# DIFFERENCE OR MAKE-A-DIFFERENCE:
# A COMPARISON OF ACTUAL CONTROL METRICS

BY

KENNETH M. CRAMER

A Thesis

Submitted to the Faculty of Graduate Studies

in Partial Fulfillment of the Requirements

for the Degree of

DOCTOR OF PHILOSOPHY

Department of Psychology

University of Manitoba

Winnipeg, Manitoba

National Library of Canada
Acquisitions and Bibliographic Services Branch
395 Wellington Street
Ottawa, Ontario
K1A 0N4

Bibliothèque nationale du Canada
Direction des acquisitions et des services bibliographiques
395, rue Wellington
Ottawa (Ontario)
K1A 0N4

Your file Votre référence

Our file Notre référence

The author has granted an irrevocable non-exclusive licence allowing the National Library of Canada to reproduce, loan, distribute or sell copies of his/her thesis by any means and in any form or format, making this thesis available to interested persons.

L'auteur a accordé une licence irrévocable et non exclusive permettant à la Bibliothèque nationale du Canada de reproduire, prêter, distribuer ou vendre des copies de sa thèse de quelque manière et sous quelque forme que ce soit pour mettre des exemplaires de cette thèse à la disposition des personnes intéressées.

The author retains ownership of the copyright in his/her thesis. Neither the thesis nor substantial extracts from it may be printed or otherwise reproduced without his/her permission.

L'auteur conserve la propriété du droit d'auteur qui protège sa thèse. Ni la thèse ni des extraits substantiels de celle-ci ne doivent être imprimés ou autrement reproduits sans son autorisation.

ISBN 0-612-13050-9

Canada

Name *Kenneth M. Cramer*

*Dissertation Abstracts International* is arranged by broad, general subject categories. Please select the one subject which most nearly describes the content of your dissertation. Enter the corresponding four-digit code in the spaces provided.

*Psychology* — SUBJECT TERM

*0525* — SUBJECT CODE

U·M·I

## Subject Categories

## THE HUMANITIES AND SOCIAL SCIENCES

### COMMUNICATIONS AND THE ARTS

| | |
|---|---|
| Architecture | 0729 |
| Art History | 0377 |
| Cinema | 0900 |
| Dance | 0378 |
| Fine Arts | 0357 |
| Information Science | 0723 |
| Journalism | 0391 |
| Library Science | 0399 |
| Mass Communications | 0708 |
| Music | 0413 |
| Speech Communication | 0459 |
| Theater | 0465 |

### EDUCATION

| | |
|---|---|
| General | 0515 |
| Administration | 0514 |
| Adult and Continuing | 0516 |
| Agricultural | 0517 |
| Art | 0273 |
| Bilingual and Multicultural | 0282 |
| Business | 0688 |
| Community College | 0275 |
| Curriculum and Instruction | 0727 |
| Early Childhood | 0518 |
| Elementary | 0524 |
| Finance | 0277 |
| Guidance and Counseling | 0519 |
| Health | 0680 |
| Higher | 0745 |
| History of | 0520 |
| Home Economics | 0278 |
| Industrial | 0521 |
| Language and Literature | 0279 |
| Mathematics | 0280 |
| Music | 0522 |
| Philosophy of | 0998 |
| Physical | 0523 |
| Psychology | 0525 |
| Reading | 0535 |
| Religious | 0527 |
| Sciences | 0714 |
| Secondary | 0533 |
| Social Sciences | 0534 |
| Sociology of | 0340 |
| Special | 0529 |
| Teacher Training | 0530 |
| Technology | 0710 |
| Tests and Measurements | 0288 |
| Vocational | 0747 |

### LANGUAGE, LITERATURE AND LINGUISTICS

| | |
|---|---|
| Language | |
| General | 0679 |
| Ancient | 0289 |
| Linguistics | 0290 |
| Modern | 0291 |
| Literature | |
| General | 0401 |
| Classical | 0294 |
| Comparative | 0295 |
| Medieval | 0297 |
| Modern | 0298 |
| African | 0316 |
| American | 0591 |
| Asian | 0305 |
| Canadian (English) | 0352 |
| Canadian (French) | 0355 |
| English | 0593 |
| Germanic | 0311 |
| Latin American | 0312 |
| Middle Eastern | 0315 |
| Romance | 0313 |
| Slavic and East European | 0314 |

### PHILOSOPHY, RELIGION AND THEOLOGY

| | |
|---|---|
| Philosophy | 0422 |
| Religion | |
| General | 0318 |
| Biblical Studies | 0321 |
| Clergy | 0319 |
| History of | 0320 |
| Philosophy of | 0322 |
| Theology | 0469 |

### SOCIAL SCIENCES

| | |
|---|---|
| American Studies | 0323 |
| Anthropology | |
| Archaeology | 0324 |
| Cultural | 0326 |
| Physical | 0327 |
| Business Administration | |
| General | 0310 |
| Accounting | 0272 |
| Banking | 0770 |
| Management | 0454 |
| Marketing | 0338 |
| Canadian Studies | 0385 |
| Economics | |
| General | 0501 |
| Agricultural | 0503 |
| Commerce-Business | 0505 |
| Finance | 0508 |
| History | 0509 |
| Labor | 0510 |
| Theory | 0511 |
| Folklore | 0358 |
| Geography | 0366 |
| Gerontology | 0351 |
| History | |
| General | 0578 |
| Ancient | 0579 |
| Medieval | 0581 |
| Modern | 0582 |
| Black | 0328 |
| African | 0331 |
| Asia, Australia and Oceania | 0332 |
| Canadian | 0334 |
| European | 0335 |
| Latin American | 0336 |
| Middle Eastern | 0333 |
| United States | 0337 |
| History of Science | 0585 |
| Law | 0398 |
| Political Science | |
| General | 0615 |
| International Law and Relations | 0616 |
| Public Administration | 0617 |
| Recreation | 0814 |
| Social Work | 0452 |
| Sociology | |
| General | 0626 |
| Criminology and Penology | 0627 |
| Demography | 0938 |
| Ethnic and Racial Studies | 0631 |
| Individual and Family Studies | 0628 |
| Industrial and Labor Relations | 0629 |
| Public and Social Welfare | 0630 |
| Social Structure and Development | 0700 |
| Theory and Methods | 0344 |
| Transportation | 0709 |
| Urban and Regional Planning | 0999 |
| Women's Studies | 0453 |

## THE SCIENCES AND ENGINEERING

### BIOLOGICAL SCIENCES

| | |
|---|---|
| Agriculture | |
| General | 0473 |
| Agronomy | 0285 |
| Animal Culture and Nutrition | 0475 |
| Animal Pathology | 0476 |
| Food Science and Technology | 0359 |
| Forestry and Wildlife | 0478 |
| Plant Culture | 0479 |
| Plant Pathology | 0480 |
| Plant Physiology | 0817 |
| Range Management | 0777 |
| Wood Technology | 0746 |
| Biology | |
| General | 0306 |
| Anatomy | 0287 |
| Biostatistics | 0308 |
| Botany | 0309 |
| Cell | 0379 |
| Ecology | 0329 |
| Entomology | 0353 |
| Genetics | 0369 |
| Limnology | 0793 |
| Microbiology | 0410 |
| Molecular | 0307 |
| Neuroscience | 0317 |
| Oceanography | 0416 |
| Physiology | 0433 |
| Radiation | 0821 |
| Veterinary Science | 0778 |
| Zoology | 0472 |
| Biophysics | |
| General | 0786 |
| Medical | 0760 |

### EARTH SCIENCES

| | |
|---|---|
| Biogeochemistry | 0425 |
| Geochemistry | 0996 |
| Geodesy | 0370 |
| Geology | 0372 |
| Geophysics | 0373 |
| Hydrology | 0388 |
| Mineralogy | 0411 |
| Paleobotany | 0345 |
| Paleoecology | 0426 |
| Paleontology | 0418 |
| Paleozoology | 0985 |
| Palynology | 0427 |
| Physical Geography | 0368 |
| Physical Oceanography | 0415 |

### HEALTH AND ENVIRONMENTAL SCIENCES

| | |
|---|---|
| Environmental Sciences | 0768 |
| Health Sciences | |
| General | 0566 |
| Audiology | 0300 |
| Chemotherapy | 0992 |
| Dentistry | 0567 |
| Education | 0350 |
| Hospital Management | 0769 |
| Human Development | 0758 |
| Immunology | 0982 |
| Medicine and Surgery | 0564 |
| Mental Health | 0347 |
| Nursing | 0569 |
| Nutrition | 0570 |
| Obstetrics and Gynecology | 0380 |
| Occupational Health and Therapy | 0354 |
| Ophthalmology | 0381 |
| Pathology | 0571 |
| Pharmacology | 0419 |
| Pharmacy | 0572 |
| Physical Therapy | 0382 |
| Public Health | 0573 |
| Radiology | 0574 |
| Recreation | 0575 |
| Speech Pathology | 0460 |
| Toxicology | 0383 |
| Home Economics | 0386 |

### PHYSICAL SCIENCES

#### Pure Sciences

| | |
|---|---|
| Chemistry | |
| General | 0485 |
| Agricultural | 0749 |
| Analytical | 0486 |
| Biochemistry | 0487 |
| Inorganic | 0488 |
| Nuclear | 0738 |
| Organic | 0490 |
| Pharmaceutical | 0491 |
| Physical | 0494 |
| Polymer | 0495 |
| Radiation | 0754 |
| Mathematics | 0405 |
| Physics | |
| General | 0605 |
| Acoustics | 0986 |
| Astronomy and Astrophysics | 0606 |
| Atmospheric Science | 0608 |
| Atomic | 0748 |
| Electronics and Electricity | 0607 |
| Elementary Particles and High Energy | 0798 |
| Fluid and Plasma | 0759 |
| Molecular | 0609 |
| Nuclear | 0610 |
| Optics | 0752 |
| Radiation | 0756 |
| Solid State | 0611 |
| Statistics | 0463 |

#### Applied Sciences

| | |
|---|---|
| Applied Mechanics | 0346 |
| Computer Science | 0984 |
| Engineering | |
| General | 0537 |
| Aerospace | 0538 |
| Agricultural | 0539 |
| Automotive | 0540 |
| Biomedical | 0541 |
| Chemical | 0542 |
| Civil | 0543 |
| Electronics and Electrical | 0544 |
| Heat and Thermodynamics | 0348 |
| Hydraulic | 0545 |
| Industrial | 0546 |
| Marine | 0547 |
| Materials Science | 0794 |
| Mechanical | 0548 |
| Metallurgy | 0743 |
| Mining | 0551 |
| Nuclear | 0552 |
| Packaging | 0549 |
| Petroleum | 0765 |
| Sanitary and Municipal | 0554 |
| System Science | 0790 |
| Geotechnology | 0428 |
| Operations Research | 0796 |
| Plastics Technology | 0795 |
| Textile Technology | 0994 |

### PSYCHOLOGY

| | |
|---|---|
| General | 0621 |
| Behavioral | 0384 |
| Clinical | 0622 |
| Developmental | 0620 |
| Experimental | 0623 |
| Industrial | 0624 |
| Personality | 0625 |
| Physiological | 0989 |
| Psychobiology | 0349 |
| Psychometrics | 0632 |
| Social | 0451 |

DIFFERENCE OR MAKE-A-DIFFERENCE:

A COMPARISON OF ACTUAL CONTROL METRICS

BY

KENNETH M. CRAMER

A Thesis submitted to the Faculty of Graduate Studies of the University of Manitoba in partial fulfillment of the requirements of the degree of

DOCTOR OF PHILOSOPHY

© 1995

Permission has been granted to the LIBRARY OF THE UNIVERSITY OF MANITOBA to lend or sell copies of this thesis, to the NATIONAL LIBRARY OF CANADA to microfilm this thesis and to lend or sell copies of the film, and LIBRARY MICROFILMS to publish an abstract of this thesis.

