy and neutral, will exhibit an illusion of control. In the low reinforcement condition, no group differences in JTC should exist;

2)in the observer condition, the reverse should hold true. Non-depressed subjects will accurately judge the lack of control held by the participants, whereas the depressed will overinflate the control held by those they observe;

3)if asked how well they would perform relative to the individuals they watched, the depressed observers will report low efficacy expectations. Non-depressives will report that they would do better;

4)as in experiment 1, non-depressive participants will rely on invalid heuristics in forming their control judgements whereas depressives will utilize valid sources. The reverse will hold true for the observer condition.

METHOD

Subjects

As in the first experiment, subjects in the second study were female undergraduates at the University of Calgary, ranging in age from 17 to 34. These subjects were drawn from two sources. 34 came from the longitudinal study of responses to stress, and 26 came from recruitment efforts in 2nd year psychology classes, for a total of 60 subjects. Subjects from the longitudinal study whose first testing CCDS scores were within dispositional group limits were contacted by telephone. Other subjects were approached during a tutorial period, and filled out

the CCDS. As in the first study, final assignment to a dispositional group was based on categorical agreement between CCDSI, CCDSII and the BDI. Mean time lapsed between first and second administration of the CCDS for the longitudinal study was 11.3 months, whereas it was 3.2 weeks for the subjects drawn from the classroom.[2] Criteria for assignment to dispositional group (Happy, Neutral or Depressed) were identical to those used in experiment 1. Of the 79 subjects originally contacted, 17 were dropped from the study because of disagreement between dispositional measures, and 2 because of equipment malfunction. Of the 17 who had discrepant disposition scores, 15 came from the classroom, 2 from the longitudinal study.

Following classification into dispositional group on the basis of their CCDS and BDI scores, subjects were randomly assigned to one level of problem type (25-25, 75-75) and one level of activity (participant - observer) with the restriction that each condition contain an equal number of subjects. Subjects were paid $5.00 for their participation.

Experimental Design

The second experiment involved a 3(disposition) X 2(problem type) X 2(activity level) factorial design. Each subject was assigned to only one combination of levels, giving a total of 5 subjects per cell.

As in experiment 1, the problems differed only in the frequency of reinforcement (onset of a red light at either 25% or 75%). In both problems, because the frequency of reinforcement was equal irrespective of responses choice, subjects objectively had no control over the onset of the red light.

Dependent Measures

Subjects who actively participated in the task completed the SPAQ as described in experiment 1 (see Appendix 1). Observers completed a similar scale, labelled the Subjective Probability Assessment Questionnaire - Observers (SPAQ-O). The first five scales of the SPAQ-O were identical to those of the SPAQ, except worded in the third person (e.g. "Please indicate... how much control the person you watched had over the onset of the red light"). An additional sixth scale was added to the SPAQ-O, labelled as Estimate of Personal Ability (ESTP). On this scale, with points ranging from -100% to +100%, subjects in the observer condition were asked to estimate whether they thought they would perform worse, better, or about the same as the person they watched if they had to complete the same task. This scale was added as a means of assessing dispositional differences in perceived self-efficacy. All subjects completed a post-experimental questionnaire identical to that described in the first experiment.

Apparatus

Subjects were kept in separate rooms during completion of the dispositonal scales and the probability assessment forms. The rooms were connected via a one-way mirror so that the observer could see the experimental apparatus while she listened to an audio-tape of instructions. The actual experimental apparatus was identical to that used in experiment 1, with the addition of a chair to the right and behind the participipant. From this vantage, the observer was certain of

seeing both the sequencing of lights and button presses, without being obtrusive.

Procedure

Subjects whose CCDSI scores placed them in appropriate categories were contacted by telephone and asked if they would be willing to participate in a short study. As in the first experiment, subjects from the longitudinal study were explicitly informed that the experimenter had no access to information from this previous study save for the mood scale scores. Subjects were contacted so as to arrive at the testing centre in pairs, with the stipulation that both were members of the same dispositional group. Upon arrival, both members were asked if they were personally acquainted with the other. If they were, they were immediately rescheduled with other subjects, then dismissed, so as to control for familiarity. If they had not previously met, they were escorted to the room with the experimental apparatus, and were given both the CCDS and the BDI to fill out in separate rooms. The experimenter was absent during this period. If any of the three scores (CCDSI, CCDSII, BDI) disagreed with the original assignment, the subject was paid $2.00, and the other was rescheduled. If all three scores were in agreement, the subjects were randomly assigned to either the participant, or observer condition. This assignment was explained to subjects as a means of cutting experimental time in half. Both were told that they would be doing essentially the same thing, except that one would watch.

In the period that followed, subjects were again separated, this

time to listen to the audiotape of instructions. Active participants heard the same tape as the participants in the first study, with the addition of a final paragraph explaining that the observer would watch, then fill out the same scale as the participant had in front of her. She was assured that no communication of results would be made to the observer. A final sentence warned the subject not to talk during the experiment. Observers heard essentially the same tape, with modifications designed to ensure that the subject understood her purpose as observer. (The observer instructional set is included as Appendix 3).

Procedure during the 40 trials which made up the problem was identical to that described for experiment 1. Following completion of the forty trials, subjects were again separated, and replayed the section of the tape dealing with the concept of control. They were assured by the experimenter that accuracy of judgements would not be revealed to the other subject.

After completion of the probability assessment questionnaires, subjects were re-united, debriefed, then paid $5.00.

RESULTS

Mood scales

Table 2 presents the means, standard deviations and ranges of scores on the scales by dispositional group.

Correlation coefficients calculated betweeen the measures were as

TABLE 2

Mood Scale Means, Standard Deviations, and Ranges : Experiment 2

GROUP	SCALE	MEAN	STD. DEV.	RANGE
DEPRESSED	CCDS1	16.5	13.32	-15-23
	CCDS2	14.4	12.43	-22-27
	BDI	16.9	6.91	10-36
NEUTRAL	CCDS1	36.5	1.27	35-39
	CCDS2	38.7	1.80	37-40
	BDI	2.9	2.78	00-06
HAPPY	CCDS1	47.4	2.6	44-53
	CCDS2	48.3	3.42	44-53
	BDI	3.1	2.85	00-08

follows; CCDSI with CCDSII ($r(60)=.95$, $p<.001$); CCDSI with BDI ($r(60)=-.79$, $p<.001$); CCDSII with BDI ($r(60)=-.81$, $p<.001$).

SPAQ and SPAQ-O

All scales (excepting the ESTP, to be discussed shortly) were subjected to a 3(disposition) X 2(problem) X 2(activity level) analysis of variance.

Analysis of raw scores on the first scale, JTC, revealed a significant effect for mood ($F(2,48)=4.28$, $p<.05$), and no other significant main or interaction effects. A Newman-Keuls multiple comparison test revealed the Happy and Neutral groups to be undifferentiated , whereas they differed significantly from the depressives ($p<05$)(see Appendix 5 for full details). As illustrated in Figure 5 depressives were relatively accurate on both problems, whereas non-depressives displayed the illusion of control at both levels of reinforcement. Of interest, 25% of depressives reported 0% control, as did 15% of the non-depressives. In the first study, only 5% of the non-depressives reported no control.

