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The Cross-Situational Generalisation  
of Learned Helplessness

A thesis presented in partial fulfilment of  
the requirements for the degree of  
Master of Arts in Psychology  
at Massey University

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1982

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## Abstract

This study aimed to investigate the cross-situational generalisation of learned helplessness. The experiment was conducted in two distinct phases, with 90 first-year psychology students volunteering as subjects. In phase one, subjects were exposed to one of three pretreatment conditions; contingent reinforcement, noncontingent reinforcement and control. In the second phase the performance of these three groups was compared on a test task involving social interaction. Affective changes were noted (on the Multiple Affect Adjective Check List) following the first phase for helpless subjects. Helplessness failed to generalise across situations and no performance deficits were evident on the test task. The results did not support predictions of wide generality. The failure to demonstrate cross-situational generalisation was discussed in terms of the implications for learned helplessness theory.

## Acknowledgements

My thanks to Beryl Hesketh and John Podd for their good counsel and continual support.

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## Introduction

### Overview

Learned helplessness may be regarded as an expectation of noncontingency learned following exposure to outcome-response independence. Following a review of the helplessness literature this chapter focusses on a theoretical weakness. This weakness is concerned with the extent to which helplessness effects generalise beyond the situation in which they originated. Generalisation is at the heart of the significance of learned helplessness. Demonstrations of learned helplessness show that subjects inappropriately generalise a belief in noncontingency to situations where reinforcements are contingent. The significance of the helplessness paradigm is linked to the degree of generalisation that occurs. The current research problem attempted to assess the degree of generalisation across dissimilar situations using dissimilar tasks.

### Definitions and Infrahuman Findings

The term 'learned helplessness' was initially coined after a series of experiments involving traumatic avoidance learning in dogs (Overmier & Seligman, 1967; Seligman & Maier, 1967). Dogs given inescapable shock in a hammock subsequently failed to escape shock when placed in a shuttle box. In contrast, dogs given escapable shock or no prior shock, escaped well. These studies concluded that exposure to uncontrollable and inescapable electric shock subsequently interfered with the acquisition of escape-avoidance learning. The term 'learned helplessness' is used variously to refer to this interference or to the process underlying it. Maier and Seligman (1967) regard this as a process of learning that outcomes are independent of responses. Learned helplessness also refers to the interference which occurs in subsequent instrumental learning. This interference was readily observable in the early studies using dogs. The dogs simply failed to learn the appropriate escape response.

The early findings with dogs have subsequently been extended to a variety of other species. The cross-species generalisation of learned helplessness has been demonstrated in rats, cats, fish, dogs, monkeys and humans (Seligman, 1975). In human helplessness studies subjects either fail to escape aversive stimulation which they have been misinformed is escapable or they fail to solve problems which they have been misinformed are solvable. While

deception is a key feature of human helplessness inductions, this was not the case for the animal studies upon which human studies have been closely modelled.

### Human Helplessness

Seligman (1978) referred to the 'learned helplessness enterprise'. The term 'enterprise' is used because as Wortman and Dintzer (1978) noted, during the last decade the learned helplessness model has had a major impact on theoretical development in both social and clinical psychology. A substantial body of research has been published in this area. An entire issue of the Journal of Abnormal Psychology (February, 1978) was devoted to current research and theoretical considerations associated with learned helplessness. Other authors (e.g. Nation & Massad, 1978), have remarked on the burgeoning literature and growing interest in the area. The major impetus for the growth of interest was the application of the basic model to humans and the problems of human suffering (depression).

The following review considers the experimental paradigm used in many human helplessness studies, the deficits which have been found in such studies, the variable outcomes which may result from an experimental helplessness induction, and the meanings and interpretations placed on such results. In considering factors which impinge upon the meaning of learned helplessness both method and theory are reviewed.

### The Basic Paradigm

Hiroto (1974) provided an example of the basic experimental paradigm. His escape group received loud noise which they learned to terminate by pushing a button. The 'helplessness' group received the same noise, independently of any response they made. The control group received no noise. Each subject was then taken to a finger shuttle box (directly paralleling the animal paradigm). In order to escape noise each subject was required to move his/her hand from one side to the other. Both escape and control groups readily learned to shuttle with their hands. The 'helpless' group failed to avoid the aversive noise.

Hiroto's study is representative of the many human helplessness studies which followed it. This basic experimental paradigm was further elucidated by Miller and Norman (1979). They described the typical study as being conducted in two phases, a training phase followed by a test phase. Initially subjects were exposed to a training task in which they received (a) contingent (response-dependent) reinforcement; (b) noncontingent (response-independent) reinforcement, or (c) no treatment (control). Following the training phase, the performance of the three groups was compared on a test task, for which reinforcement was again contingent for all groups.

Following numerous replications and extensions of Hiroto's (1974) finding (e.g. Roth & Bootzin, 1974; Hiroto & Seligman, 1975; Roth & Kubal, 1975), it became apparent

that a variety of training tasks were effective in inducing helplessness. Specifically inescapable shock, inescapable noise and insoluble cognitive problems had all been employed in successful inductions.

### Motivation, Cognition and Emotion

'Successful' inductions are characterised by motivational, cognitive and emotional disturbances.

Motivational deficits are related to Seligman's (1975) assertion that, "the incentive to initiate voluntary responses to control any outcome comes from the expectation that responding will produce that outcome" (p. 49). Accordingly when a person has learned that outcomes are independent of responding, the expectation that responding produces outcomes wanes, and response initiation diminishes. The motivational deficit then stems from the belief in uncontrollability which undermines the incentive to initiate responses. An example of this deficit comes from a study by Thornton and Jacobs (1971). Their study involved human subjects receiving inescapable shock, during which subjects sat passively and took the shock. When later asked why they failed to respond 60% of the subjects reported that they had no control over the shock.

The cognitive deficit involves learning that responses and outcomes are independent. Learning response-outcome independence interferes with subsequent learning regarding the efficacy of responding. Hiroto and Seligman (1974) and Miller and Seligman (1975) provided results where subjects

came to believe that success and failure were independent of their own actions.

A number of emotional consequences are also claimed to arise from learning that outcomes are uncontrollable. Seligman (1975) reasoned that fear (heightened arousal) follows a traumatic event. When the subject learns that he cannot control the trauma the fear is reduced and is replaced by depression.

The results of extensive research have not mapped exactly the three deficits predicted by Seligman. Miller and Norman (1979) noted that the evidence fails to support the distinction between cognitive and motivational deficits. They argued for a more general performance deficit and suggested this may result from impairment of both processes (motivation and cognition).

Miller and Norman (1979) referred to increases in depression, anxiety and hostility as examples of emotional deficits. Increased hostility is neither predicted, nor explained by Seligman's theory. They nevertheless concluded that the literature generally supports predictions of emotional outcomes of learned helplessness.

#### Learned Helplessness Outcomes

Studies examining learned helplessness may be classified according to their results - performance deficits, facilitated performance, or non significant results. This classification is used in the following discussion of the

various types of results which may be obtained.

The predicted performance deficit following a helplessness induction has received extensive confirmation. Miller and Norman (1979) reviewed 23 studies which have attempted to demonstrate the occurrence of changes in performance due to the training phase in which the subjects' responses were independent of environmental outcomes. They concluded that a majority (20) of these studies reported performance deficits in the test phase.

Facilitation refers to an improvement or increase in performance as a consequence of a helplessness induction. Several studies (Thornton & Jacobs, 1971; Roth & Bootzin, 1974; Roth & Kubal, 1975; Miller & Seligman, 1976; Wortman, Panciera, Shusterman & Hibcher, 1976; Hanusa & Schulz, 1977), have reported increases in performance following a learned helplessness training phase. Such results are intriguing since they are the opposite of that predicted by the original learned helplessness theory. Facilitation effects are not well understood. Explanations vary, but an intuitively appealing view was forwarded by Roth and Kubal (1975). They maintained that an initial reaction to no-control was to behave assertively in an attempt to subsequently re-establish and exercise control. This explanation has gained only limited support. Williams and Teasdale (1982) noted that this explanation has not led to increased predictability of when the typical helplessness pretreatment will lead to facilitated rather than impaired performance.

The final possibility is that results would be

non-significant. Such results are interpreted to mean that the helplessness induction has been unsuccessful. Hanusa and Shulz (1977) referred to other studies which failed to find helplessness, (Roth & Bootzin, 1974; Wortman et al., 1976; Jones, Nation & Massad, 1977). Kilpatrick-Tabak and Roth (1978) cited four, (not surprisingly) unpublished studies which report marginal significance at best. Sacco and Hokanson (1978) also reported nonsignificant results, and cited a further unpublished study as also failing to replicate the basic helplessness hypotheses. Breen, Volcano and Dyck (1979) reported a similar result, where the performance of one of their control groups was statistically indistinguishable from that of the helplessness groups.

Clearly there exists a lack of concensus regarding the outcomes obtained by studies of human helplessness. This has had a disquieting influence on the way in which a number of researchers have viewed the helplessness literature. Miller and Norman (1979) pointed out that the absolute magnitude of deficits obtained has been relatively small. Although statistically significant (in most cases) the small absolute differences raise questions about the importance of the results. Together with the non-significant results and the facilitated performance found by some studies, these results suggest that learned helplessness is not always readily induced.

Peterson (1978) argued similarly that the typical demonstration with human subjects is not as dramatic as the typical demonstration with animal subjects. He also asserted that only very limited cross-situational generalisation has

been shown. Since 'helpless' subjects have not, for example, subsequently failed their psychology course or been unable to find their way from the experimental room, the helplessness produced in the laboratory has remained situation specific. The effects of the helplessness procedure may well be a function of the artificial conditions to which subjects are exposed for helplessness studies. This theme will be returned to later.

In summary, helplessness studies find either facilitation, debilitation, or non-significant results. Where debilitation is recorded, this is usually in terms of emotional and performance deficits.

#### Methodological Issues

A number of methodological problems exist in the learned helplessness literature. One such issue relates to the design of experiments which attempt to induce helplessness. Lazarus, Deese and Osler (1952) originally expressed doubts about the 'failure' design. Experimental manipulations employed in helplessness inductions are usually referred to as uncontrollable events (Abramson, Seligman & Teasdale 1978). Some researchers (Buchwald, Coyne & Cole 1978; Coyne, Metalsky & Lavelle 1980) regarded these manipulations as experimenter induced failure. A number of questions arise from such an alternative description. Lazarus et al. (1952), expressed scepticism regarding the motivation of the subjects, the realism of the manipulations, and the impact of perceived deception in failure experiments. They emphasised the lack of clarity concerning the exact nature of what individual

subjects respond to. Do they respond to the conditions intended by the experimental manipulation, to an awareness of having been deceived, to dissonance between their expectations and apparent performance, to demand characteristics, or to other factors? The design is ambiguous and leaves results open to various interpretations.

Coyne, Metalsky and Lavelle (1980) were aware of the impact of demand characteristics. They maintained that mildly distressed subjects are especially susceptible to cues regarding how they are subsequently expected to perform (demand characteristics). Furthermore they noted that anxiety researchers have been dissuaded from relying too heavily on the results of laboratory based studies. Some helplessness researchers (Klein & Seligman, 1976) have denied the role that demand characteristics may play.

Wortman and Brehm (1975) criticised the imprecise nature of the experimental manipulations which give rise to learned helplessness. Costello (1975) similarly suggested that helplessness research is dogged by a lack of specificity in the experimental paradigms. What is apparent is that it is unclear what produces helplessness in these experiments. Accordingly the state of knowledge regarding helplessness has progressed little.

Another methodological question concerns the dependent measures used in helplessness inductions. Many studies have used anagrams for the test task and have derived dependent measures from this task. One such measure is latency (the time to reach a correct response). Miller

and Seligman (1975) suggested that motivational deficits may be reflected in longer latencies. The problem is that the subject may still be trying and thus be motivated, yet score poorly on this measure.

Miller and Norman (1979) expressed concern at the limited variety of tasks that have been used in test phases. With few exceptions these have been cognitive, problem solving tasks. The limited range of tasks raises questions concerning the robustness of the helplessness phenomenon.

A number of methodological issues have been raised. These include the ambiguities inherent in the design, the role of demand characteristics, and the lack of precision surrounding experimental manipulations. Questions regarding the dependent measures have also been raised. The methodological confusion apparent in the helplessness literature serves further notice that results of such studies must be viewed cautiously. Similar caution is required in the theoretical developments, which are reviewed in the next section.