The author reserves other publication rights, and neither the thesis nor extensive extracts from it may be printed or other-wise reproduced without the author's written permission.

## Acknowledgements

This work could not have been completed without the wisdom and guidance of my advisor, Dr. Jim Nickels. Since 1988, we have worked together on many projects, and Jim has demonstrated to me in particular that we each have the capabilities within ourselves to do great things. I hope in my own career that I will give to at least one student the support, confidence, and skills that Jim unconditionally provided for my accomplishments.

I would like to thank the direction and helpful suggestions from my committee members, namely Drs. Jim Forest, Alf Shephard, Michael Stack, and Jerry Burger (Santa Clara University). Their knowledge and foresight prevented several pitfalls in this research.

One very important member of our research team, Deborah Gural, should be gratefully recognized for her suggestions and insight; she will one day outdistance us all. And finally, I wish to acknowledge the seldom-appreciated support of my wife, Jennifer, who so often (during the course of this thesis) believed more in me than I did.

# Table of Contents

## List of Tables

List of Figures

Abstract

The present study compared Seligman's difference metric and Nickels' make-a-difference metric of actual control to see which metric is a more useful measure of participants' ratings of perceived control. Two screening items reduced the sample of 292 to 189. These participants were shown a new format of a 24-question 2-option test with 0, 1, or 2 correct answers for each question with the wording of each question missing. Those with higher test scores could leave the experiment sooner than participants with lower test scores. Participants differed according to their level of both difference metric control, or the number of correct answers they could earn by consistently selecting one option over another option (DM=0%, 25%, and 75%); and make-a-difference metric control, or the number of questions with only 1 correct answer (MDM=0%, 25%, and 75%). Participants indicated their perceived control over the magnitude of their total test score. Results showed that participants with high difference metric tests (DM=75%) felt more control compared to participants with moderate and low difference metric tests (DM=25% or 0%). Furthermore, participants with high or moderate make-a-difference metric tests (MDM=75% or 25%) felt more control than participants with low make-a-difference metric tests (MDM=0%). It is concluded that the make-a-difference metric is a more useful measure of actual control than the difference metric.

## Introduction

Just over a quarter century ago, Overmier and Seligman (1967) demonstrated an interesting phenomenon in contingency learning. Dogs from one group were individually placed in a two-sided shuttle box with the goal to jump from one side to the other to flee an electric shock (i.e., escapable shock). Dogs from the other group were prevented from jumping to the shock-free side (i.e., inescapable shock). When later placed individually in a shuttle box with a jumpable barrier, each dog that previously experienced escapable shock quickly jumped to safety, while each dog that experienced inescapable shock showed a remarkable behaviour pattern: The dog "ran around frantically for about thirty seconds. But then it stopped moving; to our surprise, it lay down and quietly whined" (Seligman, 1975, p. 22). This phenomenon, called learned helplessness, indicates that when exposed to uncontrollable stimulation, an organism learns to expect it has no control, resulting in cognitive, motivational, and emotional deficits. Several reviews indicate that learned helplessness is an extremely robust phenomenon (see Maier & Seligman, 1976; Maier, Seligman, & Solomon, 1969; Peterson, 1993; Peterson, Maier, & Seligman, 1993; Seligman & Maier, 1967; Seligman, Maier, & Geer, 1968; Seligman, Maier, & Solomon, 1971).

<u>Seligman's Conceptualization of Control</u>

Seligman attributed the inability of the dogs' escape to be a result of the manipulation of their previous learning. That is, dogs having learned they could not escape the shock in the past believed they could not escape future situations when shock was actually escapable. Seligman's experiments of learned helplessness were based on the notion of control or contingency defined as the likelihood of a desired outcome given a particular response. In any situation, there are two important likelihoods or conditional probabilities to consider: (1) The probability of the desired outcome given a response and (2) the probability of the desired outcome given a different response. Seligman (1975) writes:

> When the probability of an outcome is the same whether or not a given response occurs, the outcome is <u>independent</u> of that response. When this is true of all voluntary responses, the outcome is <u>uncontrollable</u>. Conversely, if the probability of an outcome when some response occurs is different from the probability of the outcome when that response doesn't occur, then that outcome is <u>dependent</u> on that response: the outcome is <u>controllable</u> (p. 16).

Control, then, represents the dependence of an outcome on a response, whereas uncontrol represents the independence of an outcome on a response. Seligman's (1975, p. 16) example of persons trying to hail an elevator to their floor outlines the dynamics of the two situations. If a down-bound elevator arrives 100% of the time the down-button is pressed and 0% of the time the up-button is pressed, then elevator movement depends on which button is pressed (i.e., control). However, if a down elevator arrives 50% of the time the down-button is pressed and 50% of the time the up-button is pressed, then elevator movement is independent of which button is pressed (i.e., uncontrol).

Both Seligman's original (Seligman, 1975) and reformulated (Abramson, Seligman, & Teasdale, 1978) theories of control have been tested in a variety of learned helplessness experiments, principally with animals (Maier et al., 1969; Overmier & Seligman, 1967; Seligman & Maier, 1967; Seligman et al., 1971). Additional animal research includes studies by Glass and Singer (1972), Hokanson, DeGood, Forrest, and Brittain (1971), Maier and Testa (1975), Miller and Norman (1979), Miller and Weiss (1969), Roth (1980), Seligman et al. (1968), and Weiss (1971).

Seligman's view of control has also been tested using humans (Hiroto & Seligman, 1975). Hiroto (1974) compared three groups of college students: An <u>escapable</u> group that received experience with noises contingent on button pressing, an <u>inescapable</u> group that received experience with noises noncontingently related to button pressing, and a <u>no noise</u> group. All groups were subsequently tested on a human shuttle box for their ability to escape aversive noise. The results were similar to those obtained with other species: Students who had received prior training with inescapable noise showed reduced performance of the requisite escape/avoidance response in the shuttle box test compared to students receiving prior exposure to contingent noise or no noise. In addition to impaired problem-solving and increased passivity (Seligman, 1975), uncontrol participants experienced mood changes, such as increased anxiety, hostility, and depression (Gatchel, Paulus, & Maples, 1975; Miller & Seligman, 1973, 1975), increased psychophysiological and subjective indices of pain and distress (Miller, 1979; Thompson, 1981), and increased susceptibility to disease and unhappiness in the elderly (Langer & Rodin, 1976; Rodin & Langer, 1977).

Seligman measure of actual control. The above studies examined differences between participants given 100% control and those given 0% control, but Seligman (1975) indicates that a control continuum between 100% and 0% exists. Jenkins and Ward (1965) extended the concept of control/uncontrol to include the degree of control. Their computation, later called the difference metric (Alloy & Abramson, 1979), calculated the difference between the probability of a desired outcome given one response (e.g., button press) and the probability of that same outcome given another response (e.g., button nopress). Peterson et al. (1993) write:

> the greater the difference between the two probabilities, the greater the degree of control. When the two

> probabilities are equal for some response, the reinforcer [outcome] is not dependent on that response (p. 21).

For example, a 100-0 contingency represents 100% difference metric control, while a 100-100 contingency represents 0% difference metric control. However, contingencies such as 100-25, 100-50, and 100-75 can exist with difference metric values of 75%, 50%, and 25%, respectively. Peterson et al. (1993) assumed that the organism "somehow assesses contingency by computing such ratios and comparing their difference" (p. 39). According to Abramson and Alloy (1981), Jenkins and Ward (1965) chose this metric because it was a simple approximation to the phi coefficient statistic and it facilitated comparison of results with the results of prior investigations (e.g., Ward & Jenkins, 1965). In fact, Tang and Critelli (1990, p. 721) say

> the absolute difference between these two conditional probabilities provided a close approximation to the actual degree of contingency, and as has become standard practice in judgment of control studies, this difference served as the index of actual control.

The difference metric has typically been used in clinical studies comparing the accuracy of perceptions of control between depressed and nondepressed persons. For instance, Alloy and Abramson (1979, Experiment 1) showed that participants with 75% difference metric control felt more control than participants with either 50% or 25% difference metric control. Modified replications of their investigation suggests that participants' perceptions of control are sensitive to the manipulation of the conditional probabilities (Alloy & Abramson, 1982; Alloy, Abramson, & Kossman, 1985, Experiment 3; Alloy, Abramson, & Viscusi, 1981; Benassi & Mahler, 1985, Experiments 1 & 2; Dresel, 1984; Martin, Abramson, & Alloy, 1984; Tang & Critelli, 1990; Vázquez, 1987, Experiments 1, 2, & 3).

Challenges to Seligman's concept of control. Recent animal research has suggested that Seligman's model of control is confounded with another concept, predictability. Investigations by Jackson and Minor (1988), Maier and Keith (1987), Mineka, Cook, and Miller (1984), and Volpicelli, Ulm, and Altenor (1984) indicated that subject fear (i.e., helplessness or a lack of perceived control) was affected no more by control (i.e., escaping an electric shock) than by prediction (i.e., knowing that the outcome was either a shock or no-shock). Rosellini and colleagues (i.e., DeCola, Rosellini, & Warren, 1988; Rosellini, DeCola, & Warren, 1986; Rosellini, Warren, & DeCola, 1987) showed that providing subjects with feedback of performance (i.e., shock or noshock) following each training trial significantly reduced subject fear of shock. They concluded that helplessness could be attributed either to the controllability of the shock (Maier & Seligman, 1976; Seligman, 1975) or to the predictability of the shock-free periods, which suggests that Seligman's concept of control incorporates the concept of prediction (Matute, 1994).

Seligman defines predictability as the dependence of an outcome on a signal (1975, p. 116). For example, the destination of a down-bound elevator would be predictable if its direction was indicated, perhaps by an illuminated down-arrow. However, the destination would not be predictable if when a down arrow was lit, an elevator proceeded sometimes in an upward direction and sometimes in a downward direction. Seligman would not disagree that prediction and control are confounded; in fact, Seligman argues that the concept of control must involve prediction because "these two variables are very hard to separate; for when control is present, prediction is as well" (1975, p. 124). More recently, Peterson et al. (1993) indicate that "there may be many potential interactions between control and prediction, and they will not be easy to separate" (p. 58).

Seligman assumes that, at least in nature, prediction and control can combine in three ways: (a) prediction-control (e.g., a post-cloud-seeding downpour), (b) prediction-uncontrol (e.g., a forecasted heatwave), and (c) unprediction-uncontrol (e.g., a surprise snowstorm). Traditionally, it has been assumed that (d) unprediction-control cannot exist (so no example would be conceptualizable). That is, the forecastability of an event must be inherent in the ability to influence that event. Seligman (1975, p. 111) indicates that when an organism:

> can control an event by a response he can also use the feedback from the response to predict the event. The reverse is not always true, however: if he can predict an event, he may not be able to control it.

Nickels (1980) contends, however, that through their actions, people may exercise control over an unpredictable event and recognize these actions to be control. Returning to the elevator example, Seligman assumes that control is <u>only</u> present if you have predictability of the outcome (i.e., knowledge of which button leads to which destination) before control is exercised. This works well if the unlabelled buttons are positioned vertically (top=up; bottom=down), but not if positioned side-by-side. Pressing either button in the latter case will bring an elevator bound in some direction, but no one would know in which direction, either down (desired) or up (undesired). Still, regardless of the elevator destination, the button presser would have completely determined (controlled) by blind selection the direction of the elevator's destination.