Analysis of JTR scores was carried out by calculating discrepancy scores between judged and actual percentage of reinforcement. Analysis of these discrepancy scores revealed a significant disposition X activity interaction ($F(2,48)= 3.45$, $p<.05$), and a significant Disposition X Problem X Activity interaction ($F(2,48)= 4.53$, $p<.05$). To clarify the triple interaction a simple simple main effects test (Kirk, 1968) was carried out. Results of this test indicated a significant effect for disposition only for observers in the

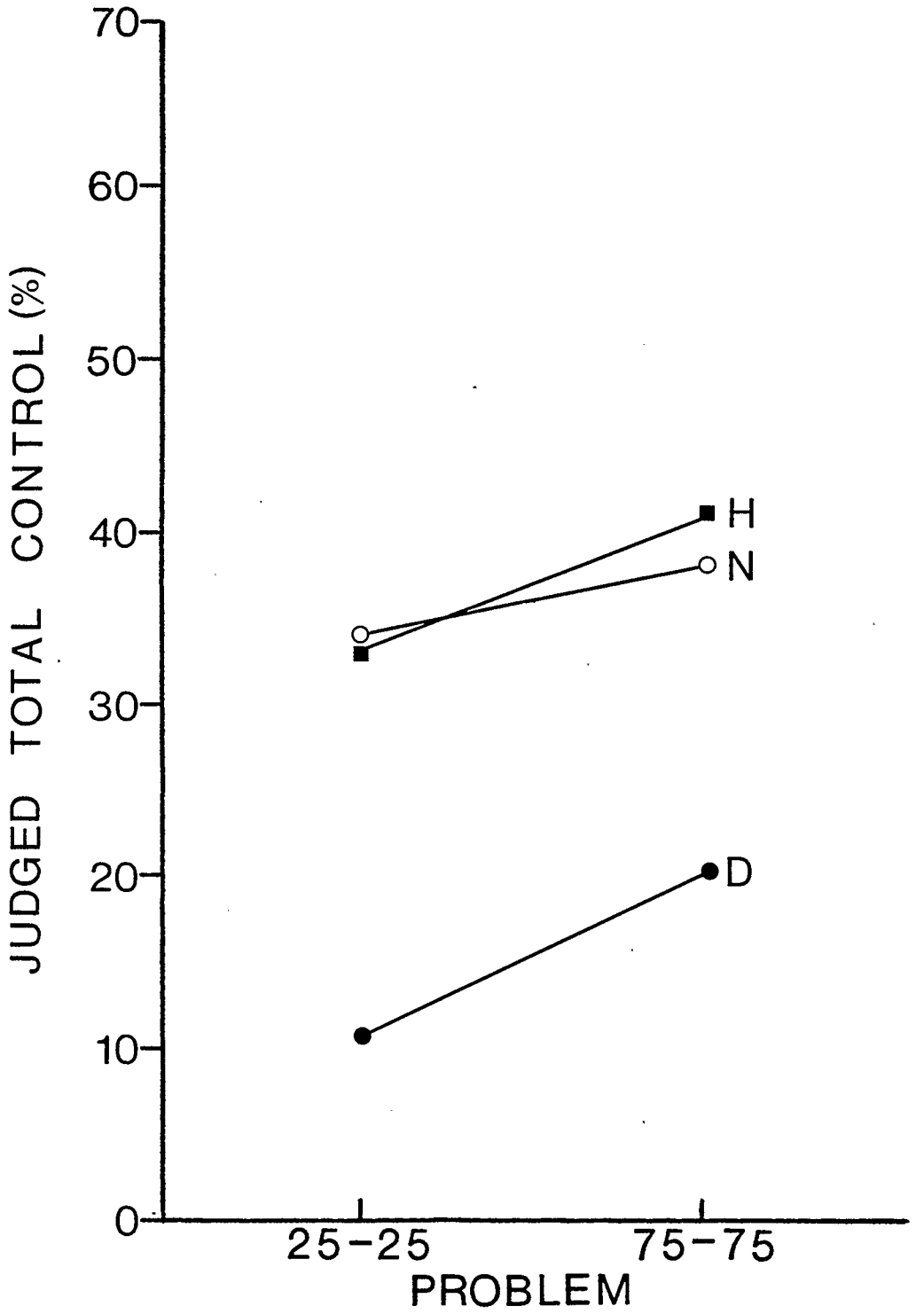

FIGURE 5 : Judged Total Control Scores for Depressed (D), Neutral (N), and Happy (H) Students - Experiment 2

TABLE 8

Mean Judged Total Control Scores

Happy, Neutral and Depressed Students

Experiment 2

		PROBLEM	
		25-25	75-75
	HAPPY	32.2	41.8
DISPOSITION	NEUTRAL	33.0	37.7
	DEPRESSED	11.7	20.0

TABLE 9

Mean Judged Total Control Scores

Happy, Neutral and Depressed Participants

Experiment 2

		PROBLEM	
		25-25	75-75
	HAPPY	26.4	56.6
DISPOSITION	NEUTRAL	18.0	37.0
	DEPRESSED	14.4	19.0

TABLE 10

Mean Judged Total Control Scores

Happy, Neutral and Depressed Observers

Experiment 2

		PROBLEM	
		25-25	75-75
	HAPPY	38.0	27.0
DISPOSITION	NEUTRAL	48.0	38.4
	DEPRESSED	9.0	21.0

25-25 problem ($F(2,24)= 7.11$, $p< .01$); a significant effect for problem only for neutral observers ($F(1,48) = 5.626$, $p<.05$); and a significant effect for participation for neutrals in the 25-25 problem($F(1,48) = 8.79$, $p<.01$). As Figure 6b indicates judgements of reinforcement by neutral observers in the low reinforcement problem are primarily responsible for the significance of the interaction and triple interaction effects.

Analysis of discrepancy scores on the third and fourth scales (JRP, JRNP) were carried out to determine if subjects had available the information necessary to calculate the relative efficacy measure described above. Analysis of JRP scores revealed no significant main effects or interactions. Analysis of JRNP discrepancies produced a significant effect of problem ($F(1,48)= 12.20$, $p<.01$), and a significant disposition x problem interaction ($F(2,48)= 3.24$, $p<.05$). As depicted in Figure 7 , happy subjects significantly overestimate reinforcement when associated with not pressing in the low reinforcement condition, and greatly underestimate it in the high reinforcement case (by a mean of -27.0%), whereas depressives and neutrals are comparatively accurate in the neutral condition, while underestimating in the high.

Analysis of statements of judgemental certainty revealed no effects.

ESTP scores of the observers were subjected to a 3(disposition) X 2(problem type) analysis of variance, and no significant main effects or interactions were found.