### Theoretical Explanations and Issues

Seligman (1975) published a theoretical account of the learned helplessness phenomenon which argued for three components. These were: information regarding the contingency of outcome upon response, a cognitive representation of the contingency and a behavioural deficit (as characterised by the passivity displayed by helpless subjects). Seligman maintained that the cognitive representation, which usually

took the form of an expectancy of uncontrollability, was the crucial determinant of helplessness. This expectancy has been described as the cornerstone of the theory (Maier & Seligman, 1976).

Clearly, there are some outcomes which we learn the futility of attempting to influence. Seligman's statement suggests that once this contingency has been learned, certain implications follow with regard to behaviour in certain contexts. It is the delineation of these contexts which is the concern of the present thesis.

The contradictory results (see Roth, 1980, for a review) provided by the helplessness literature, have failed to confirm the basic hypothesis of the early theory. Accordingly a number of researchers have suggested alternative theoretical explanations in an attempt to make sense of the inconsistent results.

In applying the reactance model to the helplessness domain, Wortman and Brehm (1975), suggested that subjects' loss of control is initially associated with reactance and attempts to re-establish control. In a situation where an individual expects control, exposure to negative outcomes initially produces reactance. This takes the form of increased motivation to execute controlling responses. With repeated exposure to noncontingency, reactance dissipates, is replaced by helplessness, and a decreased motivation to execute controlling responses.

The reactance model predicts outcomes which are at odds with the helplessness model. Helplessness theory predicts passivity and 'giving up' in individuals exposed to uncontrollable outcomes, while the reactance model predicts initial invigoration of responding. This facilitation effect is well documented in the literature. The predictions from the reactance model parallel the proposals of Dyck, Vallentine and Breen (1979). Their contribution is discussed later.

Although promising, the reactance model has not been able to explain in detail when and how the typical helplessness pretreatment would lead to facilitated rather than impaired performance (Williams & Teasdale, 1982). The predictions based on this model have not been consistently supported by empirical findings.

Sargent and Lambert (1979) provided another interpretation of learned helplessness in making a distinction between failure and noncontingency, they termed their explanation 'learned incompetence'. They reasoned that when subjects in helplessness inductions are exposed to a noncontingent relationship between responses and outcomes, the subjects are also exposed to an experience of failure. While failure and noncontingency are confounded in pretreatment tasks it becomes difficult to determine which is responsible for the learned helplessness effect.

The learned incompetence interpretation suggests that subjects simply learn that they are incompetent. They

became increasingly aware of their inability to succeed, in spite of trying and (initially at least), expecting to succeed.

Frankel and Snyder (1978) proposed that the notion of egotism is a viable one in terms of being able to account for helplessness deficits. Egotism is defined as a general tendency to take credit for good outcomes and to deny blame for bad ones. Their explanation centered around the hypothesis that people are motivated to protect their self-esteem. Subjects not only learn that outcomes are independent of responses, they also learn that they have failed the tasks assigned to them. This failure is interpreted as a threat to self-esteem. It is suggested therefore that following experience with unsolvable problems, subjects do not try when presented with the test task. By not trying a subject is able to attribute his subsequent failure to lack of effort, which in terms of preserving ones self-esteem is a more palatable attribution than lack of ability.

The egotism explanation accounts for debilitated performance on test tasks as a strategy on the subjects part in an attempt to preserve self-esteem by refusing to try to complete the test task. There is some conceptual similarity between the egotism explanation and the attributional strategy of self handicapping (Jones and Berglas, 1978). Self handicapping involves protection of ones self image by arranging conditions such that one's failures will be attributed to external causes rather than internal traits and abilities.

Peterson (1978) put forward yet another theory, an 'altered hypothesis pool' explanation, which assumes that subjects are both active and problem solving. He argued that subjects actively attempt to discover what is happening in experiments. This active discovery takes the form of successive formulation and evaluation of hypotheses, a process similar to that outlined by Kelly (1955).

Assuming 'active discovery', Levine's, (1971, cited in Peterson, 1978), hypothesis testing theory was applied to the learned helplessness paradigm. The following alternative explanation resulted : subjects given insoluble problems learn not that the problems are insoluble, but that simple solutions do not suffice. Their subsequent attempts to solve problems draw upon complex hypotheses as potential solutions. When subjects are later given problems with simple solutions, their tendency to seek complex answers impairs performance.

In summary, a number of alternative interpretations have been proposed to account for the various findings which research has uncovered. The reactance proposal suggests initial loss of control is met with immediate attempts to re-establish control. The learned incompetence proposal advocates that subjects learn incompetence and come to believe in their own inability. The egotism model views subjects as protecting self-esteem. Little effort is invested in tasks whereby subsequent failure maybe attributed to reduced effort, not inability. Other explanations emphasise the active problem solving nature of subjects.

A number of moderating variables may influence the outcome of helplessness inductions. Dyck et al. (1979) proposed that previous failure may be such a factor. They attempted to explain the incongruity between impaired and facilitated performance, by hypothesising a relationship between previous failure and subsequent persistence at problem solving. They proposed a curvilinear relationship, such that brief duration failure increases subsequent performance, while extended failure decreases subsequent performance. Roth and Kubal (1975) provided the initial demonstration of this effect. They also suggested that the training task may be another possible moderator. They reasoned that uncontrollability in relation to a task presented as important would more likely result in subsequent deficits. They showed that uncontrollable outcomes had more impact when the training task was presented as being important. On the basis of this and the recommendations of Margolies (1978) an importance manipulation was employed in the current study.

Lavelle, Metalsky and Coyne (1979) view anxiety as a further moderating variable. They maintained that high test-anxious subjects are especially prone to the debilitating effects of a helplessness induction. Low test-anxious subjects are correspondingly predicted not to display subsequent task interference. It is argued that low test-anxious subjects are able to focus behaviour and cognition toward task specific requirements, to the exclusion of extraneous ideation.

Hiroto (1974) identified Rotter's concept of locus of control as a further moderating variable. The helplessness notion of learning that outcomes are uncontrollable is similar to Rotter's concept of external locus of control. Rotter classified beliefs about causality as internal or external. Simply viewed, one's locus of control is a statement concerning one's generalised expectations regarding how reinforcement is controlled. Internals perceive reinforcements as response dependent and controllable, while Externals view reinforcements as response independent and uncontrollable. Hiroto (1974) demonstrated that Externals suffered greater performance deficits following exposure to noncontingent reinforcement, than did Internals. Albert and Geller (1978) similarly concluded that Externals are more susceptible to helplessness manipulations than Internals.

Seligman (1975) originally presented learned helplessness as a model for reactive depression. Although the model stimulated considerable research, it has not been accepted uncritically. For example, Depue and Monroe (1978) made three criticisms. Firstly they pointed out that reactive depressives are not a single homogeneous group. Secondly they argued that the etiological role of environmental events is neither as simple nor as distinctive as Seligman suggested. Finally they noted that many of the features of depression that Seligman believed parallel learned helplessness in animals are features more characteristic of endogenous rather than reactive depression.

Buchwald et al. (1978) maintained that the demonstration that a procedure produces some features of a disorder in the

laboratory is not sufficient to demonstrate the etiology of the disorder. Although the model may be insufficient, there are still grounds for assuming that there exists a relationship between depression and learned helplessness. Klein, Fencil-Morse and Seligman (1976) provided evidence to this effect. Accordingly it may be prudent to assume that depression acts as another moderating variable.

A final moderating variable may be time. Seligman (1975) predicted dissipation of the effect of learned helplessness over time. The process involved was discussed by Zuroff (1980) who suggested that with the passage of time, subjects' expectancies shift toward what they had been prior to training. With delay there may be a resurgence of subject's generalised expectancy resulting in the undoing of the effects of training. A time delay was incorporated into the current study to investigate the effects of time on helplessness effects.

A number of writers have questioned the assumptions on which the helplessness model is based. While interference with subsequent task performance has been well documented, Lavelle et al. (1979) argued that such demonstrations should not be taken, ipso facto, as proof of the underlying cognitive and motivational deficits postulated by the model. Rather these demonstrations may be indicative of other processes which are at play (e.g. anxiety).

The undesirable, and maladaptive nature of helpless behaviours is apparent in the work of researchers (Jones et al., 1977; Prindaville & Stein, 1978; and Teasdale, 1978) who have concerned themselves with such issues as inoculation, therapy, and prevention. Wortman and Dintzer (1978) questioned the validity of this assumption. They asked, what types of reactions are to be considered adaptive in the face of uncontrollable outcomes? The behaviours associated with helplessness (giving up, losing interest in outcomes) are maladaptive only when the outcome is in fact controllable. They argued that if the outcome is truly uncontrollable, as appears to be the case during the training phase, these behaviours may be highly functional. Even depressed affect may prove to be functional in serving the purpose of motivating the person to re-evaluate his goals in the light of uncontrollable outcomes. The assumption of undesirability may be unjustifiable in this case.

Peterson (1978) criticises the premise that subjects learn noncontingent relationships. There are a number of reasons for believing that people have difficulty in learning that events in a psychology experiment are noncontingent. Jones (1971, cited in Petersen, 1978) asserts that subjects are inclined to believe there are patterns even when randomness is allegedly explicit. Orne (1970, cited by Peterson, 1978) similarly argued that subjects in a psychology experiment fail to consider the possibility that the events within it are objectively without meaning. If in fact, subjects fail to learn noncontingent relationships, such learning can hardly be used to explain the helplessness effects.

In summary a number of important questions have been raised concerning the fundamental assumptions of the helplessness model. The phenomenon has also been open to a number of alternative interpretations regarding the meaning of the results. Furthermore a number of moderating variables (both situational and personal) have been shown to affect the experimental outcome with humans. These factors all serve as testimony to the complexity of the phenomenon. In an attempt to better account for this complexity on a theoretical level, the 'reformulated' model of learned helplessness was developed (Abramson, Seligman & Teasdale, 1978). This reformulation is reviewed in detail in the following section.

#### The Reformulated Model

Abramson et al., (1978) acknowledged two inadequacies which the reformulation was designed to overcome. The first inadequacy concerned the lack of distinction between the case in which neither the individual nor others control outcomes, and the case in which only the individual is unable to control outcomes. The former case is labelled universal helplessness and the latter personal helplessness. The model suggests that personal helplessness leads to an internal attribution, while universal helplessness results in external attributions.

The second inadequacy concerns the lack of specification of when and where a person who expects the outcomes to be uncontrollable will show the motivational and cognitive deficits of helplessness. The reformulated model holds that

an individual's reaction to an uncontrollable event is mediated by the attributions which are made regarding that event.

Attributions are to be understood along three orthogonal dimensions: internal-external, stable-unstable, and global-specific. The internal-external dimension differentiates between causes that stem from the individual, versus those that stem from environmental or situational factors. It is the contention of the reformulated model that attributions to internal factors result in greater loss of self-esteem, than external attributions.

Stable attributions pertain to factors that persist over time whereas unstable factors are viewed as being more transient in nature. Correspondingly the Abramson et al. model predicted that stable attributions result in helplessness deficits which extend across time. Unstable attributions result in short lived deficits.

Global attributions refer to factors which are prevalent across situations, while specific attributions refer to factors which are unique to the uncontrollable event. Attributions to global factors are predicted to lead to wide generalisation of helplessness deficits across situations.

Jackson and Larrance (1979) questioned the necessity of refining attribution theory to accommodate the reformulation of learned helplessness. They suggested that the three attributional dimensions of Abramson et al. can be derived

directly from Kelley's (1971) original criteria of consensus, consistency and distinctiveness. They concluded that the similarities between the two are great, while the differences are minimal.

While the reformulation is acknowledged as representing a major step forward (Zureff, 1980) it has been criticised on a number of grounds. An important criticism concerns the fundamental premise - that people make attributions, even when psychologists do not solicit them. Wortman and Dintzer (1978) suggested the best way to resolve this was to expose subjects to uncontrollable outcomes and allow them the opportunity of a free response. Hanusa and Schulz (1977) offered their subjects this opportunity. Generally their subjects failed to spontaneously report attributions for their success or failure.

Another criticism regards the relationship between attributions of causality and subsequent behaviour. Wortman and Dintzer (1978) reviewed a number of studies relevant to this question, before concluding that the link between attributions and behaviour is less straightforward than Abramson et al. have implied. Zuroff (1980) similarly commented on the unpredictable ways in which attributions relate to subsequent performance.