Nickels admits that it is often difficult to illustrate everyday situations of predictionless control, simply because, as Seligman (1975) indicates, people prefer to live and work in predictive situations. Badia and Culbertson (1972) found a majority of subjects chose longer signalled shock over shorter unsignalled shock. Moreover, Averill and Rosenn (1972) found that 75% of subjects who could avoid medium- or high-intensity shock preferred to listen for a warning signal, and showed less psychophysiological stress through galvanic skin response and heart rate measures than subjects who chose not to listen for a warning signal.

Although the debate continues on the possible separation of predictability and controllability, it has been shown that if not properly accounted for, predictability and controllability can confound one another and contaminate the results of a study (Averill, 1973; Burger & Arkin, 1980; Geer & Maisel, 1971; Hokanson et al., 1971; Staub, Tursky, & Schwartz, 1971, Experiment 2; Wortman, 1975). Therefore, investigations based on Seligman's conceptualization of control that show high ratings of controllability may be a result of the predictability element, the controllability element, or some combination of the two.

<u>Nickels' Reconceptualization of Control</u>

While Seligman's view of control confounds control with prediction, Nickels (1980) reconceptualizes prediction and control as independent concepts thereby isolating their separate effects. According to Nickels, control denotes the exercise of making "an impact on an event, regardless of whether or not the individual can anticipate the event"; prediction denotes the "anticipation of an event through the availability of relevant information about an event, regardless of whether or not the individual affects this event" (1980, p. 4). For instance, you would have control without prediction if you wanted a down-bound elevator and pressed one of two buttons placed side-by-side (one for down and the other for up, but you do not know which button corresponds to which direction). Conversely, you would have prediction without control if you knew which button gave you which elevator destination, but someone else pressed a button for the destination they wanted.

Nickels would expect that participants who know their choice has an impact on their outcome (control) should feel more control than participants who know their choice has no impact (uncontrol). In a two-experiment study, Nickels, Cramer, and Gural (1992) found that regardless of predictability, participants given control cues (100% impact) reported higher perceived control, influence, responsibility, and credit/blame than participants given no control cues (0% impact). In addition, Nickels and Cramer (1993) manipulated actual control by providing participants with either two different-coloured pencils (one red pencil and one blue pencil, i.e., control) or two same-coloured pencils (two red or two blue pencils, i.e., uncontrol). Results indicated that participants given the choice between one red pencil and one blue pencil perceived more control and responsibility than participants given the choice between either two red pencils or two blue pencils. That is, perceived controllability was affected by actual control alone, and not by outcome predictability (i.e., knowing after pencil selection but before the dependent measures questionnaire what colour led to what outcome) or their interaction.

Reconceptualized measure of actual control. Nickels (1980) maintains that the difference metric represents not a measure of actual control but the difference in getting a success by consistently exercising one response rather than another. That is, in a 75-50 contingency situation, there is a 25% greater likelihood of obtaining success from one response versus an alternative response. When the difference is zero, or rather, when neither option is more advantageous to the attainment of the desired outcome (e.g., 100-100, 75-75, 50-50, or 25-25), then outcome and response are considered to be noncontingently related.

According to Nickels' definition, actual control is better represented by a make-a-difference metric, or the percentage of trials on which the choice of one option results in a different outcome than the choice of an alternative option. For instance, if on a given trial, the same desired outcome (success) occurs regardless of whether or not one gives either a button press or nopress, then a successful outcome will be received on that trial regardless of the option chosen; that is, nothing the participant can do will change the outcome of the trial (i.e., 0% control).

As taken from Nickels (1980), Figure 1 represents the three possible forms actual control can take in Alloy and Abramson's

| | Method #1 L-Press / R-Press | Method #2 L-Press / R-Press | Method #3 L-Press / R-Press |
|---|---|---|---|
| Trial 1: | down----down | down----down | down----up |
| Trial 2: | down----down | down----up | down----up |
| Trial 3: | up----up | up----down | up----down |
| Trial 4: | up----up | up----up | up----down |
| Difference Metric Control | 0% | 0% | 0% |
| Make-a-Difference Metric Control | 0% | 50% | 100% |

_Figure 1_. Comparison of two control measures across different methods of obtaining a 50-50 (0%) Seligman metric.

simple 50-50 difference metric. Each diagram shows how the depression of a left or right button (L-Press or R-Press) in four mutually exclusive trials could bring either a down- or up-bound elevator (e.g., for Trial 2 in Method #3, pressing the left button would bring a down-bound elevator; pressing the right button an up-bound elevator). The success level is consistent across all three methods (i.e., 50-50, or a 50% probability of the desired outcome given a left or right button press), and Alloy and Abramson interpret this to mean all three cases represent 0% control. However, the degree of actual control defined as outcome-influence in each method can vary considerably depending on the paired sequences of reinforcement. According to Nickels (1980), in any given block of four trials, Method #1 has no instance whereby the outcome can be altered by an action (0% actual control). Two of the four trials in Method #2 offer control of the outcome (50% actual control), while all of the four trials in Method #3 offer control over the outcome (100% actual control).

Several studies have examined the effects of manipulating Nickels' make-a-difference metric control while keeping Seligman's difference metric control constant at zero. For instance, Guttormson (1984) varied the degree of control participants had over the matching of letters to a preset pattern. In a 16-letter pattern, control was based on the number of trials (either 4, 8, or 12) on which participants were able to alter the outcome from one of two possible responses. Guttormson found that "the higher the level of real [actual] control, the higher the level of perceived control" (p. 30).

Furthermore, Cramer and Nickels (1994) found that both depressives and nondepressives who determined approximately half of the trial outcomes (i.e., 52% make-a-difference metric) perceived more control than participants who determined none of the trial outcomes (i.e., 0% make-a-difference metric). Finally, Cramer, Nickels, and Gural (1994) and Nickels, Gural, and Cramer (1993) used four levels of make-a-difference metric control (0%, 25%, 50%, and 75%) and found that participants given no opportunity to change the final outcome (0% actual control) felt more helpless and less control than participants given any amount of control above 0%. That is, participants with some control felt they had greater influence over the overall outcome than participants with no control.

Present Study and Hypotheses

Investigations of Seligman's difference metric have neither controlled nor specified the make-a-difference metric, so past research has been unable to address the contribution of this variable to participants' control ratings. Moreover, Cramer et al. (1994) concluded that since investigations of Nickels' make-a-difference metric have typically held Seligman's difference metric at zero, it remains to be tested whether Nickels' metric is as efficient at nonzero difference metric values. More generally, it is of interest to determine which of the two metrics represents a better measure of actual control. Indeed, Peterson et al. (1993) write that:

> we had no good reason for choosing the [metric] that we did. We simply wished to argue for the importance of contingency in general terms, and we suggested the metric that seemed most obvious. But the [organism] could be using any of a number of computations, and they embody different theories. The obvious solution would be to manipulate some parameter and determine which metric best predicts the behaviour that results from the manipulation" (pp. 39-40).

The present study advanced three hypotheses. If Seligman's measure of actual control (i.e., difference metric; DM) is useful, then (1) participants perceiving an equal likelihood of success from all response options (i.e., difference metric values of zero) will report more perceived helplessness and less control than participants with an unequal likelihood of success from all response options (i.e., difference metric value greater than zero). If Nickels' measure of actual control (i.e., make-a-difference metric; MDM) is useful, then (2) participants perceiving they have no impact on their final outcome (i.e., make-a-difference metric values of zero) will report more perceived helplessness and less control than participants who have some impact over the final outcome (i.e., make-a-difference metric values greater than zero). While no interaction between the two metrics is anticipated, it is expected that (3) Nickels' metric will be a more sensitive measure of participants' perceived control than Seligman's metric.

## Method

### Participants

There were 117 male and 175 female University of Manitoba Introductory Psychology students who participated in the study for course credit. Participants were not selected on the basis of demographic variables, such as race or socioeconomic status. However, due to a failure to correctly answer questions about the experimental manipulations, 45 males and 58 females were excluded from analysis, leaving 189 participants in the reduced sample. The average age of this sample was 19.80 years (S.D.=3.74).

### Overview

Participants were told they were participating in two independent studies: The first study involved a 24-question test each with two options (A or B), but with the actual wording of the questions omitted. One option had 0, 6, or 18 more correct answers than the other option. Moreover, 0, 6, or 18 of the 24 questions had different outcomes (i.e., only one correct answer), while the remaining questions had the same outcomes (either zero or two correct answers). Participants selected one of the two options for each of 24 questions and then completed the dependent measures questionnaire. Participants immediately engaged in the second study which involved performing a motor task for 1 minute.

Materials

Participants received a large packet containing (a) an instruction booklet, (b) a sealed envelope containing a test answer key, and (c) several blank IBM answer sheets. The instruction booklet (see Appendix A) contained a sample of the answer key, the experimental manipulations, the 24-question test sheet, and the dependent measures questionnaire.

Independent and Dependent Variables

To test the three hypotheses, the study used a completely randomized 3 (difference metric actual control) x 3 (make-a-difference metric actual control) between-subjects fixed factorial design. The first independent variable, difference metric control, was operationally defined as the success differential between the two options. Participants were told whether one option had 0, 6, or 18 more correct answers (i.e., 0%, 25%, or 75% DM, respectively) than the other option (although participants did not know which option had more).

The second independent variable, make-a-difference control, was operationally defined as the number of questions with only 1 correct answer. Participants were told whether for 0, 6, or 18 questions (i.e., 0%, 25%, or 75% MDM, respectively), they completely determined whether or not they got the correct answer. The remaining 24, 18, or 6 questions had either zero or two correct answers in which participants knew they did not determine whether or not they got the correct answer. Participants did not know whether these "make-no-difference" questions had zero or two correct answers.

Two of the experimental groups (namely DM=25%/MDM=0% and DM=75%/MDM=0%) were presented conflicting information. Although these participants knew that both options were identical for each question (MDM=0%), they also knew that one option had more correct answers (either DM=25% or 75%) than the other option. However, it is unlikely that participants could appreciate this conflict through simple mental arithmetic, and as a result, the two groups were included. Moreover, while the three hypotheses were testable through the manipulation of the difference and make-a-difference metrics, the present study included three other variables in the analyses: (1) Questionnaire Type (i.e., order of presentation of the first two questions), (2) Sex of Participant, and (3) Lecture Section from which the data were collected.

On the dependent measures questionnaire, participants indicated their perceived control, responsibility, influence, opportunity, credit or blame, helplessness, hopelessness, dissatisfaction, sadness, failure, depression, success, happiness, pleasure, luck, satisfaction, and predictability, certainty, and knowledge of the question outcome on 1-7 point Likert scales. The questionnaire also assessed the following other variables and manipulation checks: The number of controllable trials (0-24), the number of questions with different outcomes, the difference in the number of correct answers between the two options, the number of correct answers attainable by consistently selecting the first option, the number of correct answers attainable by consistently selecting the second option, and the total number of correct answers they believed they got.

Experimental Controls

The study employed three experimental controls. To begin, all participants were given the opportunity to actively choose either option for each of the 24 questions, which eliminated the possibility of an illusion of control through differential effects of active involvement and choice on participants' perceptions of control (Langer, 1975). Moreover, participants were run simultaneously in large classroom settings, which reduced the opportunity for participants to tell one another about the experimental details (Nickels & Cramer, 1993). Finally, participants were given no predictability of each question's outcome or the total number of correct answers they earned (their total test score) until after the experiment was over. This final control was included for two reasons: It permitted the measure of pure control for each metric without contamination from predictive information, and it permitted the investigation of difference metric and make-a-difference metric values which have been traditionally impossible to examine (e.g., DM=75% and MDM=0%). Through randomization, the effects of several personality variables, including desire for control (Burger, 1992; Burger & Cooper, 1979), explanatory style (Peterson & Villanova, 1988), and locus of control (Rotter, 1966), were minimized.