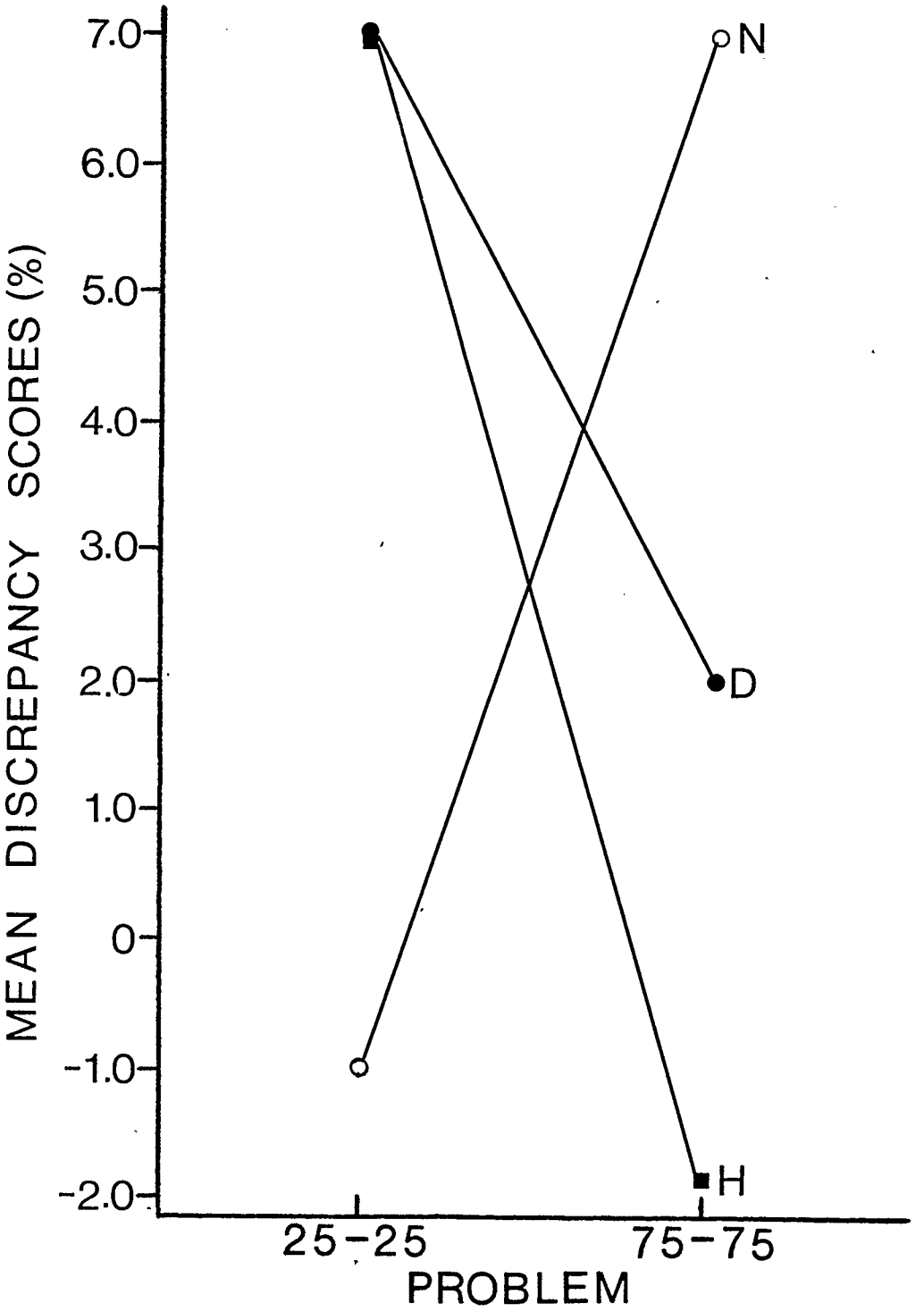

FIGURE 6a : Discrepancy Scores Between Judged and Actual Percentage of Reinforcement - Depressed (D), Neutral (N), and Happy (H) Participants - Experiment 2

TABLE 11

Mean Discrepancy Scores Between Judged and Actual Total Reinforcement - Happy, Neutral and Depressed Students Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	2.5	-2.7
	NEUTRAL	6.5	4.5
	DEPRESSED	2.0	-0.5

TABLE 12

Mean Discrepancy Scores Between Judged and Actual Total Reinforcement - Happy, Neutral and Depressed Participants Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	7.0	-2.0
	NEUTRAL	-1.0	7.0
	DEPRESSED	7.0	2.0

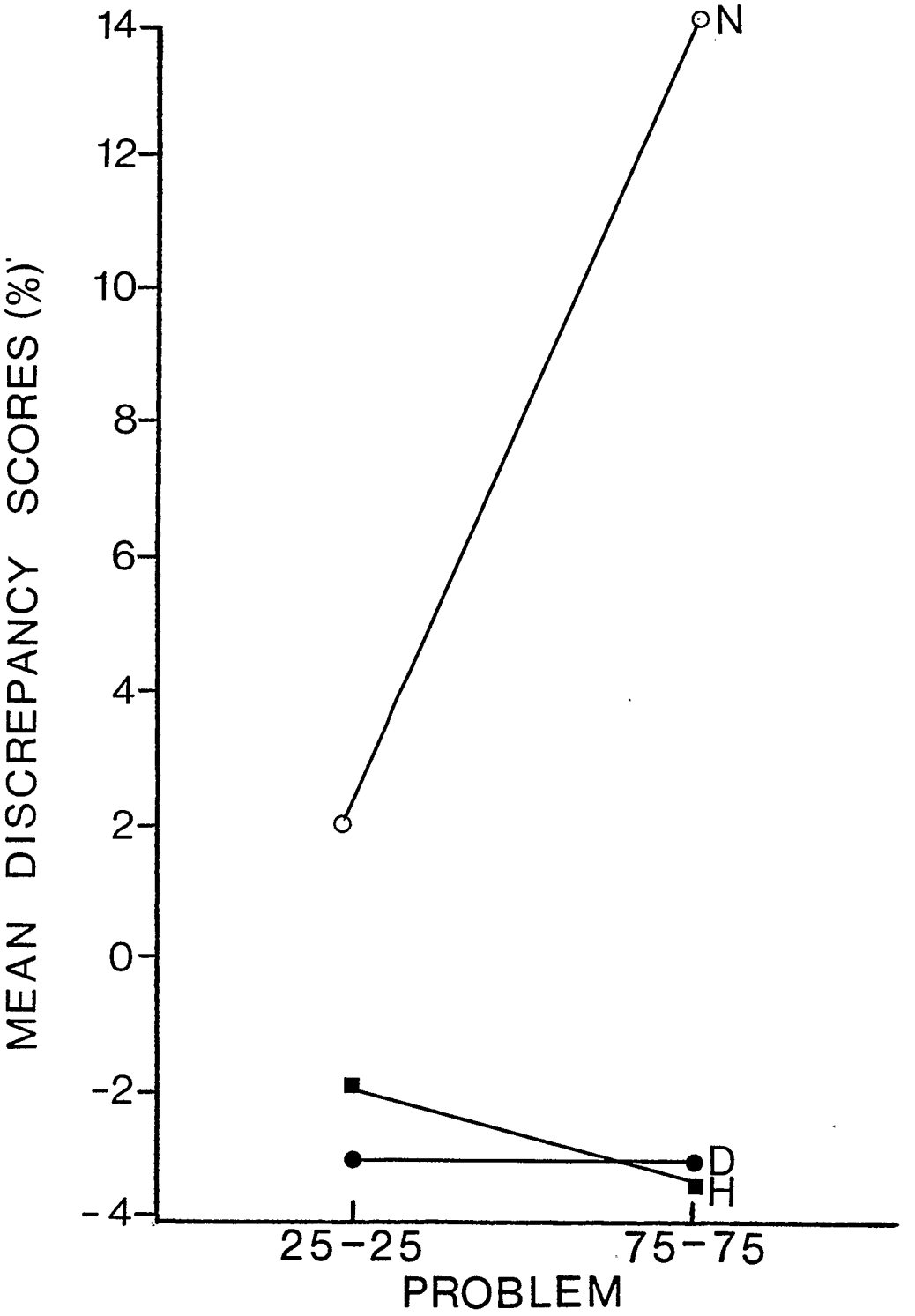

FIGURE 6b : Discrepancy Scores Between Judged and Actual Percentage of Reinforcement - Depressed (D), Neutral (N), and Happy (H) Observers - Experiment 2