Wortman and Dintzer (1978) distinguished between attributions regarding the outcome and attributions regarding one's reaction to the outcome. They proposed that the magnitude of helplessness effects may be influenced by people's

attributions of causality (as the reformulated model predicts), and also attributions regarding one's ability to cope with the outcome.

A number of other issues have been raised in connection with the reformulated model. Zureff (1980) pointed out that it fails to adequately account for facilitation effects, neither does it pay sufficient attention to the importance of individual differences. Other factors were also mentioned by Wortman and Dintzer (1978). These included whether the subject expected control or not; whether subjects find meaning in the outcome; and the extent to which subjects assess the cost of attempting to influence outcomes. They also questioned whether the attribution dimensions selected by Abramson et al. were the most important ones in predicting the nature and magnitude of future deficits.

Arising out of the inadequacies of the early theory the reformulated account has attempted to employ a cognitive moderating variable (causal attributions) to increase the power of the theory. However as the above analysis shows a number of critical factors are not accounted for by the model. What this probably demonstrates is the complex nature of learned helplessness.

#### The Generalisation of Learned Helplessness

Two interrelated aspects of generalisation are important. The first regards the process of helplessness as an inappropriate generalisation from the training phase

of the helplessness induction to the test phase. The second views generalisation in a broader context. It considers the link between the results of experimental research and the application of these results to areas such as depression. Both aspects are important for the rationale of the present study.

Roth and Bootzin (1974) expressed the view that an inappropriate generalisation is at the very heart of the helplessness model. They suggested that the interest in learned helplessness is an interest in inappropriate expectancies (believing external events are uncontrollable, when in fact they are not), due to an inappropriate generalisation from an experience where control was not possible. Thus the inappropriate generalisation (belief in noncontingency) is generated in a situation where control is not possible and generalises to a situation where control is, in fact possible.

In the case of a successful helplessness induction a subject acquires the belief in noncontingency, appropriately, as a result of the noncontingent nature of the reinforcement received. This belief is then generalised (inappropriately) to a second situation where reinforcements are contingent upon response. Several researchers (Roth & Kubal, 1975; Wortman & Brehm, 1975) have questioned the inappropriateness of this generalisation which is linked to the degree to which the training and test tasks are separated. When helplessness effects are obtained, and training and testing are not clearly separated, the results may simply reflect

learned scepticism. Subjects may not believe the experimenter when informed that the testing task is soluble.

In short, to the extent that helplessness effects can be demonstrated when the testing phase is clearly separated from the training phase, the phenomenon becomes theoretically interesting.

Miller and Norman (1979) maintained that the significance of the helplessness paradigm is largely tied to the degree of generalisation that occurs. They argued that a reduction in responding is adaptive in a situation where responses do not influence outcomes. It only becomes maladaptive when it is generalised to a new situation in which outcomes are contingent upon responses.

Miller and Norman (1979) reviewed six studies which investigated the generalisation issue by using a test phase that was situationally dissimilar from the training phase. Half these studies reported deficits (Roth & Kubal, 1975,; Sherrod & Downs, 1974; Tennen & Eller, 1977), while half reported no deficits, (Roth & Bootzin, 1974; Wortman, et al., 1976; Kilpatrick - Tabak & Roth, 1978). Of those that obtained cross-situational generalisation, two common factors emerged; (a) prolonged exposure of subjects to noncontingent failure outcomes, and (b) instructions designed to induce an attribution of task performance to ability. Tennen and Eller (1977) for example, separated the two phases of the experiment, used different experimenters and separate rooms. Additionally subjects were exposed to extensive

pretreatment (up to three problems, each consisting of 48 Levine-type discrimination trials) and internal attributional cues. (Subjects were told that successive problems during the training phase were easier). One study, however (Kilpatrick-Tabak and Roth, 1978) exposed subjects to both these conditions and still failed to obtain generalisation.

In addition to the studies reviewed by Miller and Norman (1979), studies into the parameters of generalisation by Cole and Coyne (1977) and Tiggemann and Winefield (1978) are relevant. Cole and Coyne (1977) argued for the situational specificity of laboratory-induced helplessness. They reported helplessness effects when training and test situations were presented as one experiment and administered by one experimenter. However they failed to demonstrate helplessness when the two phases were presented as different, and administered by different experimenters. This result was regarded as a failure to provide evidence of cross-situational generalisation. Tiggemann and Winefield (1978) reported a similar finding. Neither of these studies attempted to employ attributional manipulations to induce attributions for performance to stable, global, internal factors. Both these studies predate the attributional reformulation.

In summary, an inappropriate generalisation may only be expected when the training and test tasks are clearly separated. The current study conducted the two phases as if they were two unrelated experiments. Hiroto and Seligman (1975) and Cohen, Rothbart and Phillips (1976)

reported that helplessness generalises from one type of task to another (across training and testing situations). Accordingly the current study employed a traditional task for training but a novel testing task. This task involved social interaction, which previous studies had not tried.

Miller and Norman (1979) noted the lack of conclusive evidence regarding the degree of generalisation across situations. This failure to demonstrate cross-situational generalisation is a major weakness in the helplessness literature. Without conclusive evidence for cross-situational generalisation the significance of the learned helplessness phenomenon becomes questionable. Without such evidence the learned helplessness effect must be regarded as only situation specific.

The current study attempted to demonstrate cross-situational generalisation (i.e. inappropriate generalisation on the part of the subject) by using a traditional training task and a novel testing task.

### The Importance of Learned Helplessness

The second issue pertaining to generalisation is found in a somewhat wider context. Dewey (1899, cited in Sarason, 1981) issued an early warning,

Unless our laboratory results are to give us artificialities, mere scientific curiosities, they must be subjected to interpretation by gradual reapproximation to conditions of life. The results

may be very accurate, very definite in form; but the task of reviewing them so as to see their actual import is clearly one of great delicacy and liability to error. (p. 75).

Westland (1978) updated the same problem in his discussion of the 'Laboratory' crisis. He referred to research which abstracts from life and imposes arbitrary constraints. He regarded the validity of the results as being limited to the context in which they were obtained, telling us little about what will happen in normal everyday life. This criticism is directed at the meaningfulness of results obtained and ultimately the relevance of such results for the so called 'real world'. Rippere (1977) made the point that the helplessness effect is an artifact of the arbitrary laboratory conditions in which subjects are typically placed.

As condemning as these arguments may appear for laboratory studies, they actually serve to emphasise the importance of the present study, and will also be used as further justification of the need to undertake the current research. The essence of the preceding criticisms is that the results of laboratory studies have validity only for those contexts in which they originated. The current study similarly possesses many experimental artificialities, but its strength lies in its attempt to demonstrate that results are not limited to the context in which they were obtained. In attempting to demonstrate cross-situational generalisation of learned helplessness this study attempts a first step in the process of gradual reapproximation. As Dewey (cited in Sarason, 1981) suggested, the outcome of such reapproximation will assist

in defining the importance of the results. In this regard a fundamental premise of this study is that the significance of the helplessness phenomenon is inexorably linked to the issue of generalisation.

Following their demonstration of cross-modal helplessness, Hiroto and Seligman (1975) suggested that learned helplessness may be an induced trait. In describing the helplessness effect as being trait-like they implied that helplessness is a rather robust phenomenon, which once induced may have applications beyond the situation in which it was conceived. Maier and Seligman (1976) stated their belief that what is learned when the environment is uncontrollable can have consequences for a wide range of behaviour. They further noted "we believe that the psychological state produced by uncontrollability may undermine response initiation quite generally" (p. 12).

The limits of generalisation have also been discussed by Hiroto and Seligman (1975) when they asked whether any helplessness was actually taken out of the laboratory. The current research asks essentially the same question. Maier and Seligman (1976) urged that the limits of generalisation be explored and further delineated. The present thesis holds similar aims.

The wide applications of the helplessness model (e.g. Seligman's 1975 book was entitled "Helplessness, On Depression, Development and Death"), assume the generality of the phenomenon. In discussing the learned helplessness model of

depression, Cole and Coyne (1977) noted that the model viewed depression as a behavioural pattern characteristic of situationally generalised helplessness. The argument presented here is that the appropriateness of such applications should be based on empirical demonstrations that helplessness is capable of being widely generalised. Indeed, the significance of the phenomenon must be related to demonstrations of its generality.

The issue of generality is of singular importance. The very importance of the phenomenon rests upon demonstrations of broad generality. Similarly, practical applications assume that helplessness has high generality - this assumption demands assessment.

### Objectives and Hypotheses

The prime objective of the study was to provide a test of cross-situational generalisation of learned helplessness. Before considering whether helplessness effects had generalised it was first important to establish that the helplessness induction had been successful. This gave rise to the first hypothesis.

Hypothesis 1: That subjects in the noncontingent group would show increased levels of emotionality after exposure to uncontrollable outcomes, while those in the contingent and control groups would not.

Increased emotionality was used to determine the successfulness of the helplessness induction. This assumes the presence of performance deficits, although does not specifically test for these. The theory of learned helplessness predicts increased affect and performance deficits following a helplessness induction. A number of studies (Gatchel, Paulus & Maples, 1975; Miller & Seligman, 1975; Roth & Kubal, 1975; and Pittman & Pittman, 1979) have documented impaired performance in conjunction with increased emotion. No study has yet found the presence of affective changes, unaccompanied by behavioural deficits, although behavioural deficits have sometimes occurred without an emotional change (Cole and Coyne, 1977).

The test of generalisation was dependent on the confirmation of Hypothesis 1.

Hypothesis 2: That noncontingent subjects would show performance deficits relative to the control subjects on the test task.

The design incorporated a 10 minute wait for subjects between training and test tasks. The effect of this wait on the extent of helplessness was assessed by the third hypothesis.

Hypothesis 3: That noncontingent subjects who waited 10 minutes prior to the test task would display less debilitated performance than noncontingent subjects who experienced no time delay.

## Method

### Overview and Design

The experiment was conducted in two phases; the first established the effectiveness of the helplessness induction, while the second assessed the generalisation of helplessness. The design for the first phase was a two factor mixed design with a repeated measure on the second factor. The second phase was a 3 (group) x 2 (sex) x 2 (time) factorial design. Subjects were randomly assigned to one of three groups. The contingent group received contingent (response dependent) feedback. The noncontingent group received noncontingent (response independent) feedback, while the control group received no pretreatment. Both contingent and noncontingent groups were given a cognitive task similar to that used by Hiroto and Seligman (1975). The second phase compared the performance of subjects from all three groups on a test task. This task was conducted with groups of six subjects, two from each of the experimental conditions. For this phase subjects were run in same-sexed groups; thus sex was an added control variable.

## Subjects

Subjects were volunteers drawn from two first year psychology papers. Students from these classes were given a Consent Form for experimental subjects (see Appendix A). The information on the form was read aloud. In addition the first experiment was described as a 'learning experiment', and the second as looking at 'group processes and decision making'.

In accordance with Tesch's (1977) recommendation, the students were advised of the possible benefits if they became subjects. Specifically they were told they would have the opportunity of seeing how laboratory experiments are conducted with human subjects.

From a pool of 191 students, 121 volunteered (74 female, 47 male). Their ages ranged from 17 to 39, with the majority (86%) falling into the 18 to 21 age range. In terms of age and sex, those who volunteered were a representative sample of the available pool.

Only 90 of the 121 volunteers were actually required to participate. The design of the experiment was such that subjects were run in same-sex groups of six. Mutually convenient times for group members to meet were arranged by telephone. These arrangements were completed several days in advance, and subjects were again telephoned as a reminder on the eve of their appointed meeting time.

Those volunteers who did not actually participate, failed to do so for a variety of reasons: some were unable to be contacted; for others it was not possible to negotiate a convenient time.

The running of two groups was interrupted by subjects not arriving on time. In both cases alternative subjects were found causing only a brief delay in the procedure.

### Experimental Materials

#### Cognitive Training Task

The cognitive training task employed in the first phase of the experiment was similar to the cognitive pretreatment task used by Hiroto and Seligman (1975). This type of task has been used extensively in the helplessness research literature, (Benson & Kennelly, 1976; Klein, et al., 1976; Griffith, 1977; Jones et al., 1977; Breen et al., 1979; Pasahow, 1980).

A series of five-dimensional stimulus patterns similar to those used in discrimination learning studies, (Levine 1966,1971), formed the basis of the cognitive task. Each of the five stimulus dimensions had two associated values. The dimensions and their two values were as follows:

(a) letter, (A or T); (b) colour, (red or black); (c) letter size, (large or small); (d) border, (square or circle); (e) dots, (1 or 2).