Design and Procedure

Six sessions of the experiment were conducted in a large classroom. Each participant received a large packet containing several IBM answer sheets, an envelope containing an answer key, and an instruction booklet. Participants were told (see Experimenter's Instructions, Appendix B) they were taking part in two studies: The first study investigated a new format of multiple choice test, in which test questions could have one, two, or no correct answers; the second study investigated the speed at which people could perform a motor task, by blackening bubbles on several IBM answer sheets.

The experimenter described the properties of the multiple choice test: (a) the test questions were removed to simulate a student unprepared for a test, and (b) a test question could have 0, 1, or 2 correct answers. Participants were told that inside their packet was a sealed envelope which contained the test answer key. At the conclusion of both studies, participants would be permitted to compare their test sheet to the key to determine their total test score (total number of correct answers). Because of the number of students in the class, participants knew that at the end of the study, their total score would be verified by the experimenter as they were asked to leave in order of test score magnitude. That is, participants with higher test scores would leave the experiment sooner than participants with lower test scores.

Instruction booklet. Participants were instructed to remove the booklet from the large envelope and turn to page 2, which showed a sample of the answer key. Each participants' goal was to select either option A or B for each of 24 questions in order to get as many of the questions correct. The experimenter described the four possible locations of a correct answer, as shown on page 2 of each participants' instruction booklet. Participants were shown that Sample Questions 1 and 2 represented instances when the answer key had only 1 correct answer. That is, the choice of A or B made a difference in whether or not participants got the correct answer. Participants were also shown that for Sample Questions 3 and 4, there were either zero or two correct answers on the answer key. For these questions, their choice of A or B made no difference in whether or not they got the correct answer, but they should still select either A or B, since not answering would count against them.

Participants were instructed to turn to page 3 and read the instructions to themselves, which outlined two important aspects of the answer key: (1) The difference in the number of correct answers they could get by constantly selecting one the options rather than the other option (because participants did not know which option gave more correct answers, they were told to try an equal selection of both options); and (2) the number of questions on which their choice of option A or B made a difference in whether or not they got the correct answer (i.e., only one correct answer). Participants knew when a question had one or not-one (i.e., zero or two) correct answer, because this conveyed control information; they were not, however, given information which identified which questions had zero and which had two correct answers, since this conveyed prediction information.

Participants continued to page 4, the 24-question test sheet. They were reminded of the difference between the number of correct answers between the two options (i.e., Seligman control) and were told to circle one option (A or B) for each of the 24 questions. Beside each question was indicated whether the participants' choice of option made a difference or made NO difference. Finally, participants were asked to count the number of make-a-difference questions (i.e., Nickels' control).

Dependent measures questionnaire. Participants then completed the questionnaire on pages 5 through 7, assessing their perceived control, responsibility, influence, opportunity, credit or blame, helplessness, hopelessness, dissatisfaction, sadness, failure, depression, success, happiness, pleasure, luck, satisfaction, and predictability, certainty, and knowledge of the question outcome using 7 point (1-7) Likert scales; and number of controllable trials using a 0-24 point fill-in-the-blank format.

Post-experimental questionnaire. Page 8 of the booklet asked participants to provide additional information regarding their perceptions during the experiment, including any purpose to the experiment they may have derived, an outline of what they were asked to do, and an outline of any suspicions they may have had that the experiment was not genuinely interested in what it purported to study.

Bubble-filling task. Participants were then told to (a) place the instruction booklet inside the packet, (b) remove the IBM sheets and the sealed envelope from the packet (but not to open the envelope) and (c) return the packet under their seats. Participants would be able to examine the answer key to determine their score after they completed the second experiment, involving the blackening of IBM answer sheet bubbles. Participants were told to blacken as quickly as possible each bubble on the answer sheet, question by question. They knew that computer-scoring would not count a partially blackened bubble and would count stray marks against them. They went on to a new answer sheet once they had completely filled a sheet. After 1 minute of blackening bubbles, participants were told that the experiment was over and there would be no further bubble-filling sessions, and they were debriefed. They were allowed to open the sealed envelope to determine how they would have done if the experiment had continued.

## Results

Two manipulation checks were included as screening items in the questionnaire: (Q.21) "On how many of the 24 questions did your choice of A or B make a difference in whether or not you got the correct answer?"; and (Q.22) "How many more correct answers would you get by constantly selecting one option (either A or B) rather than the other option?" Of the 292 participants, 45 males and 58 females (35.3%) answered one or both of these questions incorrectly. Thirty-three of these participants indicated in their post-experimental questionnaire they were confused about the instructions. The remaining questionnaires indicated neither confusion during the experiment nor suspicion regarding the experimental purpose. Because this attrition was equivalently distributed across both control metrics ($z$=-1.71, $p$<.05; Rogers, Howard, & Vessey, 1993), it was not judged to invalidate the experimental results or conclusions.

Six of the remaining 189 questionnaires contained missing values for one or more of the following dependent variables: Motor speed, the total number of correct answers, and/or the number of correct answers to be attained by constantly selecting Option-A and by constantly selecting Option-B. These values were estimated by regressing the missing value with the variable of the highest absolute correlation (see Tabachnik & Fidell, 1989).

This procedure resulted in 189 complete questionnaires, with 8 males and 13 females in each of the 9 groups.

Factor Analysis

Table 1 shows the intercorrelations, means, and standard deviations of the 24 dependent variables. The empirical similarity among the dependent measures was assessed through a principal iterated factor analysis with both orthogonal (i.e., varimax) and oblique (i.e., promax) rotations and initial communality estimates derived from the squared multiple correlations. Cattell's scree eigenvalue plot (Cattell, 1966), Kaiser's unity criterion (Kaiser, 1970), and the residual correlation matrix (Cliff, 1988; Zwick & Velicer, 1986) suggested the presence of one independent and four correlated factors accounting for 92% of the total factor space. Table 2 shows the factor intercorrelation matrix and the factor-variable correlations. While the first four factors were moderately correlated with one another ($r$>.39), all factor correlations with the fifth factor did not exceed ±.08 ($p$>.05).

Factor 1 (Controllability). This factor accounted for 51.1% of the factor space and was composed of Perceived Control (1-7), Responsibility, Credit/Blame, Opportunity, Influence, Control (0-24), Success, and lack of Helplessness; it was designated a Controllability Factor.

Factor 2 (Predictability). Explaining 13.4% of the total variability of scores, this factor was composed of 5 variables:

Table 1

Dependent Variable: Intercorrelations, Means, and Standard Deviations

| Dependent Variable | 6 | 7 | 11 | 15 | 2 | 3 | 20 | 9 | 5 | 17 | 8 | 14 | 18 | 13 | 10 | 17 | 19 | 16 | 12 | 4 | 24 | 25 | 26 | Mean | SD |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. Control (1-7) | 37 | 29 | 28 | 20 | -47 | 37 | 31 | 15 | 9 | 20 | -19 | -19 | 6 | 12 | 2 | -1 | 17 | 11 | 17 | -9 | 11 | 17 | 25 | 2.71 | 1.94 |
| 6. Responsibility | 100 | 58 | 52 | 55 | -38 | 44 | 27 | 24 | 34 | 41 | -21 | -28 | 26 | 37 | 23 | 6 | 40 | 34 | 55 | 32 | 7 | 6 | 0 | 2.86 | 1.98 |
| 7. Influence | | 100 | 49 | 66 | -37 | 30 | 33 | 25 | 34 | 30 | -29 | -14 | 40 | 42 | 33 | 9 | 16 | 7 | 28 | 1 | 12 | 14 | 14 | 3.06 | 1.97 |
| 11. Credit/Blame | | | 100 | 64 | -34 | 40 | 52 | 41 | 32 | 55 | -27 | -42 | 48 | 37 | 23 | 1 | 30 | 27 | 45 | 15 | -9 | -4 | 13 | 2.41 | 1.84 |
| 15. Opportunity | | | | 100 | -30 | 22 | 49 | 37 | 37 | 44 | -30 | -18 | 40 | 43 | 35 | 10 | 28 | 24 | 41 | 14 | 2 | 4 | 22 | 3.02 | 1.92 |
| 2. Helplessness | | | | | 100 | -38 | -28 | -21 | -18 | -30 | 44 | 17 | -14 | -18 | -9 | 10 | -20 | -9 | -24 | 13 | -7 | -7 | -15 | 5.19 | 1.91 |
| 3. Success | | | | | | 100 | 17 | 27 | 29 | 35 | -34 | -26 | 21 | 31 | 31 | -7 | 22 | 18 | 26 | 11 | 9 | 18 | 17 | 2.89 | 1.64 |
| 20. Control (0-24) | | | | | | | 100 | 34 | 20 | 39 | -10 | -25 | 27 | 17 | 12 | 14 | 17 | 18 | 26 | 7 | 2 | 0 | 25 | 5.87 | 7.21 |
| 9. Knowledge | | | | | | | | 100 | 72 | 77 | -49 | -50 | 35 | 36 | 28 | -4 | 22 | 18 | 32 | 4 | 0 | -3 | 7 | 2.65 | 1.97 |
| 5. Predict | | | | | | | | | 100 | 63 | -49 | -37 | 25 | 40 | 27 | -7 | 23 | 21 | 33 | 16 | 13 | -2 | 1 | 3.12 | 1.97 |
| 17. Certain | | | | | | | | | | 100 | -38 | -46 | 41 | 44 | 30 | -2 | 36 | 27 | 45 | 17 | -5 | -10 | 1 | 2.38 | 1.71 |
| 8. Hopelessness | | | | | | | | | | | 100 | -34 | -15 | -28 | -27 | 12 | -7 | -13 | -19 | -1 | -20 | -5 | -9 | 4.49 | 2.02 |
| 14. Luck | | | | | | | | | | | | 100 | -22 | -21 | -6 | 13 | -10 | -1 | -20 | -12 | 12 | 13 | 13 | 5.34 | 2.01 |
| 18. Pleased | | | | | | | | | | | | | 100 | 71 | 38 | 3 | 15 | 20 | 20 | 3 | -5 | -4 | 15 | 2.89 | 1.79 |
| 13. Happy | | | | | | | | | | | | | | 100 | 47 | 3 | 25 | 23 | 40 | 10 | 10 | 16 | 16 | 2.80 | 1.87 |
| 10. Satisfied | | | | | | | | | | | | | | | 100 | 15 | 4 | 7 | 13 | 0 | 7 | 18 | 4 | 3.67 | 1.96 |
| 17. Motor Speed | | | | | | | | | | | | | | | | 100 | -5 | -1 | 10 | 1 | -1 | 7 | 12 | 54.52 | 19.31 |
| 19. Sad | | | | | | | | | | | | | | | | | 100 | 56 | 68 | 26 | 4 | 13 | 0 | 2.06 | 1.45 |
| 16. Dissatisfied | | | | | | | | | | | | | | | | | | 100 | 53 | 27 | 0 | 0 | 6 | 2.31 | 1.68 |
| 12. Depressed | | | | | | | | | | | | | | | | | | | 100 | 17 | 2 | 17 | 3 | 1.79 | 1.47 |
| 4. Failure | | | | | | | | | | | | | | | | | | | | 100 | -5 | -16 | -19 | 3.23 | 1.83 |
| 24. Select-A | | | | | | | | | | | | | | | | | | | | | 100 | 52 | 27 | 11.08 | 5.35 |
| 25. Select-B | | | | | | | | | | | | | | | | | | | | | | 100 | 23 | 11.50 | 5.43 |
| 26. Number Correct | | | | | | | | | | | | | | | | | | | | | | | 100 | 11.45 | 4.80 |

Note. Correlation decimals have been removed; significant if $r > \pm 14$ (df=187, $p < .05$); variables are sorted according to the factor analysis.