TABLE 13

Mean Discrepancy Scores Between Judged and Actual Total Reinforcement - Happy, Neutral and Depressed Observers Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	-2.0	-3.4
	NEUTRAL	14.0	2.0
	DEPRESSED	-3.0	-3.0

TABLE 14

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Press - Happy, Neutral and Depressed Students

Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	-0.5	1.0
	NEUTRAL	-6.5	-1.0
	DEPRESSED	-1.5	-1.5

TABLE 15

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Press - Happy, Neutral and Depressed Participant

Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	-9.0	7.0
	NEUTRAL	-10.0	-1.0
	DEPRESSED	1.0	-1.0

TABLE 16

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Press - Happy, Neutral and Depressed Observer

Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	8.0	-5.0
	NEUTRAL	-3.0	-1.0
	DEPRESSED	-4.0	-2.0

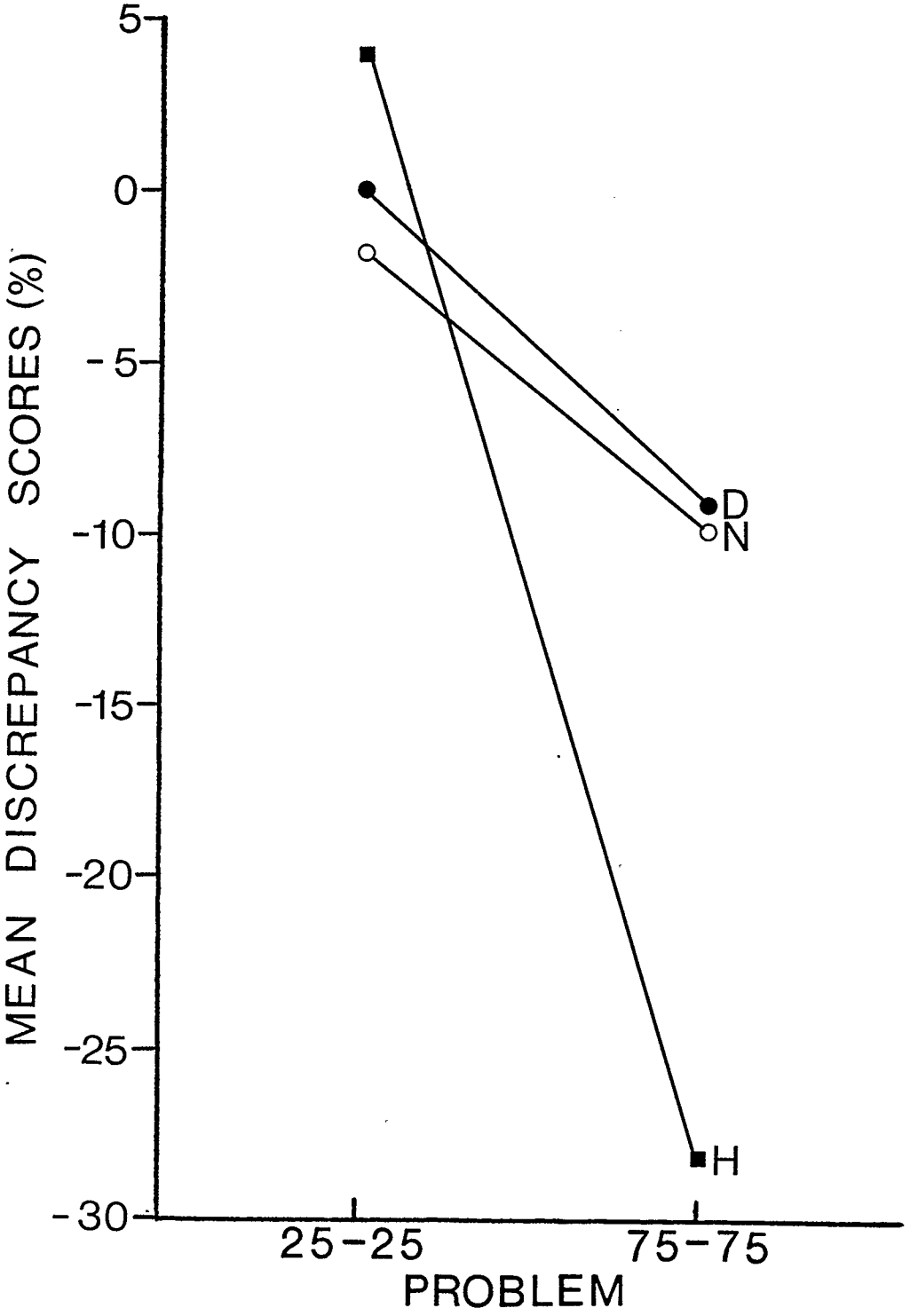

FIGURE 7 : Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Not Press - Depressed (D), Neutral (N), and Happy (H) Students - Experiment 2

TABLE 17

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Not Press - Happy, Neutral and Depressed Students

Experiment 2

		PROBLEM	
		25-25	75-75
	HAPPY	4.3	-27.5
DISPOSITION	NEUTRAL	-2.5	- 9.5
	DEPRESSED	0.0	- 8.2

TABLE 18

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Not Press - Happy, Neutral and Depressed Participants

Experiment 2

		PROBLEM	
		25-25	75-75
	HAPPY	2.0	-36.0
DISPOSITION	NEUTRAL	-4.0	- 2.0
	DEPRESSED	4.0	-15.4

TABLE 19

Mean Discrepancy Scores Between Judged and Actual Percentage of Reinforcement When Not Press - Happy, Neutral and Depressed Observers

Experiment 2

		PROBLEM	
		25-25	75-75
	HAPPY	6.6	-19.0
DISPOSITION	NEUTRAL	-1.0	-17.0
	DEPRESSED	-4.0	- 1.0

TABLE 20

Mean Statement of Certainty Scores

Happy, Neutral and Depressed Students

Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	7.2	6.6
	NEUTRAL	5.5	6.3
	DEPRESSED	5.7	6.2

TABLE 21

Mean Statement of Certainty Scores

Happy, Neutral and Depressed Participants

Experiment 2

		PROBLEM	
		25-25	75-75
DISPOSITION	HAPPY	7.4	6.4
	NEUTRAL	4.6	6.2
	DEPRESSED	5.0	5.6

TABLE 22

Mean Statement of Certainty Scores

Happy, Neutral and Depressed Observers

Experiment 2

		PROBLEM	
		25-25	75-75
	HAPPY	7.0	6.8
DISPOSITION	NEUTRAL	6.4	6.4
	DEPRESSED	6.4	6.8

Supplementary Analyses

As in the first study, it was of interest to determine the nature of heuristics used by subjects in calculating total control. To achieve this, JTC scores were correlated with a number of heuristics as defined in experiment 1. Coefficients were calculated separately for each disposition. Depressive's JTC scores were found to correlate with number of confirming cases to a significant degree ($r(20)=.60$, $p<.01$), but not with any of the other measures. Neutral's scores correlated to a significant degree only with number of successes ($r(20)=.39$, $p<.05$); while those of the happy group correlated with none of these measures. Interestingly, when the correlations were calculated separately for each level of activity, it was found that participants judgements alone accounted for the correlations obtained. The judgements of the observers appear unrelated to any of the tested measures.