These stimuli were presented in pairs. The second stimulus always being the compliment of the first i.e. if one side of the card contained an "A" then the stimulus next to it would necessarily be a "T", if the A was red then the T would be black, and so on across the five dimensions. (See Appendix B for an example of one of these stimulus cards).

From the possible total of 32 cards, 11 were chosen randomly for the current research. The first of these functioned as a demonstration card, the remaining ten comprising the stimulus cards which formed the basis of the training task. These cards were also viewed by control subjects.

The majority of the research which has used these tasks to induce learned helplessness utilised four-dimensional stimulus patterns. A number of studies, (Roth & Bootzin, 1974; Roth & Kubal, 1975; Jones et al., 1977; and Pittman & Pittman, 1979), did successfully employ five-dimensional stimulus patterns. Five-dimensional patterns were used here to increase the effectiveness of the helplessness inductions. It was assumed that a five-dimensional x four-problem induction was more likely to result in a successful induction than a four-dimensional x three-problem induction (Krantz, Glass & Snyder, 1974; Roth & Kubal, 1975; Marotta, 1977).

The stimulus patterns were presented on white cards (10 cm x 15 cm), bound in a ring binder.

### Affect Measurement

The Multiple Affect Adjective Check List (MAACL: Zuckerman, Lubin and Robins, 1965) was administered to each subject before the training phase and again after it, in order to assess mood change. The MAACL elicits self report ratings of Depression, Anxiety and Hostility.

The MAACL comprises 132 adjectives, arranged alphabetically. Of these items 21 assess Anxiety, 40 assess Depression and 25 items assess Hostility. There are 43 buffer items which are unscored.

There are two versions of the MAACL: one assesses general feelings; while the other instructs subjects to respond in terms of their immediate feelings (the so-called Today form). The Today form was used in this research. The checklist (available commercially) is reviewed (see Buros, 1972) by Kelly and also by Megargee. According to Kelly the reliability (odd-even and plus-minus) for both forms ranges between .17 and .92, with the median being .72. Thus the checklist possesses reasonable internal consistency.

The MAACL purports to measure changes in subjects' moods over relatively short time intervals. Its use in the current study was based on the assumption that it would be sensitive to affective changes. Both reviewers cite low test-retest reliability coefficients, when normal subjects are used. This is as expected, since high test-retest reliability would suggest either that subjects feelings were static, or that the check list was insensitive to change.

In discussing the validity of the instrument Megaree believed the fundamental question is whether the state scores change over time and whether the nature of these changes is consistent with clinical expectations or observations. Much of the research on the validity of the MAACL has consisted of administering the Today form under conditions likely to elicit the relevant affective states and comparing the scores with those obtained under normal conditions. Students have been tested before and after examinations, military personnel have been tested during basic training and actors have also been tested prior to going on stage. Hypnotically induced mood states, sensitivity training, stress interviews, tranquilising medication and induced relaxation have been used to alter mood states, and concomittant changes in the MAACL scores have been noted. Megargee concluded that by and large, the results of these studies have been positive. Kelly's comments were similarly encouraged when considering the validity of the MAACL,:

In spite of the very high interscale correlations noted above, the three scales of the MAACL appear to have sufficient differential validity to reflect meaningful changes in affect for groups of S's subjected to different types of stresses and stress-reducing manipulations. (Buros, 1972, P. 272).

The MAACL has been used previously in research into human helplessness. Cole and Coyne (1977), used the MAACL to assess pretreatment mood. Gatchel et al., (1975), employed multiple administrations of the MAACL to gauge mood change. Miller and Seligman, (1975) similarly used the MAACL to measure the effect of a helplessness induction on subjects' mood states.

#### Post - Experimental Questionnaire

The questionnaire was included to assess the effectiveness of the experimental manipulations. The questionnaire (see Appendix C) considered two questions. Firstly, subjects were asked to rate the importance of the task in experiment one, on a seven point scale. The exact nature of the manipulations designed to increase the importance of the training task, are outlined in the Procedure section. As discussed in Chapter 1 previous research shows the need to increase the importance of the training task.

The second question asked subjects to rate the influence of factors (task difficulty, luck, ability and effort) on their performance on the training task. These ratings (on a seven point scale) were included as a check on the effectiveness of the attributional manipulations. Also of interest was the extent to which subjects were aware of the false feedback provided by the experimenter. Although of interest, this question was not specified for fear of creating an unnecessary demand characteristic.

### Dependent Measures

Three dependent measures were obtained for each subject. The first of these was the number of utterances (Number); the second was the total time the person engaged in meaningful speech (Time); while the third (Probability) was derived from Time. The criteria for determining meaningful speech were taken from Ward (1970), (See Appendix D).

Ratings were completed by two independent raters who were blind as to which experimental condition subjects were from. The scores for these raters were averaged. Interrater reliability was assessed by Pearson product-moment correlation coefficients. For Number,  $r(88) = 0.9418, p < .005$  and for Time,  $r(88) = 0.9789, p < .005$

The third dependent measure (Probability) was derived from the second (Time). This measure was derived because it adjusts for, and takes into account, intergroup (experimental condition) dependence. (See Appendix E for further rationale and procedures relevant to this dependent measure).

As expected the three dependent measures correlated highly. Pearson correlation coefficients were: Number and Time,  $r(88) = 0.7840$ ,  $p < .005$ ; Number and Probability,  $r(88) = 0.6368$ ,  $p < .005$ ; Time and Probability,  $r(88) = 0.8851$ ,  $p < .005$ .

### Procedure

Two pilot groups were run to iron out any obvious difficulties in the procedure. As a result of these pilot runs the test task was modified. The task was made more difficult to ensure subjects did not complete the task before the standardised time of ten minutes expired.

### Pretreatment Tasks

Subjects were run in 15 same-sexed groups of six, (ten female and five male groups). Each group comprised two subgroups of three. These two subgroups were run consecutively, the first group experienced a time delay after the pretreatment task while the second group received its pretreatment.

On arrival, subjects were randomly assigned to one of the three experimental conditions. Each of the three subjects were shown to a room where the pretreatment task was conducted independently. The pretreatment began when subjects were asked to complete a MAACL.

For contingent and noncontingent subjects instructions for the training task were identical (see Appendix F). The first phase of these instructions, involved manipulations designed to increase the importance of the task and also to manipulate subjects' causal attributions for their subsequent success or failure to stable, global, internal factors. Four problems were presented, the same ten cards being used for each problem. For contingent subjects a value was

chosen, say letter t, and their task was to discover this solution as soon as possible within the space of the ten cards. For the second problem another value was selected. The correct values were always the same for each subject in this condition (Red, T, Circle and 2 Dots). For noncontingent subjects, no value was consistently correct even though subjects were deceived into thinking that problems were soluble. The exact pattern of feedback for each noncontingent subject was as follows:

(C = Correct I = Incorrect)

1	C	I	I	C	C	I	I	C	C	I
2	I	C	I	C	C	I	C	I	C	I
3	I	C	I	C	I	C	C	I	C	I
4	C	I	I	C	C	I	I	C	C	I

For each problem 50% of responses were "correct" and 50% "incorrect". The subject's response to the final card was, for each problem, incorrect. The procedure used by Jones et al. (1977) served as a model for the present study.

Pretreatment training tasks were conducted by two experimenters, the author and a female assistant (Psychology graduate). The involvement of the two experimenters was counterbalanced to account for bias resulting from the sex of the experimenter. For each group of six subjects, each experimenter was responsible for the pretreatment of one contingent and one noncontingent subject, the order of this being alternated for each group.

The pretreatment task for control subjects consisted of simply viewing the same stimulus cards as were used in the pretreatment of contingent and noncontingent subjects. Control subjects were instructed to flip through the cards for about ten minutes. Some subjects asked if they were to be tested on the content of the cards. It was considered that this question was unlikely to contaminate the hypothesis under study and so was answered honestly.

For pretreatment tasks, the second subgroup of three subjects were asked to arrive about 15 minutes after the first subgroup. Pretreatment for these subjects began at the conclusion of the first subgroup's pretreatment. This split approach was incorporated into the design as it provided the opportunity to test the effect of the passage of time on helplessness. Also this procedure reduced the number of experimenters involved, from five to three.

Accordingly the design incorporated a 15 minute wait for the first subgroup, while the second subgroup received pretreatment. On arrival at the second experiment this wait was explained to subjects who were shown to individual rooms and provided with reading material. This material consisted of art books and popular magazines. Subjects were asked to wait separately so as to reduce any possible contaminating effects before the experiment began.

### Test Task

The task (Front Page Planning) was an exercise in majority and consensus group decisions (Juniper, 1976). (See Appendix G for instructions for the test task).

Members of the group were requested to assume the roles of members of an editorial board for a leading daily newspaper. The board was democratic and their task was to rank order a number of headlines in terms of importance. The task was chosen because it was a group task in which all members had equal opportunity to contribute to the decisions made by the group. The task required a high level of social interaction, something not evident in previously used test tasks.

On the arrival of the second subgroup of three subjects the test task began. The six chairs were arranged in horseshoe shape. Subjects were randomly assigned to seating positions. This was done to ensure no positional bias occurred in the actual discussion.

Following seating, subjects were given a copy of Front Page Planning and additional instructions (See Appendix H). The experimenter left the room and all interactions were recorded on video tape for a standardised time (ten minutes). This tape was used for subsequent analysis.

### Post Experimental Checks

At the conclusion of the test task the experimenter issued each subject with an envelope and instructions to self-address it. (This was later used to post debriefing notes to subjects). Subjects were told this was to enable the experimenter to post a copy of the results to each subject.

A copy of the Post Experimental Questionnaire was given to subjects, with the explanation that the person doing the first experiment had forgotten to give those out. On the completion of this questionnaire subjects were thanked and, with the exception of the two noncontingent subjects, were dismissed.

### Ethical Considerations and Debriefing

Because of the deceptions involved and the importance of the delayed debriefing, a number of ethical questions arose. These questions were presented to, and discussed with the Departmental Ethics committee, who approved the present procedure.

Tesch (1977) criticises debriefing procedures, arguing that it becomes an article of professional faith, that debriefing sufficiently minimised or eliminated any serious vilification of devious experimenters, that it functioned remedially to disabuse participants of any negative affect or outcome, and that it

justified the experimenter's actions to the participants.  
(p. 218).

However the Abramson et al. (1978) reformulation holds that debriefing for helpless subjects is effective because it shifts subjects' attributions from global factors (e.g. "I'm stupid") to specific factors (e.g. "this was an impossible task."). Abramson et al. showed an awareness of the elements of faith, discussed by Tesch. They noted, "Experimenters in human helplessness studies seem to believe that telling a subject that no one could solve the problem will cause helplessness deficits to go away" (p. 55).

Tennen and Gillen (1979) however provided empirical support for the effectiveness of debriefing. They tested the effectiveness of debriefing in reversing the effects of laboratory induced helplessness. Their results showed that such effects were reversed by debriefing after helplessness training. The debriefing involved an explanation of the uncontrollability of the situation.

When the two noncontingent subjects were reintroduced to the principal experimenter, the uncontrollable nature of the training task was explained to them. It was further explained that this was done in order to study the effects of a frustrating situation. Any doubt or queries the subjects had were discussed.

Klein and Seligman (1976) noted performance deficits were reversed following exposure to success on similar problems, so noncontingent subjects were provided with similar exposure. The subjects were informed that the problem was of the same type, but the feedback would be veridical. Reluctance on the part of subjects was met with further encouragement by the experimenter. The problems (one per subject) were given in an informal manner, in such a way that the other was able to experience vicariously the success of the first subject.

Finally the cooperation of the noncontingent subjects was requested. They were asked to remain silent until the last of the experimental groups had been run. The importance of subjects' initial naivete was emphasised, and verbal assurance was sought that they would not break experimental confidentiality.

Subjects were again thanked and dismissed.

At the conclusion of the experiment all subjects were completely debriefed by post. The debriefing notes (see Appendix I) involved a complete disclosure of all the deceptions involved and the rationale behind them.

The experimental precedent for debriefing by post was set by Sherrod and Downs (1974). The reason for its inclusion in the present study was the importance of dealing with subjects who were naive as to the hypotheses of the study. Immediate and total debriefing would have posed a serious threat to the internal validity of the experiment.