Table 2

Intercorrelations and Loadings of Oblique Factors

| Factor or Dependent Variable | Factor Correlations/Loadings | | | | |
|---|---|---|---|---|---|
| | F1 | F2 | F3 | F4 | F5 |
| Factor 1 | 1.00 | | | | |
| Factor 2 | .39 | 1.00 | | | |
| Factor 3 | .44 | .47 | 1.00 | | |
| Factor 4 | .42 | .47 | .42 | 1.00 | |
| Factor 5 | .03 | -.08 | -.08 | -.06 | 1.00 |
| Eigenvalue | 6.30 | 1.69 | 1.27 | 1.15 | 0.92 |
| Variance | 51.1% | 13.4% | 10.3% | 9.4% | 7.4% |
| Control (1-7) | .74* | .29 | .19 | .26 | .15 |
| Responsibility | .67* | .27 | .35 | .49 | .04 |
| Credit/Blame | .64* | .46 | .64 | .49 | -.26 |
| Opportunity | .62* | .36 | .70 | .33 | -.09 |
| Influence | .61* | .30 | .59 | .27 | .08 |
| Control (0-24) | .53* | .26 | .42 | .24 | -.15 |
| Success | .49* | .40 | .29 | .34 | .17 |

continued on next page

| | | | | | |
|---|---|---|---|---|---|
| Helplessness | -.67* | -.31 | -.19 | -.24 | -.12 |
| Knowledge | .32 | .86* | .50 | .42 | -.13 |
| Certain | .43 | .79* | .58 | .58 | -.23 |
| Predict | .29 | .77* | .44 | .45 | -.01 |
| Hopelessness | -.37 | -.63* | -.25 | -.20 | -.17 |
| Luck | -.30 | -.57* | -.24 | -.23 | .28 |
| | | | | | |
| Happy | .29 | .45 | .74* | .38 | .13 |
| Pleased | .26 | .36 | .72* | .30 | -.12 |
| Satisfied | .14 | .25 | .50* | .09 | .17 |
| Motor Speed | .03 | -.15 | .17* | -.02 | .02 |
| | | | | | |
| Sad | .32 | .38 | .22 | .72* | .01 |
| Depressed | .44 | .34 | .42 | .70* | .02 |
| Dissatisfied | .16 | .22 | .20 | .54* | -.03 |
| Failure | .03 | .06 | -.01 | .22* | -.17 |
| | | | | | |
| Select-A | .11 | -.04 | .08 | .05 | .70* |
| Select-B | .11 | .05 | .00 | .01 | .63* |
| Number Correct | .24 | .00 | .20 | .02 | .29* |

Note. N=189; * theoretical factor loadings.

Perceived Knowledge, Certainty, Prediction, and a lack of Hopelessness and Luck; it was designated a Predictability Factor.

Factor 3 (Satisfaction). This factor explained 10.3% of the total variability and was composed of Happiness, Satisfaction, Pleased, and Motor Speed; it was designated a Satisfaction and Performance Factor.

Factor 4 (Dissatisfaction). Accounting for 9.4% of the total factor space, this Dissatisfaction Factor was composed of Sadness, Depression, Dissatisfaction, and Perceived Failure.

Factor 5 (Multiple-Choice Test). This factor explained 7.4% of the variability and was composed of three variables: (1) The number of correct answers participants thought they got, (2) the number they thought they would get if they constantly selected option-A, and (3) the number they thought they would get if they constantly selected option-B; it was designated a Test Factor.

Multivariate and Univariate Analyses

The factor analysis denoted five dependent-variable packages which were separately analyzed using a robust multivariate analysis of variance (MANOVA) to assess the effects of the difference metric (DM) and make-a-difference metric (MDM). Moreover, two additional variables were included in each analysis: (1) Questionnaire Type (QT), or the order of the first two questions that participants received on the questionnaire (either the control-item or helpless-item presented first), and (2) Sex of the participant. (See Tables 3 through 12 for all

Table 3

Difference Metric: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Dependent Variable | $F$ | $\omega^2$ | $M_0$<br>($SD$) | $M_{25}$<br>($SD$) | $M_{75}$<br>($SD$) |
|---|---|---|---|---|---|
| CONTROL PACKAGE | 2.37** | .12 | | | |
| Control (1-7) | 5.08** | .04 | 2.30[a] | 2.29[a] | 3.19[b] |
| | | | (1.77) | (1.43) | (2.14) |
| Responsibility | 0.61 | | 2.75 | 2.56 | 2.87 |
| Credit/Blame | 0.08 | | 2.68 | 2.29 | 2.27 |
| Opportunity | 4.55* | .04 | 3.03[ab] | 3.46[a] | 2.56[b] |
| | | | (1.94) | (2.11) | (1.60) |
| Influence | 2.67 | | 2.92 | 3.27 | 2.98 |
| Control (0-24) | 0.86 | | 6.73 | 4.83 | 6.05 |
| Success | 1.62 | | 2.84 | 2.54 | 3.29 |
| Helplessness | 1.73 | | 5.37 | 5.32 | 4.76 |
| PREDICTION PACKAGE | 3.21*** | .19 | | | |
| Know | 4.86** | .03 | 3.22[a] | 2.37[b] | 2.35[b] |
| | | | (2.27) | (1.56) | (1.60) |
| Certain | 1.99 | | 2.87 | 2.06 | 2.19 |
| Predict | 5.11** | .04 | 3.73[a] | 2.81[b] | 2.81[b] |

continued on next page

| | | | | | |
|---|---|---|---|---|---|
| | | | (2.03) | (1.58) | (1.84) |
| Hopelessness | 1.04 | | 4.49 | 4.38 | 4.59 |
| Luck | 7.23*** | .06 | 4.52[a] | 5.81[b] | 5.70[b] |
| | | | (2.35) | (1.59) | (1.77) |
| SATISFACTION PACKAGE | 2.46* | .12 | | | |
| Happy | 1.97 | | 2.46 | 3.17 | 2.78 |
| Pleased | 0.58 | | 2.94 | 2.78 | 2.95 |
| Satisfied | 4.49* | .03 | 3.41[a] | 3.87[a] | 3.98[a] |
| | | | (1.90) | (1.65) | (1.90) |
| Motor Speed | 3.04 | | 51.10 | 55.05 | 57.42 |
| DISSATISFTN PACKAGE | 1.73 | .09 | | | |
| Sad | | | 1.98 | 1.78 | 2.10 |
| Depressed | | | 1.75 | 2.02 | 1.62 |
| Dissatisfied | | | 2.13 | 2.43 | 2.30 |
| Failure | | | 3.32 | 2.75 | 3.37 |
| TEST PACKAGE | 2.40* | .09 | | | |
| Select-A | 4.82** | .04 | 9.14[a] | 12.08[b] | 12.02[b] |
| | | | (5.79) | (4.76) | (4.98) |
| Select-B | 5.44** | .05 | 9.22[a] | 12.71[b] | 12.55[b] |
| | | | (5.79) | (4.16) | (5.54) |
| Number Correct | 1.64 | | 10.48 | 12.09 | 11.78 |

Note. Identical superscripts are not significantly different.
$n$=63; * $p$<.05, ** $p$<.01, *** $p$<.001.

Table 4

Make-a-Difference Metric: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Dependent Variable | F | $\omega^2$ | $M_0$<br>(SD) | $M_{25}$<br>(SD) | $M_{75}$<br>(SD) |
|---|---|---|---|---|---|
| CONTROL PACKAGE | 2.31** | .19 | | | |
| Control (1-7) | 4.95** | .04 | 1.92[a]<br>(1.48) | 2.90[b]<br>(1.68) | 2.95[b]<br>(2.14) |
| Responsibility | 7.18*** | .06 | 2.08[a]<br>(1.66) | 2.94[b]<br>(1.69) | 3.16[b]<br>(1.98) |
| Credit/Blame | 1.31 | | 2.14 | 2.63 | 2.46 |
| Opportunity | 0.59 | | 2.78 | 3.32 | 2.95 |
| Influence | 5.94** | .06 | 2.43[a]<br>(1.71) | 3.44[b]<br>(1.85) | 3.30[b]<br>(1.93) |
| Control (0-24) | 4.18* | .03 | 3.87[a]<br>(7.42) | 6.99[b]<br>(6.25) | 6.75[b]<br>(7.58) |
| Success | 4.61* | .04 | 2.49[a]<br>(1.64) | 3.08[a]<br>(1.59) | 3.10[a]<br>(1.62) |
| Helplessness | 4.28* | .03 | 5.46[a]<br>(2.07) | 5.30[ab]<br>(1.43) | 4.68[b]<br>(2.10) |
| PREDICTION PACKAGE | 1.51 | .09 | | | |

continued on next page

| | | | | | |
|---|---|---|---|---|---|
| Know | | | 2.75 | 2.43 | 2.76 |
| Certain | | | 2.50 | 2.24 | 2.49 |
| Predict | | | 2.98 | 3.22 | 3.14 |
| Hopelessness | | | 4.62 | 4.68 | 4.16 |
| Luck | | | 5.08 | 5.51 | 5.44 |
| SATISFACTION PACKAGE | 0.72 | .04 | | | |
| Pleased | | | 2.68 | 3.00 | 2.98 |
| Happy | | | 2.63 | 2.89 | 2.89 |
| Satisfied | | | 3.71 | 3.68 | 3.87 |
| Motor Speed | | | 55.80 | 49.71 | 58.06 |
| DISSATISFTN PACKAGE | 1.85 | .09 | | | |
| Sad | | | 1.65 | 2.16 | 2.05 |
| Depressed | | | 1.84 | 1.81 | 1.73 |
| Dissatisfied | | | 2.24 | 2.37 | 2.25 |
| Failure | | | 2.60 | 3.48 | 3.35 |
| TEST PACKAGE | 3.18** | .12 | | | |
| Select-A | 2.13 | | 10.65 | 11.91 | 10.68 |
| Select-B | 0.94 | | 11.60 | 12.26 | 10.63 |
| Number Correct | 8.96*** | .07 | 9.87[a] | 12.86[b] | 11.61[b] |
| | | | (5.44) | (4.55) | (3.87) |

Note. Identical superscripts are not significantly different.

$n$=63; * $p$<.05, ** $p$<.01, *** $p$<.001.