Post-Experimental Questionnaire

As in the first study it was of interest to examine the relationship of verbal judgements of control to their numerical translations. To assess this, questionnaires were selected with clearly stated high or low control in response to the open-ended question. JTC values on these questionnaires were then subjected to a 2(disposition) X 2(problem type) X 2(labelled control - high or low) analysis of variance.[3] No significant main effects or interactions were found, indicating, as did the results of the first experiment, that verbal representations do not neccesarily mirror numerical estimates of the

same data. Individuals with statistically undifferentiated JTC values may verbally quantify these values in very different ways.

DISCUSSION

Inclusion of an observer during the completion of the contingency judgement task introduced sufficient variability in the data as to almost completely obscure the disposition and problem effects found in previous research. Of the four original hypotheses, the first was only partially confirmed, the others not at all. Overall, as predicted, the depressives were accurate in judgements of total control, whereas the neutral and happy subjects demonstrated an illusion of control. However, there were no differences in effect dependent upon level of reinforcement, contrary to consistent findings of the past. Hypothesis 2, which predicted that the character of judgements would be reversed in the observer condition was not supported. Rather, the judgements by observers on all scales were characterized by extreme variability, and thus appeared unrelated to any systematic influences. Hypothesis 3, which suggested that depressed observers would be more pessimistic in judging perceived future efficacy was not confirmed, although mean scores reveal a trend in that direction. (ESTP means: Depressed= -1.0%; Neutral=+14.0%; Happy= +16%). Finally, the fourth hypothesis, which predicted that the inaccurate non-depressives alone would rely on invalid heuristics in forming their judgements was not

substantiated. Participants in all three dispositional groups relied on invalid heuristics, whereas no systematic use of the common informational sources could be found for observers.

Evidence from the first experiment indicates discrepancy between verbal and numerical reports of control, and this problem was also encountered in the second study. This discrepancy occurs only within the happy and neutral groups, indicating, as before, that inaccuracy in judging non-contingency may be a result of difficulty in attaching a mathematical label to what is essentially an accurate verbal description of contingency , rather than an inability to recognize non-contingency.

GENERAL DISCUSSION

Although the results of the present set of studies, particularly those of Experiment 1, constitute a replication of the finding of Alloy and Abramson (1979), that depressives are relatively accurate judges of non-contingency, some important supplementary issues concerning the nature and meaning of such judgements were brought to light in the present research.

Firstly, contrary to prediction, the magnitude of illusory control appears not to be a clear function of dispositional status. Rather, depressives form a signal group of accurate assessors whose judged total control scores are uniquely conservative, rarely overlapping with the inflated values reported by the non-depressed. Although the mean scores of the neutral and happy groups appeared to indicate a difference between these dispositions, the difference was not significant. This may be partially attributable to the fact that plain logic on the part of the non-depressives might prevent exorbitantly high JTC reports. Few, for example, could logically defend a 90% control report on a non-contingent task. It should be kept in mind however that the experimental situation adopted for use was one in which responses and outcomes were relatively simple, and clearly identifiable. Real-life contingencies are likely to be less explicit, thus perhaps allowing for the lifting of this judgemental ceiling. It is therefore conceivable that a different method might permit the neutral/happy difference to reach significance.

In any case, the results of the first study do confirm the proposal that depression is associated with accurate numerical judgement of non-contingent response-outcome relationships. In this respect, relative to a great many reports in the literature of inaccuracy in normals, depressives seem to constitute a unique population. If this difference in judgemental accuracy is demonstrated to be a reasonable means of discriminating depressives from non-depressives, it could a fruitful avenue for therapeutic focus. As the data from the SPAQ and SPAQ-O scales demonstrate, both the depressed and the non-depressed have available the necessary information to calculate correct representations of contingency. Judgemental differences between the groups therefore appear to lie in the ways in which they organize and interpret these data. According to some authors (e.g. Alloy and Abramson, 1979; Greenwald, 1980; Langer, 1975), non-depressives succumb to a variety of cognitive illusions as a result of a generalized expectancy of control. The success of anti-depressant therapy then may rest upon the development of such positive expectations of control in the depressive. The implications of this point are intriguing: to effect the development of optimistic expectations may in fact be to teach the depressive to be unrealistic. Bibring (1957) for example, has proposed that the depressive is characterized by a breakdown of the normal mechanisms of self-deception. Interestingly however, Lewinsohn et al (1980) demonstrated continuity of typical self-descriptive accuracy in depressives even after treatment judged to be effective in reducing depression was completed. Obviously, further research into longitudinal changes in contingency judgements associated with recovery from

depression is necessary.

Two issues cropped up during analysis of the present experiments which have serious implications for the understanding of the role of contingency judgements outside the laboratory. These include the nature of self/other differences in judgements, and the mode of their internal representation.

Judgements of Contingency in a Social Context

The effects of including an inactive observer in the second experiment are difficult to account for. While previous research (Lobitz & Deepost, 1979) demonstrated self-other differences as a function of disposition, inclusion of the observer in the present study introduced a sufficient degree of variability in the judgements of both participants and observers to suggest that the formation of such judgements outside the laboratory may have to be investigated further. JTC scores for both the neutral and happy participants were far more conservative in the second study than in both the first study and that of Alloy and Abramson (1979, Experiment 2). Thus, in the presence of evaluative others, illusory control reports are not as robust. This cautious adjustment of control values seems to imply a questioning awareness about the accuracy of their own probability estimates by the participants. Results in a similar vein are reported by Alloy and Abramson (1979, Experiments 3 & 4). Here adjustments of contingency estimates are found when high control is associated with failure. The judgements in these experiments therefore appear to be influenced by biases in information selection and interpretation, dependent upon characteristics of the experimental

situation.

Thus, in situations outside the laboratory, where the subject will often be exposed to evaluation by others, as well as put in circumstances which are weighty in terms of outcome valence, contingency judgements may take on a character quite different from that seen in the laboratory.

The variability in judgements of control by observers may have a separate origin. A substantial literature in the domain of social psychology is devoted to examining the differences in perception of behavior by self and others (e.g. Monson & Snyder, 1977; Newman, 1978). In particular, the work of Jones and Nisbett (1971) has called to attention the differences in information available to either party in self/other perception studies. Of particular importance in the present research is the realization that the participant has accessible the opportunity to actively test any hypotheses she might entertain concerning the control of the light, whereas the observer can only guess as to the strategies used by the participant. Given the high number of subjects in both participation levels who reported using complex hypotheses in estimating control, and the fact that observers hypotheses go essentially untested, it is predictable that their estimates should be less reliable than those of the participants.