## Results

The basic design relating to the first hypothesis was a two factor, mixed design with a repeated measure on the last factor, (Keppel, 1973).

In order to establish whether significant between group differences existed prior to the training phase a one way analysis of variance was computed on pre-test scores for each of the affective scales.

As expected, no significant differences were found on any of the affective scales, (Anxiety,  $F(2,87) = 1.125$ ; Depression,  $F(2,87) = 1.409$ ; Hostility,  $F(2,87) = 0.416$ ).

A 3 (treatment groups) X 2 (pre and post test) analysis of variance with a repeated measure on the last factor was computed for each of the affective scales. This was completed in order to determine whether significant differences existed between pre and post scores, and whether such differences were the same for different treatment groups. Separate ANOVAS were computed for each scale, rather than a multivariate analysis, since the three scales were regarded as measuring separate affects, (See Appendix J for summary tables of the three ANOVAS, one for each scale). Means and standard deviations for each of the cells are also included, (See Appendix J). The means for pre-test and post-test scores for each group, on each of the affective scales are graphed in Figure 1. This figure shows the noncontingent group increasing their scores from pre-test to post-test on each

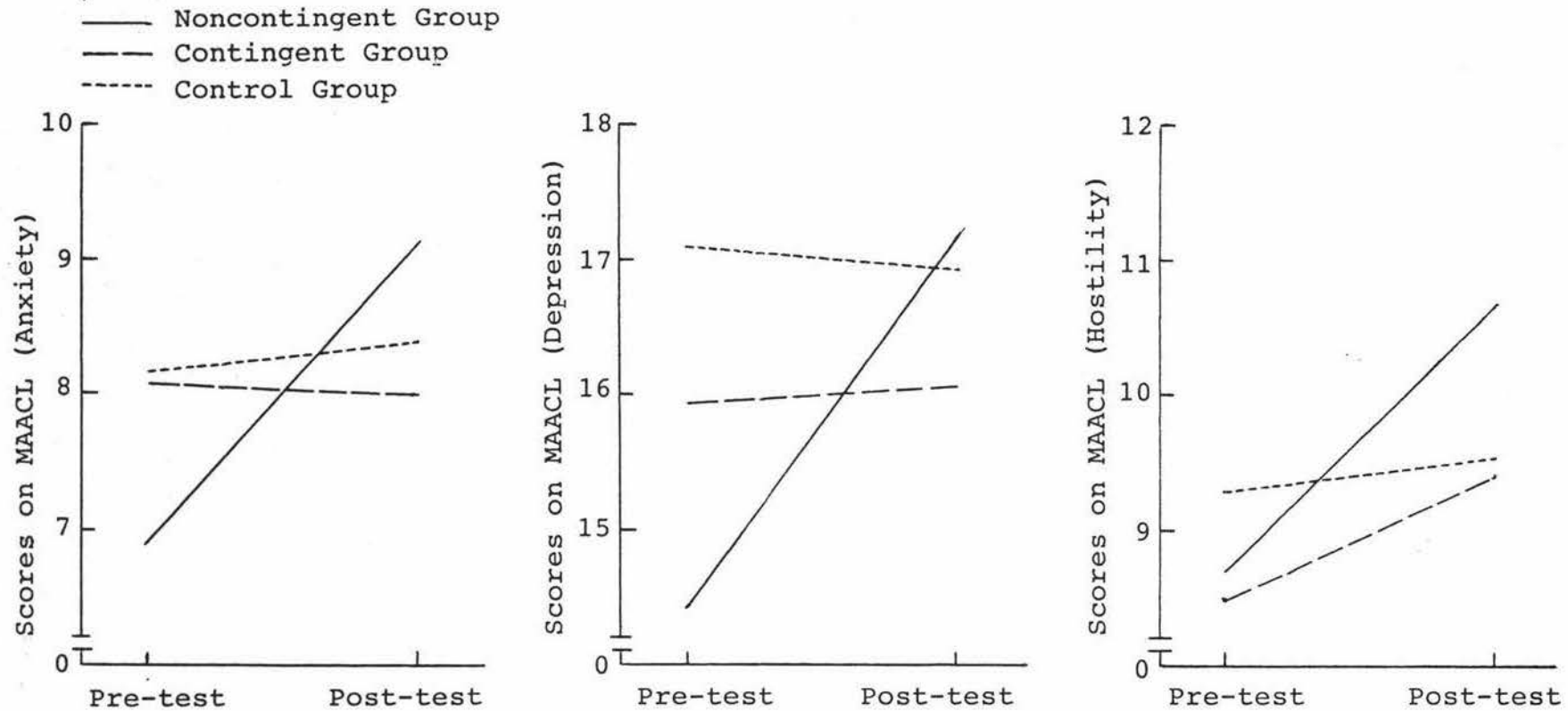


Figure 1. Pre and post-test MAACL scores for Anxiety, Depression and Hostility scales.

Note. The problem of ceiling effects when comparing pre and post-test scores did not apply in the present study. It was possible for noncontingent and control groups to have shown the same increase as the noncontingent group.

of the three affective scales. Ratings for contingent and control subjects are shown to be relatively stable between pre-test and post-test.

For Anxiety, collapsing across pre and post-test scores, no significant differences were found between groups,  $F(2,87) = 0.076$ . The same pattern was found for Depression,  $F(2,87) = 0.412$  and Hostility,  $F(2,87) = 0.488$ .

On all three scales significant increases were found to exist between pre and post-test scores. Collapsing across groups: Anxiety,  $F(1,87) = 6.603$ ,  $P = 0.0115$ ; Depression,  $F(1,87) = 4.55$ ,  $P = 0.0335$ ; and Hostility,  $F(1,87) = 13.523$ ,  $P = 0.0007$ .

Significant interactions between groups and pre and post-test scores were obtained for Anxiety,  $F(2,87) = 5.361$ ,  $P = 0.0066$ , and Depression,  $F(2,87) = 4.760$ ,  $P = 0.0109$ . The interaction for Hostility approached significance,  $F(2,87) = 2.908$ ,  $P = 0.0583$ . Within these interactions a t-test for related samples was used to test the hypothesis that changes in the noncontingent group would be significant while those in the contingent and control groups would not. A related samples t-test was chosen to avoid the danger of using an inappropriate error term in planned comparisons with a repeated measures analysis (See Keppel, 1973, p 409). There was no significant increase in Anxiety for either the contingent,  $t(29) = -0.1499$ , or the control group,  $t(29) = -0.409$ . A significant increase in Anxiety was obtained for the noncontingent group,  $t(29) = 4.431$ ,  $p < .0005$ .

A similar pattern of results was obtained for the Depression scale, with nonsignificant differences emerging between pre and post-test scores for the control group,  $t(29) = -0.357$ , and for the contingent group,  $t(29) = 0.1834$ . A significant increase in depression was demonstrated by the noncontingent group,  $t(29) = 2.897$ ,  $p < .005$ .

A slightly different pattern of results emerged for Hostility. Unexpectedly the control group showed a significant increase,  $t(29) = 2.12$ ,  $p < .025$ . The noncontingent group also showed a significant increase, although this was expected,  $t(29) = 3.652$ ,  $p < .0005$ . No significant differences existed for the contingent group,  $t(29) = 0.502$ .

As predicted, subjects from the non-contingent group showed increased levels of emotionality after exposure to uncontrollable outcomes. These increases were evident on all three affective scales. No change was noted for the control group, except for an increase in hostility. The contingent group similarly showed no changes in emotionality.

A three factor design (Keppel, 1973) and three dependent measures were used for both hypothesis 2 and 3. Of the three dependent measures (Time = total speaking time, Number = number of utterances, Probability = probability of speaking) results for only two are presented in the section.

As Probability was derived from Time, the results for Time are presented in Appendix K. A 3 (group) x 2 (sex) x 2 (time) analysis of variance was calculated on each dependent measure. The sex factor was included as a control factor. (See Appendix L for summary tables of these ANOVA'S).

To test the hypothesis that noncontingent subjects would manifest performance deficits during the test phase relative to other groups, a planned one by two orthogonal comparison was calculated on each dependent measure. No significant differences were found for Number,  $F(1,78) = 0.0405$ , or Probability,  $F(1,78) = 0.000$ .

These results fail to support the hypothesis that noncontingent subjects would show performance deficits relative to contingent and control subjects on the test task.

The nonsignificant influence of groups and the nonsignificant interaction between groups and time suggested the third hypothesis would not be supported. The hypothesis was still retained and tested as there was a possibility that the non-delay group may have shown a performance deficit, and hence evidence of generalisation. Planned comparisons were used to assess whether the time delay had any influence on the subsequent performance of the non-contingent group. These contrasts revealed no significant differences for Number,  $F(1,15) = 0.0081$ , or Probability,  $F(1,15) = 0.6048$ .

These results show that for noncontingent subjects, the time delay, did not significantly influence subsequent performance.

Analysis of the dependent measure, Number, yielded an unexpected main effect for the time delay,  $F(1,89) = 6.113$ ,  $P = 0.016$ , collapsed across groups. Subjects who experienced a time delay showed facilitated performance ( $\bar{X} = 19.43$ ) compared to subjects for which there was no such delay ( $\bar{X} = 14.67$ ). This result is further considered in the discussion section.

Post hoc analyses were undertaken to investigate the effectiveness of experimental manipulations. The effectiveness of the importance manipulation was assessed by comparing the importance rating of subjects exposed to this manipulation (contingent and noncontingent), to the ratings of control subjects who were not exposed to this manipulation.

The effectiveness of the importance manipulation was assessed by means of a one way analysis of variance to test for differences between the mean importance rating of these subgroups. This procedure was unavoidably confounded by the fact of differing pretreatment tasks for these groups. No significant differences were found between treatment means  $F(2,87) = 0.381$ . (See Appendix M for a summary table of these values.) This result suggests that the manipulation which was intended to increase subjects ratings of the importance of the training task, failed to do so.

The relationship between subjects rating of importance of the training task and subsequent performance was assessed by computing Pearson correlation coefficients. These correlations were based on contingent and noncontingent subjects ratings of importance and subsequent performance on the three dependent measures. The correlations were; Importance and Number,  $r(58) = -0.1173$ ; Importance and Time,  $r(58) = 0.0072$ ; Importance and Probability,  $r(58) = 0.0286$ . None of these correlations were statistically significant. This suggests that the importance ratings bore no relationship to subsequent performance.

The effectiveness of the attributional manipulation was assessed by means of a planned one by three comparison. This contrasted noncontingent subjects attributions to Ability, with ratings to Effort, Task Difficulty, and Luck. The contrast failed to show a significant difference,  $F(1,30) = 2.7817$ , suggesting that the attribution manipulation was ineffective. Contrary to expectations noncontingent subjects attributed their failure on the training task less to Ability than to Effort, Luck or Task Difficulty.

A similar pattern of results was obtained for contingent subjects. Although the difference was in the expected direction it failed to reach significance on the one by three contrast,  $F(1,30) = 0.3444$ . (See Appendix N for summary table of these two ANOVAS, group means and standard deviations).

## Discussion

The results of the present study demonstrate that noncontingent subjects showed increases in Anxiety, Hostility and Depression, following exposure to uncontrollable outcomes. As expected contingent subjects showed no changes on any of the scales. Unexpectedly control subjects, showed an increase in Hostility.

The major finding of the study was that helplessness failed to generalise to the test task, where noncontingent subjects were expected to display performance deficits. There were no differences on any performance measures between noncontingent subjects and either contingent or control subjects.

Results failed to support the third hypothesis. Noncontingent subjects who experienced a delay prior to the test task did not display less debilitated performance on the test task compared to noncontingent subjects who did not experience a time delay. There was an unexpected main effect for the time delay on the dependent measure, Number (number of utterances). Subjects who waited prior to the test task showed facilitated performance compared to subjects who did not experience this wait, (collapsing across the experimental groups).

The noncontingent subjects increased Anxiety, Hostility and Depression was expected. Helplessness theory (Seligman, 1975) predicts only increases in depression. However other studies (Gatchel, et al., 1975; Roth & Kubal, 1975; Miller & Seligman, 1975; Griffith, 1977, Noel & Lisman, 1980) have all reported increases on at least two of the affective scales. As discussed earlier this finding was taken as confirmation of a successful helplessness induction.