Table 5

Questionnaire Type: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Dependent Variable | *F* | $\omega^2$ | Control *M* (*SD*) (*n*=104) | Helpless *M* (*SD*) (*n*=85) |
|---|---|---|---|---|
| CONTROL PACKAGE | 1.65 | .08 | | |
| Control (1-7) | | | 2.44 (1.89) | 2.78 (1.78) |
| Responsibility | | | 2.44 (1.69) | 3.07 (1.95) |
| Credit/Blame | | | 2.47 (2.05) | 2.34 (1.55) |
| Opportunity | | | 2.97 (1.96) | 3.07 (1.88) |
| Influence | | | 3.18 (2.07) | 2.91 (1.61) |
| Control (0-24) | | | 6.16 (8.01) | 5.52 (6.13) |
| Success | | | 2.79 (1.73) | 3.01 (1.51) |
| Helplessness | | | 5.20 (1.98) | 5.08 (1.84) |
| PREDICTION PACKAGE | 1.57 | .05 | | |
| Know | | | 2.53 (1.84) | 2.79 (1.91) |
| Certain | | | 2.43 (1.73) | 2.31 (1.69) |
| Predict | | | 3.11 (1.93) | 3.13 (1.80) |
| Hopelessness | | | 4.63 (2.01) | 4.31 (2.03) |

continued on next page

| | | | | |
|---|---|---|---|---|
| Luck | | | 5.20 (2.12) | 5.52 (1.86) |
| SATISFCTION PACKAGE | 1.27 | .03 | | |
| Pleased | | | 3.09 (1.81) | 2.65 (1.52) |
| Happy | | | 2.77 (1.87) | 2.85 (1.64) |
| Satisfied | | | 3.89 (1.84) | 3.59 (1.81) |
| Motor Speed | | | 54.53 (21.57) | 54.51 (16.26) |
| DISSATISF PACKAGE | $4.73^{**}$ | .11 | | |
| Sad | $4.04^{*}$ | .01 | 1.81 (1.15) | 2.13 (1.31) |
| Depressed | $16.55^{****}$ | .07 | 1.46 (0.84) | 2.20 (1.74) |
| Dissatisfied | $8.77^{**}$ | .03 | 2.04 (1.34) | 2.59 (1.64) |
| Failure | 0.02 | | 3.23 (1.98) | 3.04 (1.41) |
| TEST PACKAGE | 0.25 | .00 | | |
| Select-A | | | 12.02 (5.69) | 10.65 (4.90) |
| Select-B | | | 12.55 (5.80) | 11.60 (4.97) |
| Number Correct | | | 11.78 (4.98) | 9.87 (4.57) |

Note. * $p<.05$, ** $p<.01$, *** $p<.001$, **** $p<.0001$.

Table 6

Sex: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Dependent Variable | *F* | $\omega^2$ | Males *M* | Males (*SD*) (*n*=117) | Females *M* | Females (*SD*) (*n*=72) |
|---|---|---|---|---|---|---|
| CONTROL PACKAGE | 0.64 | .03 | | | | |
| Control (1-7) | | | 2.67 | (1.70) | 2.55 | (1.93) |
| Responsibility | | | 2.51 | (1.78) | 2.85 | (1.86) |
| Credit/Blame | | | 2.28 | (1.65) | 2.50 | (1.94) |
| Opportunity | | | 2.82 | (1.62) | 3.14 | (2.08) |
| Influence | | | 2.94 | (1.85) | 3.13 | (1.90) |
| Control (0-24) | | | 5.21 | (6.10) | 6.28 | (7.82) |
| Success | | | 3.13 | (1.74) | 2.74 | (1.55) |
| Helplessness | | | 5.07 | (1.78) | 5.20 | (2.00) |
| PREDICTION PACKAGE | 0.94 | .03 | | | | |
| Know | | | 2.81 | (1.70) | 3.05 | (1.97) |
| Certain | | | 2.57 | (1.55) | 4.43 | (1.80) |
| Predict | | | 3.22 | (1.78) | 2.26 | (1.92) |
| Hopelessness | | | 4.58 | (1.89) | 5.19 | (2.11) |

continued on next page

| | | | | | | |
|---|---|---|---|---|---|---|
| Luck | | | 5.60 | (1.68) | 2.93 | (2.18) |
| SATISFACTION PACKAGE | 1.08 | .03 | | | | |
| Pleased | | | 2.81 | (1.72) | 2.94 | (1.68) |
| Happy | | | 2.78 | (1.75) | 2.82 | (1.78) |
| Satisfied | | | 3.78 | (2.06) | 3.74 | (1.68) |
| Motor Speed | | | 57.41 | (23.13) | 52.75 | (16.39) |
| DISSATISFTN PACKAGE | 4.10** | .10 | | | | |
| Sad | 0.27 | | 1.94 | (1.25) | 1.96 | (1.22) |
| Depressed | 5.19* | .02 | 2.04 | (1.69) | 1.64 | (1.10) |
| Dissatisfied | 0.30 | | 2.21 | (1.43) | 2.33 | (1.54) |
| Failure | 7.74** | .03 | 2.71 | (1.47) | 3.41 | (1.85) |
| TEST PACKAGE | 2.20 | .04 | | | | |
| Select-A | | | 10.32 | (4.36) | 11.55 | (5.84) |
| Select-B | | | 11.54 | (4.97) | 11.47 | (5.72) |
| Number Correct | | | 11.03 | (4.71) | 11.71 | (4.85) |

<u>Note</u>. * $p<.05$, ** $p<.01$.

Table 7

Difference Metric by Make-a-Difference Metric: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Variable<br>MDM Level | $F$ | $\omega^2$ | Difference Metric Control<br>$M_0$ ($SD$) | <br>$M_{25}$ ($SD$) | <br>$M_{75}$ ($SD$) |
|---|---|---|---|---|---|
| CONTROL PACKAGE | 0.84 | .16 | | | |
| PREDCTN PACKAGE | 2.52*** | .28 | | | |
| Know | 4.90*** | .07 | | | |
| 0% MDM | | | 4.24(2.43) | 1.48(0.87) | 2.52(1.94) |
| 25% MDM | | | 1.95(1.47) | 2.90(1.76) | 2.43(1.60) |
| 75% MDM | | | 3.48(2.25) | 2.71(1.55) | 2.10(1.22) |
| Certain | 1.41 | | | | |
| Predict | 1.52 | | | | |
| Hopelessness | 1.03 | | | | |
| Luck | 2.52* | .03 | | | |
| 0% MDM | | | 3.90(2.72) | 5.62(2.11) | 5.71(2.08) |
| 25% MDM | | | 5.67(1.46) | 5.71(1.52) | 5.14(1.90) |
| 75% MDM | | | 4.00(2.35) | 6.10(1.00) | 6.24(1.09) |
| SATISF PACKAGE | 1.17 | .11 | | | |
| DISSATSF PACKAGE | 1.53 | .15 | | | |
| TEST PACKAGE | 0.97 | .07 | | | |

*Note*. $n$ = 21. * $p<.05$, *** $p<.001$.

Table 8

Difference Metric by Questionnaire Type: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Variable<br>Questionnaire | $F$ | $\omega^2$ | Difference Metric Control<br>$M_0$ ($SD$) | <br>$M_{25}$ ($SD$) | <br>$M_{75}$ ($SD$) |
|---|---|---|---|---|---|
| CONTROL PACKAGE | 1.63 | .16 | | | |
| PREDCTN PACKAGE | 2.05* | .12 | | | |
| Know | 0.87 | | | | |
| Certain | 0.16 | | | | |
| Predict | 0.26 | | | | |
| Hopelessness | 1.42 | | | | |
| Luck | 1.82 | | | | |
| SATISF PACKAGE | 1.65 | .08 | | | |
| DISSATSF PACKAGE | 2.91** | .14 | | | |
| Sad | 0.25 | | | | |
| Depressed | 2.50 | | | | |
| Dissatisfied | 6.26** | .05 | | | |
| Control | | | 1.51(0.74) | 2.30(1.31) | 2.31(1.65) |
| Helpless | | | 2.89(1.81) | 2.57(1.74) | 2.30(1.30) |
| Failure | 0.73 | | | | |
| TEST PACKAGE | 0.63 | .02 | | | |

*Note*. * $p<.05$, ** $p<.01$.

Table 9

Difference Metric by Sex: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Variable<br>Sex | $F$ | $\omega^2$ | Difference Metric Control $M_0$ ($SD$) | $M_{25}$ ($SD$) | $M_{75}$ ($SD$) |
|---|---|---|---|---|---|
| CONTROL PACKAGE | 1.72* | .16 | | | |
| Control (1-7) | 0.13 | | | | |
| Responsibility | 4.48* | .03 | | | |
| Males ($n$=24) | | | 2.42(1.35) | 2.92(2.12) | 2.21(1.79) |
| Females ($n$=39) | | | 2.95(1.85) | 2.33(1.66) | 3.28(1.97) |
| Credit/Blame | 1.77 | | | | |
| Opportunity | 0.31 | | | | |
| Influence | 0.83 | | | | |
| Control (0-24) | 1.37 | | | | |
| Success | 1.84 | | | | |
| Helplessness | 2.79 | | | | |
| PREDCTN PACKAGE | 1.21 | .08 | | | |
| SATISF PACKAGE | 1.84 | .09 | | | |
| DISSATSF PACKAGE | 2.09* | .10 | | | |
| Sad | 2.88 | | | | |

continued on next page

| | | | | | |
|---|---|---|---|---|---|
| Depressed | 3.11* | .03 | | | |
| Males ($n$=24) | | | 1.71(1.27) | 2.54(2.08) | 1.88(1.57) |
| Females ($n$=39) | | | 1.77(1.04) | 1.69(1.22) | 1.46(1.05) |
| Dissatisfied | 4.24* | .02 | | | |
| Males ($n$=24) | | | 1.83(1.24) | 2.54(1.53) | 2.25(1.48) |
| Females ($n$=39) | | | 2.31(1.61) | 2.36(1.53) | 2.33(1.53) |
| Failure | 2.35 | | | | |
| TEST PACKAGE | 0.75 | .03 | | | |

Note. * $p<.05$.

Table 10

Make-a-Difference Metric by Questionnaire Type: Multivariate and Univariate Statistics and Means (Standard Deviations)

| PACKAGE<br>Variable<br>Questionnaire | *F* | $\omega^2$ | Make-a-Difference Metric Control<br>$M_0$ (*SD*) | <br>$M_{25}$ (*SD*) | <br>$M_{75}$ (*SD*) |
|---|---|---|---|---|---|
| CONTROL PACKAGE | 1.75* | .17 | | | |
| Control (1-7) | 0.15 | | | | |
| Responsibility | 1.27 | | | | |
| Credit/Blame | 2.57 | | | | |
| Opportunity | 0.62 | | | | |
| Influence | 0.11 | | | | |
| Control (0-24) | 2.35 | | | | |
| Success | 0.78 | | | | |
| Helplessness | 0.01 | | | | |
| PREDCTN PACKAGE | 0.54 | .03 | | | |
| SATISF PACKAGE | 1.10 | .06 | | | |
| DISSATSF PACKAGE | 3.28** | .15 | | | |
| Sad | 5.47** | .10 | | | |
| Control | | | 1.74(0.93) | 1.89(1.35) | 1.79(1.15) |
| Helpless | | | 1.55(1.01) | 2.52(1.37) | 2.34(1.32) |

continued on next page

| | | | | | |
|---|---|---|---|---|---|
| Depressed | 0.99 | | | | |
| Dissatisfied | $9.45^{**}$ | .07 | | | |
| Control | | | 2.53(1.56) | 1.86(0.99) | 1.74(1.31) |
| Helpless | | | 1.90(1.21) | 3.04(1.81) | 2.86(1.66) |
| Failure | 0.09 | | | | |
| TEST PACKAGE | 1.29 | .05 | | | |

<u>Note</u>. * $p<.05$, ** $p<.01$.