This first issue of concern considers the possibility that judgements characteristic of individual participant in the laboratory setting may be somewhat unrepresentative models of real-life behavior. In socially evaluative situations, the illusory control reported by non-depressives seems to be cautiously reduced, therefore claims to both

the robustness and relevance of such measures may be challenged. The most serious threat to the meaningfulness of these measures however comes from an entirely unexpected finding.

Verbal-Numerical Report Discrepancies

As presented in the introduction, the logic behind the investigation of contingency judgements as a function of disposition lies in the ways in which these judgements are felt to determine the nature and direction of behavior. That objective contingencies somehow direct behavior has been amply demonstrated (see Alloy and Abramson, 1979, for a review), and is therefore not subject to dispute here. The object of the present analysis was primarily to ascertain the degree to which objective contingencies are reflected by subjective representations. Past research has clearly demonstrated that contingent relations are well represented by most populations (Alloy and Abramson, 1979; Ward & Jenkins, 1965; Peterson, 1980). When subjective representations of non-contingency are examined however, results are far less clear-cut. With the exception of the depressed group of Alloy and Abramson, few, if any populations are felt to accurately judge non-contingency.

Most normals have been shown to have a consistent tendency to overestimate control. While the depressed/non-depressed differences are indeed interesting and revealing from a theoretical perspective, the discrepancies between verbal and numerical translations of control found in this research may force a re-analysis of the role of these judgements in actually controlling behavior. When considering the accuracy of

response-outcome representations it seems equally crucial to determine their mode of representation. As explained above, acumen in a structured laboratory task need not necessarily imply day-to-day utilization of such ability.

Evidence exists in the literature that subjective contingencies may not be represented internally in a numerical mode. Rather, operative hypotheses directing behavior are said to be cognitively represented verbally (Karpf & Levine, 1971), and deterministically (Brehmer, 1974). With particular relevance to the issue under discussion, Shapira (1975) has shown that alternative measures of subjective probability do not always coincide. He found, as in the present case, that verbal and numerical estimates may correlate poorly. Which forms of representation our non-depressives would be guided by in a real-world task is an important question for future research.

In any case, consideration of this issue is important, for it brings to bear the relevance of findings dealings exclusively with numerical measures. It seems at least intuitive that individuals do not typically interact with the environment by consciously construing it in probabilistic terms. The findings of Alloy and Abramson (1979), and the numerical results of the present research stand in the face of a substantial literature which suggests humans in general to be poor judges of contingency. For reasons elegantly laid out in Hogarth's (1975) review, verbal, holistic labels are more likely to be used in portraying the environment than are numerical representations of contingency. He suggests that an accurate probabilistic representation would necessitate the availability to the subject a range of statistical

abilities which are not commonly demonstrated in experimental analysis.

Parenthetically, it should be kept in mind that most previous studies demonstrating the illusion of control in non-contingent situations have based their findings on relatively indirect measures of contingency. (e.g. lottery gaming behavior, Langer, 1975). It may be unwise to generalize so far as to assume that gambling behavior realistically represents the typical behaviors of a subject.

Attention to the numerical estimates of contingency alone could potentially bias our conceptualization of the relation of contingency judgements to disposition. When verbal translations are analysed alongside the numerical labels, the dispositional difference is drastically reduced. It is up to future research to determine whether numerical differences in contingency estimates are a true source of behavioral differences between depressed and non-depressed subjects.

SUMMARY AND CONCLUDING REMARKS

This research has attempted to clarify several issues in the literature on contingency judgements as a function of disposition. The essence of many cognitive approaches to a variety of psychological phenomena hold that operative environmental contingencies are represented internally by an organism, and that this representation determines behavior. Various influential approaches to depression, particularly that proposed by Seligman (1975), suggest that depressives are characterized by some biases or distortions in their internal representation, so that the external environment is not adequately reflected. In contrast, some research, including the present work, finds no such evidence of depressive distortion. These findings indicate that both depressives and non-depressives accurately judge contingent relationships, whereas non-depressives characteristically overestimate control when they objectively have none. Two primary questions were addressed in the present work. First, it was of interest to determine whether "happy" individuals would show even greater illusions of control than did the neutrals of previous studies, the logic being that happy subjects would be characterized by significantly stronger self-serving biases than would the depressives and neutrals. Although trends in the data indicated slightly higher control judgements by the happy, the difference did not prove significant.

The second point of interest dealt with the potential relativism of judged control values. It was suggested that judgements of one's

control are not made in an evaluative vacuum. Rather, the outcome of any sequence of behavior is likely to be appraised with respect to expectancies, performance of relevant others, and several other comparative markers. Predictions were not upheld. When subjects judged control in actor/observer pairs, judgements were substantially different in character than when subjects participated alone. The illusory control of the non-depressive participants was considerably less robust when they were being observed than found in previous research. Scores obtained from observers seemed to have a disputable origin. Whereas in the first study non-depressives were shown to use identifiable, albeit invalid, informational bases for their judgements, estimates by observers in the second study correlated with no recognizable sources.

The replication of earlier work (e.g. Alloy and Abramson, 1979) documenting depressive's judgemental accuracy has important implications. Firstly, if depressives can be readily distinguished from non-depressives on this basis, therapy might be wisely directed at building expectations of success. Several therapies are, of course, so inclined. Before assessing the importance of this characteristic in distinguishing groups however, we are forced to consider the evidence found in the present research suggesting that numerical estimates of contingency may not be the only information guiding behavior. On the basis of verbal translations of contingency, many non-depressives were found to be as accurate judges as were the depressed.

Given that much of this present work was carried out under the broad auspices of the learned helplessness model of depression, it seems fitting that final comments include reference to the model. Much of the

work done by Alloy and Abramson, confirmed in part by the present research, suggests that the associative deficit proposed by Seligman may not in fact exist. Had the present work found that depressives overestimate the performance of others relative to their own, some argument might have been made that depressives would then be suffering from a contextually bound, or relative deficit. Even if accurate about their own performance, overestimation of the abilities of others could serve to lower the perceived self-efficacy of the depressed (Bandura, 1977). No such support for the model could be found however.

Finally, a point of caution. As mentioned early on in this paper, some authors have expressed concern over the tendency to infer complex cognitive dysfunctions as the source of depression. In particular, Coyne and Lazarus (Note 1), have argued that many of the current studies providing data on depressed/non-depressed differences in cognition rely on laboratory methodologies which may be of questionable relevance to real-life situations. As the present research has demonstrated, when elements of greater realism (e.g. social evaluation) are added to the laboratory methods, the character of data produced is distinctly altered. Thus, until we can find means of assessing cognitive differences in more appropriately natural environments, it would be wise to use caution in interpreting the available data.

FOOTNOTES

(1) : The term 'control' rather than contingency was used in past experiments, as well as in the present, as it was felt to convey the notion of contingency in a way more readily understood by subjects.

(2) : Given the rather large differerence in elapsed time between CCDSI and CCDSII administration between subjects from the longitudinal study and those culled from the psychology classes, an analysis of variance was carried out to determine if significant judged total control or mood scale score differences between these groups existed. No such difference could be found.

(3) : As no depressives reported that they had high degrees of control verbally on the Post-Experimental Questionnaire, only the responses of the Happy and Neutral groups were included in the analysis.

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APPENDIX 1

Subjective Probability Assessment Questionnaire

Page 1 S.P.A.Q.