As expected the contingent group showed affective stability between pre-test and post-test. The control group showed similar stability on only two of the three scales, showing an increase on the Hostility scale. Although unexpected, this is explicable when the procedure used for control subjects is considered. After volunteering for what they thought was an experiment in learning, they found their task was in fact to just flip through ten stimulus cards. Following approximately ten minutes of this activity the experimenter returned, with the request that they repeat the same rating scale (MAACL). The increase on the Hostility scale may have reflected a sense of growing antagonism to the apparent meaninglessness of their participation . Although this increase is not of theoretical importance, it does serve as testimony to the sensitivity of the MAACL.

The major finding of the present study was that helplessness failed to generalise to the test task, which unlike most previous studies was radically different from the training task. If helplessness had generalised then noncontingent subjects would have displayed performance deficits, relative to control and contingent subjects. No such deficits were evident.

As was argued in Chapter One, the issue of cross-situational generalisation is crucial to the theory of learned helplessness. Learned helplessness may be regarded as an inappropriate generalisation from a situation where outcomes are uncontrollable, to a situation in which they are controllable. The extent to which these situations are similar however, reduces the inappropriateness of this generalisation. Accordingly, studies which have failed to separate the two situations may not have demonstrated learned helplessness, but rather a form of learned scepticism on the part of the subjects. The deficits which were noted in the test phase of such studies may not have been a function of subjects' belief in noncontingency, but due to subjects' growing scepticism regarding the experimenter and the experimental procedures.

In attempting to discount the likelihood of the learned scepticism explanation and provide an adequate test of the generality of learned helplessness, the current study clearly separated the training and test phases of the experiment.

Yet the major finding of this study was the failure of learned helplessness to generalise. This lack of generality together with similar evidence (Cole & Coyne, 1977; Tiggemann & Winefield, 1978), raises doubts concerning the theoretical predictions (Seligman, 1975; Abramson, et al., 1978) of high generality. In failing to confirm these predictions the current result suggests that when training and test phase are adequately separated, learned helplessness does not readily generalise across situations. This emphasises a theoretical weakness, the theory lacks predictive ability regarding cross-situational generalisation.

This finding also questions the assumption that helplessness tends to be a relatively stable 'trait-like' condition. The implication of the current finding is that helplessness is a situation specific phenomenon. When the training and test task are different and when these two phases are conducted in dissimilar situations, learned helplessness fails to generalise. This result must be regarded as strong evidence against Maier and Seligman's (1976) belief in generalised response initiation.

The lack of generalisation shown by this study also has important implications for the notion that learned helplessness may underlie reactive depression. The learned helplessness model of depression argues that clinical depression is a behavioural pattern characterised by

situationally generalised helplessness. At the basis of this model is the notion of repeated helpless experiences, resulting in a generalised trait. The present finding must be regarded as a contradiction to the helplessness model of depression since it demonstrates the lack of cross-situational generalisation of the helplessness effect.

A limitation of this contradiction also warrants discussion. Although the present study showed that helplessness failed to generalise across situations, this failure involved only two situations - training and test. Depression may in fact result from multiple exposures to helplessness provided these inductions occur in sufficient number and in a sufficient variety of differing situations. These differing situations may also need to be important to the person. Experimentally this poses an empirical question concerning the effects of multiple helplessness inductions (each occurring in a different situation). On a practical level this possibility suggests that becoming financially helpless (unemployed) and also becoming helpless with regard to ones love life, may simultaneously predispose a person toward depression.

A number of therapeutic implications arise from the learned helplessness model of depression. One suggestion is that the therapist attempts to change the individuals expectations from uncontrollability to controllability.

This therapeutic strategy is based on the assumption that such expectations are fundamental to the etiology of depression. Secondly, and more importantly, it assumes the wide generality of such expectations. The current study, bearing in mind the above qualification, suggests this assumption may be unjustified.

No evidence of helplessness was found in the current test task. This result must be viewed as contradicting the wide generalisation implied by some authors (Hiroto and Seligman, 1975). It must also be regarded as failing to support one of the crucial predictions of learned helplessness theory. It seems unlikely that helplessness may possess the rudiments of a trait, and yet fail to show cross-situational generalisation. The principal implication of the present study is that claims for the wide generality of the phenomenon should be regarded cautiously. Furthermore, as the learned helplessness model of depression assumes wide generality, this model fails to receive support from the present study.

The dissipation of the effects of learned helplessness over time has been predicted by Seligman (1975) and Abramson et al., (1979). Noncontingent subjects who waited prior to the test task, did not show less debilitated performance than noncontingent subjects who experienced no wait. A problem arises, since as a group noncontingent subjects did not display any deficits in performance relative to contingent and control subjects. This fact reduces the likelihood of there being a dissipation of deficit (since there was no deficit shown).

However it was still possible for the time delay, noncontingent subjects to outperform their non time delay counterparts, and yet for the noncontingent group as a whole, not to perform markedly differently from control and contingent groups.

The unexpected main effect for time delay (on the dependent measure, Number) is in stark contrast to the result obtained for the two groups of noncontingent subjects. This main effect indicated that time delay subjects showed facilitated performance on the test task when compared to non time delay subjects. Interestingly the effect was predicted for noncontingent subjects, but was not supported by the results. Their performance was somewhat contrary to the rather unexpected tendency which emerged. For control and contingent subjects the effect of the time delay was to facilitate performance. (See Appendix O for relevant cell means).

This result may reflect the annoyance which subjects possibly felt at being kept waiting, resulting in an increased motivational state. This explanation fails, however, to explain why a similar effect was not apparent for the noncontingent subjects who similarly experienced the time delay.

The effectiveness of the experimental manipulations was assessed by a series of post hoc analyses. The importance manipulation at the start of the training phase was designed to increase the importance which subjects attached to the training task. Results of the post-experimental

importance ratings showed no significant differences between either of the groups that received this manipulation, or the control group. In fact the mean rating of importance for subjects who received this manipulation was identical to the mean importance rating of the control group. If these ratings accurately reflect subjects views, then it would appear that subjects attached no more importance to the training task as a result of the experimental manipulation. The current importance rating was apparently unsuccessful.

Roth and Kubal (1975), found increased helplessness resulted from increasing the importance of the training task. The present study's apparent failure to increase importance may explain the lack of generalisation evident in the current study. If importance of the training task is crucial for a strong helplessness effect, the lack of importance assigned to the task by subjects, may have lessened the effect of the helplessness induction. A further possibility also exists, in that noncontingent subjects may have attempted to lessen the impact of their failure, by reducing the importance of the task on the post-experimental questionnaire. Unfortunately there is no way of assessing the extent to which such strategies on the part of noncontingent subjects account for low ratings of importance.

Correlation coefficients were computed for the three pretreatment groups, between importance ratings and subsequent scores on the three dependent measures. None of the correlations approached statistical significance. This strongly suggests that importance ratings bore no relationship to performance in the test task. On the basis of Roth and Kubal's (1975) study, importance ratings may have been expected to correlate negatively with scores on the dependent measures, (for noncontingent subjects). No significant correlations were noted for the noncontingent group. This result casts some doubt on the effect which importance has for increasing the likelihood of helplessness deficits.

Attributional manipulations designed to encourage subjects to attribute their performance to stable, internal, global factors were apparently ineffective. The results suggested that attributions were made no more to Ability, than to Effort, Task Difficulty or Luck. The Abramson et al., (1978), reformulation predicts that causal attributions made by subjects, determine the chronicity and generality of helplessness deficits. The results suggest that attempts to manipulate such attributions were unsuccessful. This apparent failure may explain the lack of generalisation.

A bias working against the attributional manipulation is discussed by Zuckerman (1979). In discussing attributions of success and failure he reviews evidence suggesting that

people are more likely to make internal attributions for success than for failure. The current attribution manipulation was an attempt to manipulate noncontingent subjects attributions in a direction counter to this bias.

Another question concerns whether people actually make attributions. Deiner and Dweck (1978) regard the spontaneous occurrence of attributions as a critical issue. Wortman and Dintzer (1978) question the wisdom of accepting at face value, the assumption that people make attributions. They note that subjects may often be cued by psychologists scales and questionnaires. Hanusa and Schulz (1978) provided subjects with the opportunity for free response, and subjects failed to spontaneously report attributions for success or failure. This point coupled with the attributional bias raises questions about the validity of subjects' responses to the post experimental questionnaire. Perhaps these responses should be taken *cum grano salis*.

A further point concerns the inconsistency between causal attributions and their relationship to subsequent behaviour. Baum and Gatchel (1981) discuss this inconsistency and cite studies where the opposite effect to that predicted, on the basis of an attributional analysis, occurred.

In summary, the attributional manipulations were apparently unsuccessful. This lack of success may have been due to a defensive bias on the part of subjects, or inadequacies in the instrument used to assess the effectiveness of the manipulations. If in fact attributions were not successfully manipulated then this failure may help explain the lack of generalisation.

The partial debriefings conducted with noncontingent subjects following the test task were recorded on video. Subsequent analysis of these proved interesting in terms of understanding how subjects viewed their poor performance, the reasons for it and what it meant to them. Perhaps the most revealing aspect of these interviews was the large number of subjects (21 out of 30) who verbalised their suspicions concerning the experiment. A variety of cues were mentioned by subjects which alerted suspicions. These included the very fact that it was a psychology experiment, and other aspects of the experiment which subjects perceived as demand characteristics e.g. one subject commented, " I had a feeling when you showed me the graph that I was going to get them all wrong." (See Appendix F for the graph). Others were more logical, arriving at the conclusion that the training task was rigged because they had logically explored all possibilities and yet had still failed the problems.

Subjects suspicions were often voiced after the deception had been revealed to them. The extent to which this suspiciousness was merely post hoc wisdom is unknown. Often subjects responded to information concerning the impossibility of the task with a comment suggesting that they thought there was something "funny" going on. Yet these suspicions were often not expressed earlier when the opportunity was given. The apparent high degree of suspicion may have reduced the impact of the helplessness induction. Subjects may have acquired a 'learned suspiciousness' which contaminated the effectiveness of uncontrollability. The doubts expressed by Lazarus et al., (1952) are perhaps worth reviewing. Specifically they regarded the 'failure' design as ambiguous, leaving experimenters uncertain of what subjects actually respond to. The partial debriefings suggest that subjects may well have been aware of having been deceived, and viewed some aspects of the procedure as demand characteristics.

An interesting aspect of these debriefings arose when subjects were given the opportunity to succeed at the training task which they previously had failed. An air of reluctance marked this phase of the debriefing. A number of subjects expressed doubts about their ability to succeed and were very reluctant to attempt them again. The experimenter assured them of contingent feedback and of the simplicity of the task, but the reluctance persisted in some cases. One subject refused totally to have anything more to do with the tasks. Another explained her reluctance by commenting

" this will really show how dumb I am." Yet another, "I'm petrified I won't be able to do it." A few subjects (5) failed to successfully complete the task and were given a coaching assisted second trial which proved successful in all cases. These results suggest that helplessness, for some subjects, may have generalised across situations but was task specific.

Noncontingent subjects (who did not voice suspicions) mentioned a variety of 'reasons' for their failure on the training task. Among these reasons were : lack of concentration; being confused by the task; feeling tired; and doubting their own ability. Contrary to the failure attributional bias these reasons were largely internal. They may also be characterised as unstable and task specific, and according to the Abramson et al. (1978) reformulation, predictive of low generality.

A possible criticism of the study might be that the pretreatment task may have been insufficient. The current design accepted changes on the MAACL as evidence of the sufficiency of the pretreatment. A test of the adequacy of the pretreatment would have involved providing a fourth experimental group and using a traditional anagram task to test for deficits. This was not done for two reasons. Firstly this would have represented yet another replication of an effect which has already received multiple demonstrations.

Secondly such a test would have required resources beyond what were available. Also a number of experimental manipulations were included (importance and attributional manipulations) in order to strengthen the effectiveness of the pretreatment task.

The different nature of the test task in the present research warrants comment. To date a variety of test tasks have been used. These include : escape avoidance tasks, (Hiroto, 1974); anagram solution tasks, (Benson & Kennelly, 1976); intelligence tests, (Thornton & Jacobs, 1972); block design, (Dweck & Reppucci, 1973); digit letter substitution, (Dweck & Bush, 1976); discrimination learning, (Eisenberger, Park & Frank, 1976); and concept-formation tasks (Roth & Bootzin, 1974). Although a range of tasks have been used these have all been cognitive in nature. The test task in the present research had a cognitive component, but was primarily social in nature. The task occurred in a group setting, within which individual performance was the focus for the dependent measures. Unlike the anagram task which has been validated repeatedly (e.g. Hiroto, 1974; Hiroto & Seligman, 1975; Miller & Seligman, 1975; Winefield & Tiggemann, 1978), the current task had not previously been used. This was regarded as a strength of the study, and fundamental to the adequate assessment of the question of cross-situational generalisation. The task was regarded as a reasonable one, providing opportunity for performance deficits to manifest

themselves. In fact some subjects were observed sitting passively, contributing little.