Table 11

Make-a-Difference Metric by Sex: Multivariate Statistics

| PACKAGE<br>Variable<br>Sex | *F* | $\omega^2$ | Make-a-Difference Metric Control | | |
|---|---|---|---|---|---|
| | | | $M_0$ (*SD*) | $M_{25}$ (*SD*) | $M_{75}$ (*SD*) |
| CONTROL PACKAGE | 0.92 | .09 | | | |
| PREDCTN PACKAGE | 0.91 | .06 | | | |
| SATISF PACKAGE | 0.85 | .04 | | | |
| DISSATSF PACKAGE | 1.00 | .05 | | | |
| TEST PACKAGE | 1.59 | .06 | | | |

Table 12

Three-Way and Four-Way Interactions: Multivariate and Univariate F-Statistics (Proportion of Explained Variance)

| Package Variable ANOVAs | Interactions | | | | |
|---|---|---|---|---|---|
| | S*N*Q ($\omega^2$) | S*N*X ($\omega^2$) | S*Q*X ($\omega^2$) | N*Q*X ($\omega^2$) | S*N*Q*X ($\omega^2$) |
| CONTROL PACKAGE | 2.03*** (.34) | 1.16 (.22) | 0.99 (.10) | 1.21 (.12) | 1.03 (.20) |
| Control (1-7) | 0.72 | ____ | ____ | ____ | ____ |
| Responsibility | 0.70 | ____ | ____ | ____ | ____ |
| Credit/Blame | 0.36 | ____ | ____ | ____ | ____ |
| Opportunity | 1.52 | ____ | ____ | ____ | ____ |
| Influence | 2.30 | ____ | ____ | ____ | ____ |
| Control (0-24) | 0.86 | ____ | ____ | ____ | ____ |
| Success | 1.39 | ____ | ____ | ____ | ____ |
| Helplessness | 1.86 | ____ | ____ | ____ | ____ |
| PREDICTION PACKAGE | 0.72 (.09) | 1.02 (.13) | 1.31 (.08) | 1.82 (.11) | 1.68* (.20) |
| Know | ____ | ____ | ____ | ____ | 0.94 |
| Certain | ____ | ____ | ____ | ____ | 1.22 |

continued on next page

| | | | | | |
|---|---|---|---|---|---|
| Predict | ____ | ____ | ____ | ____ | 0.87 |
| Hopelessness | ____ | ____ | ____ | ____ | 0.75 |
| Luck | ____ | ____ | ____ | ____ | 0.70 |
| SATISFACTN PACKAGE | 1.00 | 1.38 | 1.63 | 1.16 | 1.66 |
| | (.10) | (.14) | (.08) | (.06) | (.16) |
| DISSATISF PACKAGE | 1.90* | 1.70* | 1.97 | 1.46 | 1.01 |
| | (.18) | (.16) | (.10) | (.07) | (.10) |
| Sad | 1.06 | 0.96 | ____ | ____ | ____ |
| Depressed | 0.98 | 1.31 | ____ | ____ | ____ |
| Dissatisfied | 1.78 | 1.60 | ____ | ____ | ____ |
| Failure | 1.72 | 0.50 | ____ | ____ | ____ |
| TEST PACKAGE | 1.73 | 1.10 | 0.93 | 0.82 | 0.82 |
| | (.13) | (.08) | (.04) | (.03) | (.06) |

Note. S=Seligman Metric; N=Nickels Metric; Q=Questionnaire Type; X=Sex; * $p<.05$, *** $p<.001$; dashes indicate unpursued univariate effects.

multivariate and univariate effects, means, standard deviations, and magnitude of effect size estimates.)

Controllability package. The MANOVA (with Wilks' criterion and Satterthwaite's solution for degrees of freedom) assessed the effects of the DM, MDM, QT, Sex, and all interactions on the Controllability dependent variables and found a significant main effect for the DM, $F(16,292)=2.37$, $p=.0025$; and for the MDM, $F(16,292)=2.31$, $p=.0033$. There was a significant interaction for the DM x Sex, $F(16,292)=1.72$, $p=.0425$; the MDM x QT, $F(16,292)=1.75$, $p=.0381$; and the DM x MDM x QT, $F(32,540)=2.03$, $p=.0009$.

An analysis of variance (ANOVA) for each dependent variable found that the DM significantly affected participants' perceived control (1-7) and opportunity. Ryan, Einott, Gabriel, and Welch F-statistic comparisons (REGWF; Ryan, 1960) indicated that participants with 75% DM felt more control than participants with either 0% or 25% DM (whose estimates did not significantly differ), while participants with 25% DM felt greater opportunity than participants with 75% DM (neither of which differed from participants with 0% DM). Furthermore, the ANOVAs and Ryan comparisons showed that participants with 0% MDM perceived more control, responsibility, influence, success, and felt they controlled more trials than participants with either 25% or 75% MDM, whose ratings did not differ. However, only 0% MDM participants felt more helpless than 75% MDM participants.

A simple effects analysis of the DM x Sex interaction indicated a significant difference in perceived responsibility, such that females felt significantly more responsible for their test score than males, but only when DM=75%, $F(1,61)=4.71$, $p=.0339$. Analysis of the MDM x QT and DM x MDM x QT interactions revealed no significant univariate differences ($p>.05$).

Predictability package. The MANOVA for the Predictability variables indicated a significant main effect for the DM, $F(10,298)=3.21$, $p=.0006$; and significant interactions for DM x MDM, $F(20,495)=2.52$, $p=.0003$; DM x QT, $F(10,298)=2.05$, $p=.0284$; and DM x MDM x QT x Sex, $F(20,495)=1.68$, $p=.0332$. The ANOVAs indicated that the DM significantly affected participants' perceived knowledge, prediction, and luck, such that 25% and 75% DM participants felt less knowledge and prediction of their total test score but attributed their total test score to luck to a greater degree than 0% DM participants.

Analysis of both the DM x QT and DM x MDM x QT x Sex interactions revealed no significant univariate differences ($p>.05$), but analysis of the DM x MDM interaction indicated a significant effect for both knowledge and luck. Simple effects and Ryan comparisons of perceived knowledge indicated that at DM=0%, the 25% MDM participants felt less knowledge of their test score than the 0% and 75% MDM participants, $F(2,61)=6.51$, $p=.0028$; at DM=25%, the 0% MDM participants felt significantly less knowledge of their test score than the 25% and 75%

participants, F(2,61)=6.05, p=.0040; but at DM=75%, participants' knowledge of their test score did not significantly differ among the MDM levels, F<1. Simple effects and Ryan comparisons of perceived luck indicated that at DM=0%, the 25% MDM participants attributed their test score to luck to a greater extent than the 0% and 75% MDM participants, F(2,61)=4.12, p=.0211; but when the DM was either 25% or 75%, participants' attributions to luck did not differ among the MDM levels, p>.05.

Satisfaction package. The MANOVA for the Satisfaction variables indicated a significant main effect for only the DM, F(8,300)=2.46, p=.0136. Univariate analyses indicated that the DM affected participants' perceived satisfaction, F(2,153)=4.49, p=.0128, although Ryan comparisons found no significant differences among the DM levels.

Dissatisfaction package. The MANOVA for the Dissatisfaction variables indicated significant main effects for QT, F(4,150)=4.73, p=.0013; and Sex, F(4,150)=4.10, p=.0035. There was also a significant interaction for DM x Sex, F(8,300)=2.09, p=.0366; DM x QT, F(8,300)=2.91, p=.0039; MDM x QT, F(8,300)=3.28, p=.0013; DM x MDM x QT, F(16,459)=1.90, p=.0189; and DM x MDM x Sex, F(16,459)=1.70, p=.0431. Univariate analyses of the main effects indicated that participants who completed the questionnaire headed by the control question felt less sad, dissatisfied, and depressed than participants who completed the questionnaire headed by the helplessness question. Moreover, females felt less depressed but believed they failed to a greater degree than males.

Univariate analysis of the DM x Sex interaction showed significant effects for dissatisfaction and depression. A simple effects analysis indicated that at 0% DM, females felt more dissatisfied than males, $F(1,51)=7.77$, $p=.0074$; but no more depressed, $p>.05$. At 25% DM, males felt more depressed than females, $F(1,51)=8.61$, $p=.0050$, but no more dissatisfied, $p>.05$. Finally, at 75% DM, males and females did not differ in their degree of dissatisfaction or depression, $p>.05$.

Univariate analysis of the DM x QT interaction showed significant effects for dissatisfaction; a simple effects analysis indicated that at 0% DM, participants who received the control-item questionnaire felt more dissatisfied than participants who received the helpless-item questionnaire, $F(1,34)=26.65$, $p<.0001$; but no differences emerged at other DM levels. Analysis of the MDM x QT interaction indicated that at 0% MDM, participants who received either questionnaire did not differ in their perceptions of sadness and dissatisfaction. However, at 25% MDM, participants who received the control-item questionnaire felt less dissatisfied than participants who received the helpless-item questionnaire, $F(1,38)=15.59$, $p=.0002$; and at 75% MDM, participants who received the control-item questionnaire felt less sad and less dissatisfied than participants who received the helpless-item questionnaire,

$\underline{F}(1,61)=8.83$, $\underline{p}=.0045$; and $\underline{F}(1,61)=10.02$, $\underline{p}=.0026$, respectively. Analysis of the DM x MDM x QT and DM x MDM x Sex interactions revealed no significant univariate differences.

Multiple-choice test package. The MANOVA for the Multiple-Choice Test variables indicated a significant main effect for the DM, $\underline{F}(6,302)=2.40$, $\underline{p}=.0279$; and the MDM, $\underline{F}(6,302)=3.18$, $\underline{p}<.0049$. Univariate analyses indicated that the DM significantly affected participants' estimates of the number of correct answers they thought they would get if they constantly selected option-A and if they constantly selected option-B. Ryan comparisons showed that 0% DM participants felt they would get fewer correct answers by constantly selecting option-A and get fewer correct answers by constantly selecting option-B than 25% and 75% DM participants. The univariate analysis also indicated that the MDM significantly affected participants' estimates of the number of correct answers they got, such that 0% MDM participants felt they would get a lower test score than 25% and 75% MDM participants.

Metric comparison. The DM and MDM were compared for their relative usefulness in four individual analyses. The first compared the degree of relationship between the two metrics and two measures of perceived control (measured on either a 7- or 25-point scale). Results showed that the DM significantly correlated with the 7-point (DM-7; $\underline{r}=.19$, $\underline{p}=.0091$) but not the 25-point scale (DM-25; $\underline{r}=-.02$, $\underline{p}=.8002$), whereas the MDM significantly correlated with both the 7-point (MDM-7; *r*=.14, *p*=.0479) and 25-point scales (MDM-25; *r*=.16, *p*=.0250). A dependent measures *z*-test for differences in correlations in a single sample (Darlington & Carlson, 1987) indicated no difference between the DM-7 correlation and both the MDM-7, *z*=.45, *p*=.3264; and MDM-25 correlations, *z*=1.12, *p*=.1314.

In the second analysis, the 25-point scale was regressed in an intercept model including both the DM and MDM to see if either metric could reliably predict the number of questions participants believed they controlled. The intercept was not significantly different from zero, ß=3.67, *t*(187)=1.96, *p*=.0516, and was subsequently dropped from the model. As a result, a no-intercept model using the same predictors was tested. This model explained 40.49% of the variability in participants' perceived control. Results showed that the number of questions participants believed they controlled could be reliably predicted using the MDM, ß=2.28, *t*(187)=2.26, *p*<.0001; but not the DM, ß=0.51, *t*=1.08, *p*=.2810.

In the third analysis, two accuracy of control variables were constructed by subtracting each of the DM and MDM from participants' perceived control (25-point scale). The group means of the accuracy measure of the more useful metric would approximate zero. Table 13 shows the means, variances, and *t*-statistics for both DM and MDM accuracy of control variables. Results indicated that both metrics produced control inflations

Table 13

Difference Metric and Make-a-Difference Metric Accuracy Variables: Means, Variances, and t-statistics

| Actual Control<br>Mean<br>Variance<br>t-statistic | Accuracy Variable<br>DM Accuracy | <br>MDM Accuracy |
|---|---|---|
| 0% | | |
| Mean | 6.73 | 3.87 |
| Variance | 67.07 | 55.08 |
| t-statistic | 6.52* | 4.14* |
| 25% | | |
| Mean | -1.17 | 0.99 |
| Variance | 30.37 | 39.08 |
| t-statistic | -1.69 | 1.26 |
| 75% | | |
| Mean | -11.95 | -11.25 |
| Variance | 58.44 | 57.52 |
| t-statistic | -12.40* | -11.87* |

Note. $n=63$; $*p<.0001$.

when actual control was 0% ($p<.0001$), and produced control deflations when actual control was 75% ($p<.0001$), but produced accurate judgments when actual control was 25% ($p>.05$).