Name _____

Group _____

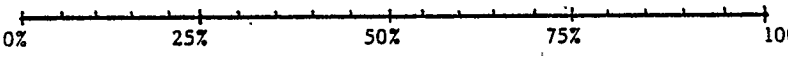
Test # _____

Date _____

The following questions apply to the test session you have just completed. Please be as precise as possible in your responses.

1) Judgement of Total Control

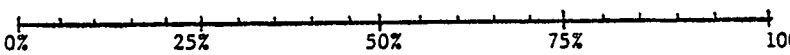
Please indicate how much control you believe you had over the onset of the red light (either by pressing or not pressing) by placing an "X" somewhere on this scale:



I had no control — I had intermediate control — I had complete control

2) Judgement of Total Reinforcement

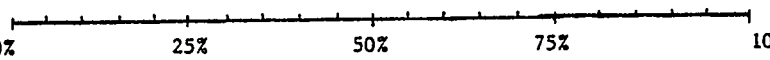
Please indicate by placing an "X" somewhere on this scale on what percentage of all trials the red light came on:



The red light never came on — The red light always came on

3) Judgement of Reinforcement When Pressed

Please indicate by placing an "X" somewhere on this scale what percentage of the time the red light came on when you pressed the button:



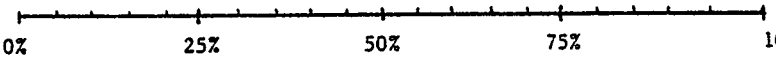
0% 25% 50% 75% 100%

The red light never came on if I pressed

The red light always came on if I pressed

4) Judgement of Reinforcement When Not Pressed

Please in dicate by placing an "X" somewhere on this scale what percentage of the time the red light came on if you did not press the button:



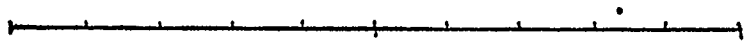
0% 25% 50% 75% 100%

The red light never came on if I did not press

The red light always came on if I did not press

5) Statement of Certainty Scale

Please indicate by placing an "X" somewhere on this scale how certain you are of the judgements you have just made:



0 10

I am completely uncertain

I am completely certain

6) Strategies and Information

We are interested in the way you completed the test. Please describe briefly any evidence which convinced you that you did or did not have control over the onset of the red light. As well, please try to describe what information would have convinced you otherwise:

_____

_____

_____

_____

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_____

_____

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APPENDIX 2

PARTICIPANT INSTRUCTIONAL SET

In this problem-solving experiment it is your task to learn what degree of control you have over whether or not this red light on the panel in front of you comes on. Each time the yellow light comes on indicates the start of a new trial, the occasion to do something. For each trial, after the yellow light comes on, you have the option of either making a button-press response, or not making a button press response. A button press response consists of pressing this white button once and only once immediately after the yellow light comes on. Not making a button press response consists, of course, of doing nothing when the yellow light comes on. If you do intend to press the button on a given trial, you must press within five seconds after the yellow light comes on, otherwise the trial will be counted as a not press trial. So, in this experiment there are only two possibilities as to what you can do on each of the trials; either press the button within five seconds after the yellow light goes on, or just sit back and do nothing.

You may find that the red light will go on on some percentage of trials on which you do make a button press response. You may also find that the red light will go on on some percentage of trials on which you do not make a button press response. Alternatively, you may find that the red light will not go on on some percentage of the trials on which you make a button press response. And, you may find that the red light will not go on on some percentage of trials on which you do not make a

button press response. So, there are four possibilities as to what may happen on any given trial:1)you press and the red light does come on; 2)you press and the red light does not come on;3)you don't press and the red light does come on;and 4)you don't press and the red light does not come on. Since it is to your task to learn how much control you have over whether the red light does come on, as well as whether the red light does not come on, it is to your advantage to press on some trials, and not on others, so that you know what happens when you don't press, as well as when you do.

Forty trials will constitute the problem. After the problem you will be asked to indicate your judgement of control by placing an "X" somewhere on the first scale in front of you. At 100 if you have complete control over the onset of the red light, at 0 if you have no control over the onset of the red light, and somewhere between these extremes if you have some but not complete control over the onset of the red light. Complete control means that the onset of the red light on any given trial is determined by your choice of responses, either pressing or not pressing. In other words, whether or not the red light goes on is totally determined by whether you choose to press, or just sit back and not press. No control means that you have found no way to make response choices so as to influence the onset of the red light. In other words, the onset of the red light has nothing to do with what you do, or don't do. Another way to look at having no control is that whether of not the red light comes on on any given trial is totally determined by factors such as chance or luck, rather than by your choice of pressing or not pressing. Intermediate degrees of control mean that your choice of

responses, either pressing or not pressing, influences the onset of the red light even though it does not completely determine whether the red light goes on or not. In other words what you do or don't do matters to some extent but not totally. Another way to lookat having intermediate control is that one response, either pressing or not pressing, produces the red light onset more often than does the other response. So, it may turn out that you have no control, that is, your responses will not affect the onset of the red light, or it may turn out that you have some degree of control, either complete or intermediate, where one response produces red light onset more often than does the other response. Any questions before we begin?

APPENDIX 3

OBSERVER INSTRUCTIONAL SET

In this experiment, we are interested in finding out how well you are able to judge how much control other people have on a particular task. In the experiment itself we will ask you to watch a person attempting the task, but before you do, we would like to acquaint you with the nature of the task so that you have a better idea of what is going on. The girl you see has been told that she is going to try and gain control over the red light on the panel in front of her. So that yoy understand her task, we will read you the instructions she heard.

In this problem solving experiment, it is her task to learn what degree of control she has over whether the red light comes on or not. Each time the yellow light comes on indicates the start of a new trial, the occasion to do something. For each trial, after the yellow light comes on, she has the option of either making a button press response, or not making a button press response. A button press response consists of pressing the white button in front of her once and only once immediately after the yellow light comes on. Not making a button press response would consist, of course, of her doing nothing when the yellow light comes on. If she does intend to press the button on a given trial, she must press within 5 seconds of the onset of the yellow light, otherwise the trial will be counted as a not press trial. So, in this experiment, there are only two possibilities as to what she may do on any of the given trials - either press the button within 5 seconds after

the yellow light goes on, or just sit back and do nothing.

You may find that the red light will go on some percentage of trials on which she does make a button pres response. You may also find that the red light will go on on percentage of trials on which she does not make a button press response. Alternatively, you may find that the red light will not go on on some trials on which she does not make a button press response. And you may find that the red light does not go on on some trials on which she does not make a button press response. So, there are four posibilities as to what may happen on any given trial: 1)she presses and the red light does come on; 2) she presses and the red light does not come on; 3) she does not press and the red light does come on; and 4) she doesn't press and the red light does not come on. Since it is her job to learn how much control she has over whether the red light does come on, as well as whether the red light does not come on, it is to her advantage to press on some trials, and not on others, so that she knows what happens when she does press, as well as when she doesn't.