A criticism may be that this study confounded test task and situation. In attempting to demonstrate cross-situational generalisation, this type of confound may have been avoided by separating training phase and test phase, but using a similar type of task. The argument against this is that in order to provide a test situation which was truly dissimilar from the training situation it was important to provide a test task which was also dissimilar from the training task. In this regard the test task was viewed as part of the wider test situation.

The current study has demonstrated the failure of learned helplessness to generalise across situations. Two recommendations stem from this finding. Firstly that assertions that helplessness generalises widely or possesses the strength of a trait or state, be modified in the light of recent evidence. Secondly that future researchers direct themselves to further delineating the exact conditions under which helplessness may generalise. This will involve determining which situational aspects serve as the most powerful discriminative cues in controlling cross-situational generality. The study by Dweck and Repucci (1973) offers an important lead in this respect. They found differential impairment in children's problem-solving abilities dependent on the identity of the task administrator.

Seligman (1975) notes discriminative control as being a possible limitation on the generality of helplessness. The parameters of these limitations warrant further research.

The mixed success accorded to the role of attributions in predicting the effects of learned helplessness, suggests that this area requires considerable research in order to clarify the role of attributions. Before the reformulated model may be adequately tested some fundamental questions concerning attributions will need to be answered. The current study's failure to successfully manipulate such attributions may well be a starting point from which to ask such questions as : are peoples attributional styles open to situational manipulation? Do people make spontaneous causal attributions? If so, how do those attributions relate to subsequent behaviour? Another research question also surrounds the accurate assessment of attributions and the problem of such assessments reflecting positive self-presentation on the part of subjects.

A review of the debriefing tapes provided evidence for the obvious influence of individual differences, which have a strong bearing on individual reactions to the induction of helplessness. The present study attempted (unsuccessfully) to manipulate the importance which subjects attached to the outcome which was uncontrollable. Other characteristics of the outcome may also have a bearing on the effectiveness of induced helplessness. The meaningfulness of the outcome may be one such variable worth investigating. Another variable

may be the extent to which subjects expect to be able to control outcomes. For example, we lack control over the weather, but we similarly do not expect control in this domain. Such expectancies may influence the magnitude of subsequent deficits.

## Conclusion

Predictions of high generality were not supported by the present findings. This study provided a clear demonstration that when training and test phase are adequately separated, learned helplessness fails to generalise across situations. This result was used to argue for the situation specificity of the helplessness phenomenon. This evidence also called for a tempering of the assumption that helplessness be regarded as a trait-like condition. Without evidence for wide generality the significance of the phenomenon may be regarded as questionable.

The lack of cross-situational generalisation raised questions concerning the expedience of the helplessness model of depression. This model regards depression as a behavioural pattern characterised by situationally generalised helplessness.

Finally the study attempted a first step in the gradual process of the reapproximation to the conditions of 'life', (See p.27 ). This step was not taken without faltering.

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## Appendix A

CONSENT FORM FOR EXPERIMENTAL SUBJECTS

We are requesting the help of Introductory Psychology students to assist, by volunteering as subjects, in two separate experiments.

The two experiments are to be run consecutively, and would require approximately one hour of your time.

You will be contacted sometime before the end of the year (prior to final exams) at which time we will arrange a time which is convenient for you.

Your support and involvement will be greatly appreciated.

NAME: \_\_\_\_\_

AGE: \_\_\_\_\_

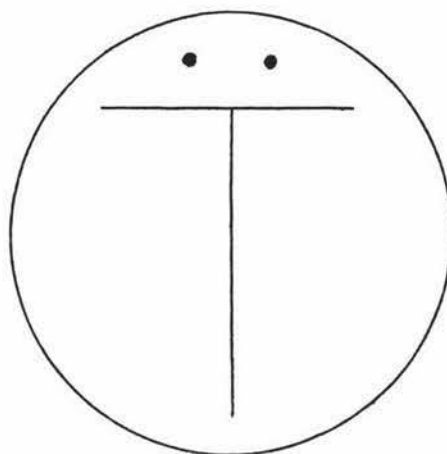
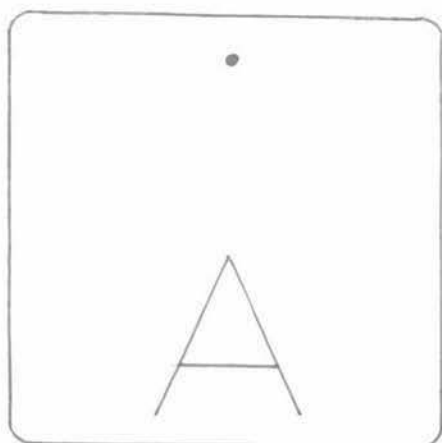
SEX: \_\_\_\_\_

TELEPHONE NO. \_\_\_\_\_

In completing the details of this form I hereby volunteer to act as a subject for the experiments referred to above.

\_\_\_\_\_  
SIGNATURE

## Appendix B

An Example of a Stimulus Card Used for Pretreatment

## Appendix C

Post Experimental Questionnaire

For each question circle the number which most accurately reflects your answer.

1. How important was it for you to perform well in Experiment 1.?

UNIMPORTANT 1 2 3 4 5 6 7 VERY IMPORTANT

2. Rate the following factors in terms of their influence on your success/failure on the problems in Experiment 1.

TASK DIFFICULTY

NO INFLUENCE 1 2 3 4 5 6 7 STRONG INFLUENCE

LUCK

NO INFLUENCE 1 2 3 4 5 6 7 STRONG INFLUENCE

ABILITY

NO INFLUENCE 1 2 3 4 5 6 7 STRONG INFLUENCE

EFFORT

NO INFLUENCE 1 2 3 4 5 6 7 STRONG INFLUENCE

Criteria for Determining a Unit of Speech

1. Must be verbal in nature - excluded non-verbal gestures (nods, frowns etc.)
2. Must express at least one complete thought, i.e., contain a subject and a predicate which, if not actually present, was at least implied. For example, the comment "yes" would be recorded, but the comment "well, uh" would not.
3. A speech unit was an utterance of speech which was unbroken by a lengthy pause or by an acknowledged interruption.
  - a) If the speaker paused for less than three seconds, and if no other member spoke during that pause, then the persons overall contribution was recorded as a single comment. If the speaker paused for three seconds or longer, he was considered as having made two separate comments.
  - b) An interruption by another member was not recorded if the interruption was not acknowledged by the speaker or the remainder of the group.

## Rules for Determining the Identity of the Speaker.

1. Only one person ordinarily was considered to speak at one time.
2. If two persons spoke simultaneously only the dominant comment was recorded. Dominant meant the comment which eventually won the attention of the majority of the group, or changed the direction of conversation.

Rationale and Procedure for Obtaining Probability Measure

The scores for noncontingent subjects were dependent on the scores of control and contingent subjects. This derivative overcomes the problem of comparing scores across the three experimental conditions, by expressing a subjects time score as a function of the time made available to him/her after the rest of the groups contribution had been subtracted. An example may illustrate; two subjects from different experimental conditions and from different social groups (test task) obtain identical Time scores. Before any meaningful comparison can be made these scores must be viewed in relationship to the social group from which they originated. One score may have come from a 'quiet' social group where discussion did not flow easily and there was no shortage of opportunities to speak, while the second may have come from a 'chatty' group where few such opportunities were presented.

The third dependent measure was the probability that the subject talked during the available time. Thus  $PROBABILITY = \text{observed time for subject "i"}, \text{divided by the maximum possible time for subject "i"}$ . Since each group had 600 seconds of available time the maximum possible time =  $600 \text{ seconds} - (\text{total time for group} - \text{time for subject "i"})$ . Thus the PROBABILITY measure accounts for the interdependent nature of the social groups from which the dependent measures were derived.

Instructions to Subjects

"This is an experiment in learning.

"The task you will be asked to do serves as a very good indicator of intelligence and academic potential. The task is also a good predictor of how well people perform on other psychological experimental tasks.

"Your task involves solving 4 problems.

"These problems have been used in past years and a graph on this research report shows the results for the past 3 years." (This graph is included in this Appendix).

"In this experiment you will be looking at cards like this one. Each card has two stimulus patterns on it. The patterns are composed of 5 different dimensions and two values associated with each dimension. The dimensions and their values are (point to demonstration card) : Letter - A or T; Colour - Red or Black; Letter Size - Large or Small; Border - Square or Circle; Dots - 1 or 2. Each stimulus pattern has one value from each of the five dimensions.

"I have arbitrarily chosen 1 of the 10 values as being correct. For each card I want you to choose by pointing to the side (Left or Right) which contains this value, and I will tell you if your choice was correct or incorrect. In a few trials you can learn what the correct value is by this feedback. The object is for you to figure out what the answer is so you can choose correctly as often as possible. Remember just choose one side of the card by pointing."

Two sample trials were then given to clarify the task of finding the correct value.

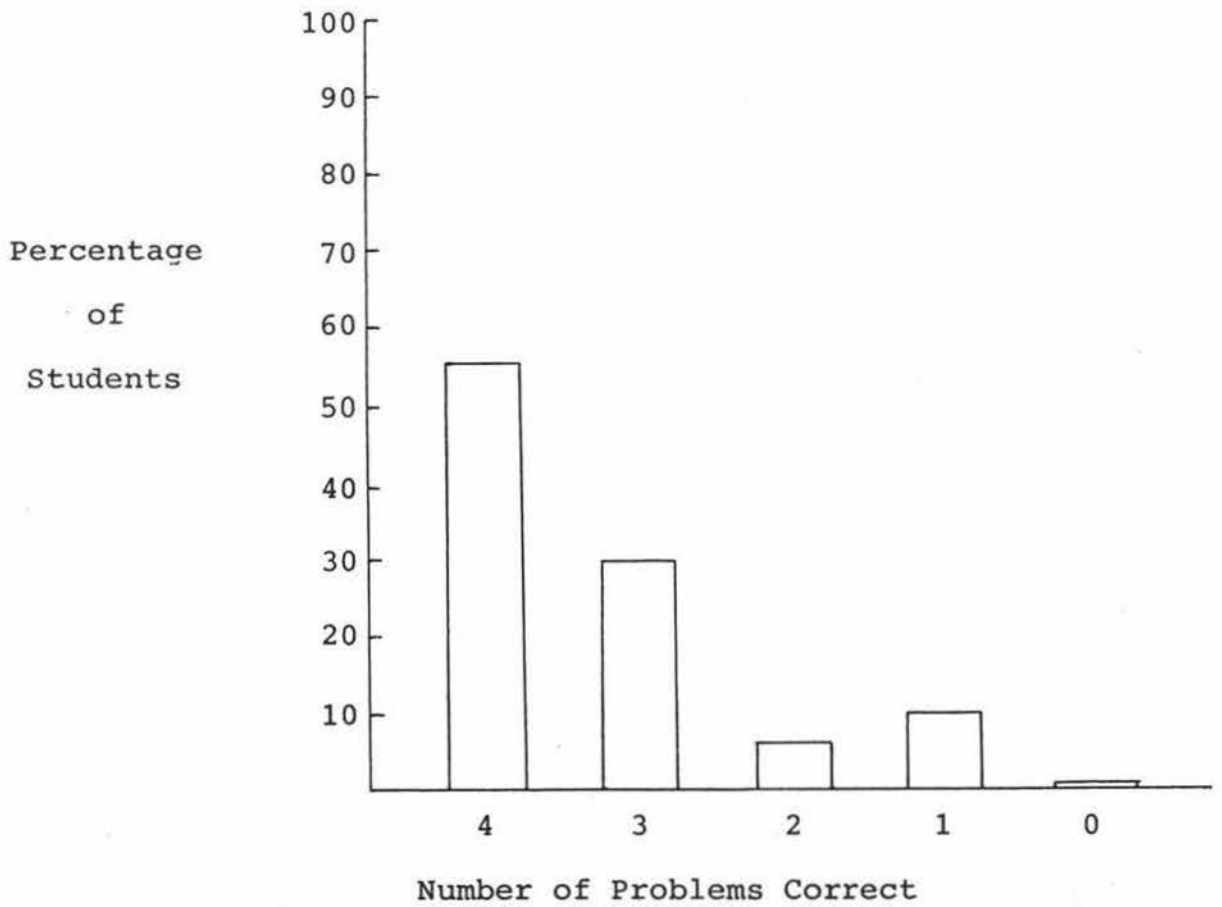
Subjects were then asked "are there any questions?"

At the end of each trial of 10 cards, subjects were asked "what is the correct answer?" The contingent subjects were given veridical feedback at this point, while the noncontingent subjects were all told "that is the wrong answer." All subjects were then told "We are now starting a new problem. You do not know at this point if I have chosen a different value for this problem. I will continue telling you if you are correct or incorrect."

Figure Used in Conjunction with Instructions to Subjects

Figure 1.

Bar Graph Showing Percentage of  
Students for Each Number Correct



Front Page Planning

In this exercise you are to imagine that you are a member of the Editorial Board of a large daily newspaper (printed in Wellington) and you have ten items for the front page of the morning edition. In order to allocate space appropriately to the various items, you and your colleagues must agree on their order of importance as news. This Board is democratic, and so each ranking must have the support of the majority of members. A clear majority is required. Remember that you must argue to convince the majority of your colleagues that your ranking is the right ranking. You are in the role of a Board member, and you must make your views felt. The Board can produce only one order.

- A. U.K. jobless total tops 3 million
- B. Primary schools to close for stopwork meetings
- C. P.M. Hospitalised
- D. Major Flooding in Southland
- E. German airliner crashes in Argentine, twenty killed
- F. T.A.B. robbery, manager killed by gunblast
- G. T.V. Licence fees to double in 1982
- H. U.S. & U.S.S.R. disarmament talks to resume
- I. Early morning quake shakes capital
- J. 18 year old dies of rugby injuries

It is expected that this task will take 15 minutes.

Instructions to Subjects for Test Task

Read in conjunction with copy of "Front Page Planning."  
(See Appendix G).

1. "I'm studying the processes involved in group decision making. These instructions further explain your task."
2. A copy of "Front Page Planning" was handed to subjects.
3. "I'm not so much interested in you completing the task, as observing the processes you go through as a group. The orange light will flash five times and then stop. When it stops could you please begin the task."

Debriefing Notes

To everyone who kindly volunteered for and participated in "Two Research Studies to be run consecutively, the first a learning experiment and the second concerned with group processes."

I would like to take the opportunity to thank you again for your co-operation and help. There were deceptions involved in this research. The purpose of these notes is to reveal what the deceptions were and why they were necessary. The "two studies" were in fact part of one experiment testing the generality of learned helplessness. Recent research in this area has shown that learned helplessness may have applications in many areas. Aside from the theoretical questions concerning the nature of learned helplessness, psychologists have found that learned helplessness may have practical relevance for problems of depression, anxiety, failure, ageing and death. I believe that research which may increase our understanding in these areas is vitally important and therefore goes a long way toward justifying the deceptions involved in this research.

As I want you to be fully debriefed and because I feel that you may have an interest in the research the hypotheses and method for this study are outlined below.

The experiment was run in two phases, (these corresponded with what you believed were the two experiments). Subjects were run in groups of six, arriving in threes about 15 minutes apart. Prior to arrival subjects were randomly assigned to one of three experimental conditions. The three conditions were: (you should be able to recognise the one which applied to you).

1. Contingent reinforcement : subjects were given the task of solving four problems. Contingent (true) feedback was given to assist in the solving of the problems.
2. Noncontingent reinforcement (helplessness condition) subjects were given the task of solving four problems. Subjects were led to believe that they would receive contingent (true) feedback that would assist in finding solutions. In fact they were given noncontingent (false) feedback, thus making it impossible for them to solve the problems. The two subjects (out of each group of six) who were exposed to this condition were always asked to wait behind for a minute at the conclusion of the group task. These two subjects were then debriefed about their noncontingent reinforcement and also given an opportunity to solve the problems.

Prior to being given the problems subjects from both contingent and non-contingent conditions were told that the problems served as good indicators of "intelligence and academic potential". Subjects were told that the

task was a "good predictor of how well people perform on other psychological experimental tasks". Subjects were also shown a bar graph which suggested that most students were able to solve either all the problems or make one error. These deceptions were designed such that the "helpless" subject would attribute his failure to solve the problems to internal factors, such as his own inadequacy to solve problems rather than external factors such as the difficulty of the problems

3. Control : subjects were given the task of looking through the cards which were used in both the other two conditions.

Subjects from all three conditions were asked to complete an adjective check list before and after the task. The check list is sensitive to changes in mood (anxiety, depression and hostility) and was used to see if the helpless subject displayed any increases on these three scales.

The second phase of the experiment was pretty much as it seemed. The group interaction was recorded on video tape for subsequent analysis. You were led to believe that the experimenter was interested in the process involved in group decision making, but all we were interested in was providing subjects with the opportunity of talking for a standard time (10 minutes).

The basic hypothesis of the experiment was that the helpless subjects would show performance deficits on the second task, i.e. would talk less, and would also show increases in their ratings of anxiety, hostility and depression. The experiment was designed as a test of the generality of the helplessness phenomenon - to see if the deficits normally associated with a state of helplessness would also be observed on a second, unrelated task, run by a different experimenter and advertised as a completely independent experiment.

If you want any further clarification or explanation with regard to this experiment then please don't hesitate to contact me.

I can be reached by phone at home, 76-234. During the day I may be located in Room P.110 on the ground floor of the Psychology building.

Thanks again,

KEITH TUFFIN.

Summary ANOVA Tables for MAACL Scales

A = Experimental group, B = Pre-test/Post-test

Summary ANOVA Table for Anxiety

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Between Subjects					
A	2.8789	2	1.4395	0.076	
Error Between	1641.6172	87	18.8692		
Within Subjects					
B	28.0059	1	28.0059	6.603	0.0115
AB	45.4766	2	22.7383	5.361	0.0066
Error 2	369.0176	87	4.2416		

Summary ANOVA Table for Depression

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Between Subjects					
A	51.0781	2	25.5391	0.412	
Error Between	5393.0938	87	61.9896		
Within Subjects					
B	38.2813	1	38.2813	4.555	0.0335
AB	80.0078	2	40.0039	4.760	0.0109
Error 2	731.2109	87	8.4047		

Summary ANOVA Table for Hostility

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Between Subjects					
A	16.5332	2	8.2666	0.488	
Error Between	1473.2676	87	16.9341		
Within Subjects					
B	49.0879	1	49.0879	13.523	0.0007
AB	21.1113	2	10.557	2.908	0.0583
Error 2	315.8008	87	3.6299		

MAACL Scores, Means and Standard Deviations

Standard deviations appear in parentheses

Anxiety Scale

	<u>Pre-Test</u>	<u>Post-Test</u>
Control Group	8.167 (3.643)	8.400 (3.105)
Contingent Group	8.067 (3.032)	8.000 (2.967)
Noncontingent Group	6.900 (3.986)	9.100 (3.197)

Depression Scale

	<u>Pre-Test</u>	<u>Post-Test</u>
Control Group	17.100 (6.074)	16.933 (5.983)
Contingent Group	15.933 (4.999)	16.067 (4.494)
Noncontingent Group	14.400 (7.163)	17.200 (5.913)

Hostility Scale

	<u>Pre-Test</u>	<u>Post-Test</u>
Control Group	8.500 (2.930)	9.433 (2.526)
Contingent Group	9.300 (3.504)	9.567 (2.717)
Noncontingent Group	8.733 (3.812)	10.667 (3.239)

Results for Dependent Measure: Time

## Hypothesis 2.

To test the hypothesis that noncontingent subjects would manifest performance deficits during the test phase relative to other groups, a planned one by two orthogonal comparison was calculated. No significant differences were found,  $F(1,78) = 0.0740$ . This result failed to support the hypothesis.

## Hypothesis 3.

To test the hypothesis that noncontingent subjects who experienced a time delay would display less debilitated performance on the test task, a planned comparison between these two groups was calculated. No significant differences were found,  $F(1,15) = 0.5057$ .

Summary ANOVA Table for Dependent Measure : Time

A = Group, B = Time, C = Sex

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Main Effects	3711.500	4	927.875	0.384	0.819
A	215.450	2	107.725	0.045	0.956
B	2689.600	1	2689.600	1.114	0.295
C	806.450	1	806.450	0.334	0.565
2-Way					
Interactions	13132.070	5	2626.414	1.088	0.374
AB	6346.350	2	3173.175	1.314	0.275
AC	5568.921	2	2784.460	1.153	0.321
BC	1216.800	1	1216.800	0.504	0.480
3-Way Interactions					
ABC	4211.575	2	2105.787	0.872	0.422
Explained	21055.156	11	1914.105	0.793	0.647
Residual	188348.094	78	2414.719		
Total	209403.250	89	2352.845		

Summary ANOVA Table for Dependent Measure : Probability

A = Group, B = Time, C = Sex

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Main Effects	0.017	4	0.004	0.137	0.968
A	0.001	2	0.000	0.011	0.989
B	0.016	1	0.016	0.504	0.480
C	0.001	1	0.001	0.021	0.886
2-Way					
Interactions	0.173	5	0.035	1.106	0.364
AB	0.064	2	0.032	1.018	0.366
AC	0.088	2	0.044	1.412	0.250
BC	0.021	1	0.021	0.670	0.416
3-Way Interactions					
ABC	0.048	2	0.024	0.767	0.468
Explained	0.238	11	0.022	0.692	0.742
Residual	2.435	78	0.031		
Total	2.672	89	0.030		

Summary ANOVA Table for Dependent Measure : Number

A = Group, B = Time, C = Sex

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Main Effects	761.325	4	190.331	2.276	0.069
A	38.850	2	19.425	0.232	0.793
B	511.225	1	511.225	6.113	0.016
C	211.250	1	211.250	2.526	0.116
2-Way					
Interactions	538.792	5	107.758	1.289	0.277
AB	294.817	2	147.408	1.763	0.178
AC	237.925	2	118.962	1.423	0.247
BC	6.050	1	6.050	0.072	0.789
3-Way Interactions					
ABC	218.608	2	109.304	1.307	0.276
Explained	1518.725	11	138.066	1.651	0.101
Residual	6522.773	78	83.625		
Total	8041.498	89	90.354		

Summary One-Way ANOVA Table for Importance Ratings

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Between	2.4001	2	1.2001	0.381	0.6843
Within	237.9997	87	3.1494		
Total	276.3998	89			

Summary ANOVA Table for Noncontingent Subjects' Attribution Ratings

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Between Subjects	118.2417	29	4.0773		
Within Subjects	259.7500	90	2.8861		
Between					
Measures	26.0250	3	8.6750	3.2291	0.0263
Residual	233.7250	87	2.6865		
Total	377.9917	119	3.1764		

Attribution Ratings, Means, and Standard Deviations for Noncontingent Subjects

	<u>MEANS</u>	<u>STANDARD DEVIATIONS</u>
1. Effort	4.6667	1.6470
2. Luck	3.6333	2.0759
3. Task Difficulty	4.4333	1.5013
4. Ability	3.6333	1.6914

Summary ANOVA Table for Contingent Subjects' Attribution Ratings

<u>Source</u>	<u>SS</u>	<u>df</u>	<u>MS</u>	<u>F</u>	<u>P</u>
Between Subjects	158.2417	29	5.4566		
Within Subjects	244.7500	90	2.7194		
Between					
Measures	52.2917	3	17.4306	7.8794	0.0001
Residual	192.4583	87	2.2122		
Total	402.9917	119	3.38865		

Attribution Ratings, Means, and Standard Deviations for Contingent Subjects.

	<u>MEANS</u>	<u>STANDARD DEVIATIONS</u>
1. Effort	4.6667	1.7622
2. Luck	2.9000	1.8251
3. Task Difficulty	4.2667	1.7798
4. Ability	4.1333	1.5698

## Appendix O

Means for Experimental Groups on Dependent Measure: Number

	<u>Time Delay</u>	<u>No Time Delay</u>
Control Group	21.67 (15)	13.73 (15)
Contingent Group	20.63 (15)	13.97 (15)
Noncontingent Group	16.00 (15)	16.30 (15)
Total	19.43 (45)	14.67 (45)