Forty trials will constitute the problem. After the problem, you will be asked to indicate your judgement of how much control you think she managed by placing an 'X' somewhere on the first scale in front of you. At 100 if she had complete control over the onset of the red light, at 0 if she had no control over the onset of the red light, and somewhere between these extremes if she had some but not complete control over the onset of the red light. Complete control means that the onset of the red light is completely determined by her choice of response, either pressing or not pressing. In other words, whether or

not the red light goes on is totally determined by whether she chooses to press, or just sit back and do nothing. No control means that she has found no way to influence the onset of the red light. In other words, the onset of the red light has nothing to do with what she does, or doesn't do. Another to way to look at no control is that whether the red light comes on on any given trial is totally determined by chance or luck, rather than by her choice of pressing or not pressing. Intermediate degrees of control mean that her choice of response, either pressing or not pressing, influences the onset of the red light even though it does not determine whether the red light goes on or not. In other words, what she does, or doesn't do matters to some extent but not totally. Another way to look at having intermediate control is that one response, either pressing or not pressing, produces the red light onset more often than does the other response. So it may turn out that she has no control, that is her responses will not affect the onset of the red light, or it may turn out that she has some degree of control, either complete or intermediate, where one response produces the red light onset more often than does the other response.

In order that you don't influence each other during the course of the experiment, we ask that you not talk. If you have any questions, please ask now.

APPENDIX 4

Newman-Keuls Test on Judged Total Control Scores - Experiment 1 (75-75 Problem)

	DEPRESSED	NEUTRAL	HAPPY
DEPRESSED		39.0*	55.0**
NEUTRAL			16.0 n.s.
HAPPY			

* $p<.05$

** $p<.01$

APPENDIX 5

Newman-Keuls Test on Judged Total Control Scores - Experiment 2 (75-75 Problem)

	DEPRESSED	NEUTRAL	HAPPY
DEPRESSED		17.7*	21.8**
NEUTRAL			4.1 n.s.
HAPPY			

* $p<.05$

** $p<.01$

APPENDIX 6

Summary of Analysis of Variance of

Judged Total Control Scores - Experiment 1

SOURCE	SS	df	MS	F	prob.
DISPOSITION	5384.6	2	2692.3	7.75	$p<.01$
PROBLEM	7905.6	1	7905.6	22.75	$p<.01$
DISP. X PROB.	2829.2	2	1414.6	4.07	$p<.05$
ERROR	8337.2	24	347.383		

APPENDIX 7

Summary of Analysis of Variance of

Judged Total Reinforcement Discrepancy Scores - Experiment 1

SOURCE	SS	df	MS	F	prob.
DISPOSITION	3.26	2	1.63	0.02	n.s.
PROBLEM	61.63	1	61.63	0.9	n.s.
DISP. X PROB.	89.26	2	44.63	0.65	n.s.
ERROR	1629.2	24	67.88		

APPENDIX 8

Summary of Analysis of Variance of

Judged Reinforcement When Press Discrepancy Scores - Experiment 1

SOURCE	SS	df	MS	F	prob.
DISPOSITION	2.46	2	1.23	1.33	n.s.
PROBLEM	5.63	1	5.63	6.11	$p<.05$
DISP. X PROB.	4.26	2	2.13	2.31	n.s.
ERROR	22.1	24	0.92		

APPENDIX 9

Summary of Analysis of Variance of

Judged Reinforcement When Not Press Discrepancy Scores - Experiment 1

SOURCE	SS	df	MS	F	prob.
DISPOSITION	351.6	2	175.83	0.88	n.s.
PROBLEM	1763.3	1	1763.3	8.87	$p<.01$
DISP. X PROB.	851.67	2	425.83	2.1426	n.s.
ERROR	4770.0	24	198.75		

APPENDIX 10

Summary of Analysis of Variance of

Statement of Certainty Scores - Experiment 1

SOURCE	SS	df	MS	F	prob.
DISPOSITION	4.20	2	2.1	0.36	n.s.
PROBLEM	5.63	1	5.63	0.97	n.s.
DISP. X PROB.	13.26	2	6.63	1.15	n.s.
ERROR	138.4	24	5.76		

APPENDIX 11

Summary of Analysis of Variance of

Judged Total Control Scores - Experiment 2

SOURCE	SS	df	MS	F	prob.
DISPOSITION (D)	5535.3	2	2767.6	4.28	$p<.05$
PROBLEM (P)	851.2	1	851.2	1.31	n.s.
ACTIVITY LEVEL (A)	41.66	1	41.66	0.06	n.s.
D X P	64.43	2	32.21	0.04	n.s.
D X A	1610.23	2	805.1	1.24	n.s.
P X A	1622.4	1	1622.4	2.51	n.s.
D X P X A	1590.3	2	795.15	1.23	n.s.
ERROR	30998.8	48	645.8		

APPENDIX 12

Summary of Analysis of Variance of

Judged Total Reinforcement Discrepancy Scores - Experiment 2

SOURCE	SS	df	MS	F	prob.
DISPOSITION (D)	364.30	2	182.15	2.84	n.s.
PROBLEM (P)	156.81	1	156.81	2.45	n.s.
ACTIVITY LEVEL (A)	98.81	1	98.81	1.54	n.s.
D X P	29.63	2	14.81	0.23	n.s
D X A	442.63	2	221.31	3.45	$p<.05$
P X A	22.81	1	22.81	0.35	n.s.
D X P X A	580.63	2	290.31	4.53	$p<.05$
ERROR	3071.20	48	63.98		

APPENDIX 13

Summary of Analysis of Variance of

Judged Reinforcement When Press Discrepancy Scores - Experiment 2

SOURCE	SS	df	MS	F	prob.
DISPOSITION (D)	160.83	2	80.41	0.25	n.s.
PROBLEM (P)	81.66	1	81.66	0.25	n.s.
ACTIVITY LEVEL (A)	15.0	1	15.00	0.04	n.s.
D X P	80.83	2	40.41	0.12	n.s.
D X A	122.5	2	61.25	0.19	n.s.
P X A	426.66	1	426.66	1.35	n.s.
D X P X A	705.83	2	352.91	1.12	n.s.
ERROR	15090.0	48	314.375		

APPENDIX 14

Summary of Analysis of Variance of

Judged Reinforcement When Not Press Discrepancy Scores - Experiment 2

SOURCE	SS	df	MS	F	prob.
DISPOSITION (D)	608.13	2	304.06	1.00	n.s.
PROBLEM (P)	3681.66	1	3681.66	12.20	$p<.01$
ACTIVITY LEVEL (A)	106.66	1	106.66	0.35	n.s.
D X P	1955.73	2	977.86	3.24	$p<.05$
D X A	707.73	2	353.86	1.17	n.s.
P X A	117.60	1	117.60	0.38	n.s.
D X P X A	1106.80	2	553.40	1.83	n.s.
ERROR	14484.40	48	301.75		

APPENDIX 15

Summary of Analysis of Variance of

Statement of Certainty Scores - Experiment 2

SOURCE	SS	df	MS	F	prob.
DISPOSITION (D)	12.70	2	6.35	1.29	n.s.
PROBLEM (P)	0.81	1	0.81	0.16	n.s.
ACTIVITY LEVEL (A)	8.81	1	8.81	1.80	n.s.
D X P	5.43	2	2.71	0.55	n.s.
D X A	4.63	2	2.31	0.47	n.s.
P X A	0.416	1	0.416	0.08	n.s.
D X P X A	3.63	2	1.81	0.37	n.s.
ERROR	234.80	48	4.89		