Finally, in the fourth analysis, the variances for both accuracy variables represented the degree of dispersion or inaccuracy in participants' ratings of control. By dividing the larger by the smaller variance, a significant F-ratio would show which accuracy variable produced the lesser amount of error (i.e., inaccuracy). When comparisons were made between the two accuracy measures, results indicated no significant differences between variances at the 0% level, $F(62,62)=1.22$, $p=.2180$; the 25% level, $F(62,62)=1.29$, $p=.1593$; and the 75% level, $F(62,62)=1.02$, $p=.4691$. When comparisons were made within each accuracy variable but across the different levels of actual control, differences emerged for the DM accuracy variable, whereby the accuracy measure for the 25% DM participants was significantly less variable than the accuracy measure for the 0% DM participants, $F(62,62)=2.21$, $p=.0011$.

Analysis of section effects. Since data were collected across six introductory psychology sections, it was of interest to determine whether the effects of either control metric on the dependent variables varied according to the different periods of data collection. A MANOVA including Nickels' make-a-difference metric, Seligman's difference metric, and the six levels of section as the independent variables (and all possible interactions) assessed difference among the dependent variables in each of the five packages. All effects were nonsignificant ($p$>.05) except within the Satisfaction Package, which showed a significant interaction between difference metric and section, $F(32,518)=1.48$, $p=.0459$. Univariate analyses, however, revealed that no single dependent variable was significantly affected by the interaction of these two variables ($p$>.05).

## Discussion

The present study tested the effects of both the traditional difference metric and the recently proposed make-a-difference metric for differences in participants' perceptions of control and helplessness. It was expected that either metric would significantly affect these ratings but Nickels' make-a-difference metric would represent a more useful measure of control than Seligman's difference metric. No claims were made regarding either an interaction between the metrics or their effects on participants' perceived predictability. Because the present study uncovered effects not hypothesized, the discussion is divided into primary (hypothesized) and auxiliary findings.

### Primary Findings

Difference metric hypothesis. Three hypotheses were proposed in the present study. Firstly, it was hypothesized that participants with difference metric values of zero would perceive less control and more helplessness than participants with difference metric values above zero. This hypothesis was partially confirmed in participants' ratings of perceived control: Participants given high difference metric tests (DM=75%) felt more control than participants given low or zero difference metric tests (DM=25% and 0%, respectively), whose ratings did not differ. The hypothesis was also partially confirmed in participants' ratings of opportunity: Participants given high difference metric tests felt more opportunity than participants given low difference metric tests. However, the high-control participants felt no less helplessness, no more influence, responsibility, success, credit or blame, and felt they controlled the outcome of no more questions than the other participants. In fact, of the entire set of controllability variables (so grouped by their significant intercorrelation and nomological similarity), only control and opportunity were affected by the manipulation of the difference metric. In addition, the group differences for perceived control only emerged when participants were given large amounts of difference metric control (DM=75%); small manipulations (DM=25%) had no significant impact. Overall, these findings indicate that when people know that one action leads to a great deal more success than other actions, they perceive more control and opportunity than people who know that all actions bring an equal likelihood of success.

The difference metric studies to date have always provided participants with outcome predictability (e.g., trial outcomes).

In fact, Seligman (1975) and Peterson et al. (1993) indicate that predictability is theoretically necessary if participants are to perceive any control. Because it has been shown that this information can influence perceived controllability (Cramer & Nickels, 1994; Nickels et al., 1993), this information was eliminated in the present study. As a result, proponents of the difference metric would expect no difference in perceived control among the three levels of the difference metric. The present study found otherwise: Even without this predictive information, people's ratings of control were affected by manipulation of the difference metric, which indicates that prediction is not a necessary component for participants to perceive control.

Although large manipulations of the difference metric were needed for significant changes in ratings of perceived control, only small manipulations were needed for significant changes in perceived prediction. That is, increasing the likelihood of success for one action rather than another reduces participants' certainty in their likelihood of obtaining a high test score; this would appear to denote a metric for uncertainty, as Nickels (1980) hypothesized, rather than a metric of controllability, as traditionally believed (Abramson & Alloy, 1981; Alloy & Abramson, 1979; Peterson et al., 1993; Seligman, 1975). Research by Jenkins and Ward (1965) and Ward and Jenkins (1965) utilizing Seligman's difference metric found that participants' perceptions of control covaried more substantially with the reinforcement level (i.e., number of successes) rather than the difference metric; but since success or reinforcement levels were not provided in the present study, it further suggests that predictive information is a likely confound with controllability ratings and warrants regulation.

Make-a-difference metric hypothesis. The second hypothesis stated that participants with make-a-difference metric values of zero would perceive less control and more helplessness than participants with make-a-difference metric values above zero. This hypothesis was confirmed: Participants given some different-outcome questions (MDM=25% and 75%) felt more control, responsibility, and influence, felt they controlled the outcome of more questions, and believed they would get a higher test score than participants given no different-outcome questions (MDM=0%). Moreover, participants given a large number of different-outcome questions (MDM=75%) felt significantly less helpless than participants given no different-outcome questions. This suggests that high levels of the make-a-difference metric are needed for individuals to feel control, but smaller levels are needed for individuals to feel helpless. This result has been consistently demonstrated in previous research. Cramer et al. (1994) and Nickels et al. (1993) both reported that when the difference metric is zero, participants given no different-outcome trials felt more helplessness and less control than participants given some different-outcome trials (i.e., MDM=25%, 50%, or 75%). Both studies suggested that a some-or-none phenomenon may operate in people's judgments of helplessness and control, such that any amount of control above zero is sufficient to produce significant differences in perceived control. Finally, because the differences among the make-a-difference metric were consistent across all three levels of Seligman's difference metric, it suggests that the results of past investigations, which maintained a 0% difference metric, are applicable to other difference metric values. Although the present study indicated significant differences in perceived control among the make-a-difference metric levels, the metric had no such effect on participants' predictability ratings. That is, changes in Nickels' reconceptualized metric of control (defined as independent from prediction) had no effect on ratings of predictability.

The present study supplements studies by both Cramer et al. (1994) and Nickels et al. (1993) in suggesting a critical boundary between perceptions of no control when the make-a-difference metric is zero and perceptions of control when the make-a-difference metric is greater than zero. It remains to be determined the pivotal point at which perceptions of controllability are no longer significantly different. Future studies might consider manipulation of the make-a-difference metric to values between 0% and 25% to assess the critical level at which participants perceive control.

Metric comparison hypothesis. The third hypothesis stated that the make-a-difference metric would be a more useful measure of actual control than the difference metric. This hypothesis was partially (though not conclusively) confirmed in a number of analyses. The make-a-difference metric was shown to be the better of the two metrics from the following results: (1) The make-a-difference metric significantly affected more control-related dependent variables (e.g., control, influence, responsibility, number of controllable questions, absence of helplessness) than the difference metric; (2) the make-a-difference metric correlated with both the 7- and 25-point control variables, whereas the difference metric correlated only with the 7-point control variable; and (3) the regression analysis showed that the make-a-difference metric (and not the difference metric) was a significant predictor of participants' ratings of perceived control, explaining 40% of the differences in scores. However, neither metric proved more useful in the analysis of the accuracy variables: (1) Both metrics were consistent in the mean accuracy rating at each level of actual control and (2) both metrics showed a consistent level of variability in accuracy ratings.

In summary, the present study tentatively suggests that the make-a-difference metric is a more useful measure of actual control than the difference metric, but also encourages the pursuit of more research in metric utility. Both Gibbon, Berryman, and Thompson (1974) and Hammond and Paynter, Jr. (1983) examined several contingency theories (including the difference metric) and compared their similarities and differences. They concluded that:

> all contingency formulations deal inadequately with a number of important variables, to a far greater extent than most psychologists realize. It is our guess that the problems of the existing contingency theories ... will not be solved by concatenating another probabilistic contingency formula (Hammond & Paynter, Jr., 1983, p. 547).

This new approach to contingency learning might be embodied in the make-a-difference metric, whose formulation is not based on probabilities. Future research might consider a comparison of the make-a-difference metric and those contingency theories reviewed by Hammond and Paynter, Jr. (1983).

Auxiliary Findings

Several results in the present study, although not hypothesized, were nonetheless interesting and warrant comment. These included an examination of control metric interactions, the two different questionnaire types, the effects of the sex of the participant on responsibility and dissatisfaction variables, and the absence of both motor speed differences, and general satisfaction with having control.

Control metric interactions. The present study found no interaction between the difference metric and the make-a-difference metric in participants' ratings of controllability, and this may have resulted from one of two factors. Either there was insufficient experimental power to detect an interaction or there exists no such interaction between these metrics in the general population. A post-experimental power analysis of differences among the perceived control means indicated that with 21 participants per group, there was a likelihood between 87% and 93% of detecting an interaction if it existed in the population. As a result, the present study is confident in its conclusion of no interaction.

There were, however, significant interactions between the two metrics and two predictability variables: Knowledge and luck. To tease apart these interactions, it will be shown how Nickels' make-a-difference metric affected the relationship between Seligman's difference metric and both knowledge and luck. Collapsing across the make-a-difference metric, the knowledge and luck variables had similar but opposite effects: 0% difference metric participants felt significantly less knowledge of the test score but were more likely to attribute the test score to luck than the 25% or 75% difference metric participants. However, different effects emerged at different difference metric values: When the difference metric was zero, 25% MDM participants felt less knowledgable about their test score but attributed their test score to luck to a greater extent than 0% and 75% MDM participants. When the difference metric was high, no differences among the metric levels were detected for both knowledge and luck. However, when the difference metric was low, there remained no difference among the make-a-difference metric levels for perceived luck, but the 0% MDM participants felt less knowledge about their final score than 25% and 75% MDM participants. It would seem that the significant difference for perceived knowledge when the difference metric is low is spurious, since knowledge and luck consistently and inversely covaried and there appears to be no theoretical reason to adopt a phenomenon to explain why, when the difference metric was low, 0% MDM participants feel less knowledgeable about their test score than 25% and 75% MDM participants.

There is one possible explanation for the make-a-difference metric differences of knowledge and luck when the difference metric is zero. The MDM=25% participants may have received enough actual control to perceive it, but not enough to overcome the belief that their test score may be influenced by luck; 0% and 75% MDM participants did not act similarly because they either had no controllable trials (0% MDM) or an abundance of controllable trials (75% MDM). Likewise, one reason 25% MDM participants indicated they had no knowledge of the outcome is because they had enough actual control to affect their test score, but not enough to change it according to how they would like. The 0% and 75% MDM participants did not act similarly because they either did not determine the outcome at all (0% MDM) or they determined the outcome to a great extent and could bring about the test score they wanted (75% MDM).

<u>Questionnaire type</u>. There were two different questionnaires administered randomly across all groups to assess any progressive effects of initial questionnaire items on successive questionnaire items (Dambrot, 1980; Graziano & Raulin, 1993). The questionnaires differed only by the order of presentation of the first two questions (assessing perceived controllability and helplessness), so participants answered either a control-item or a helpless-item first in their questionnaire. This is the first controllability study of its kind to assess group differences across the order of items participants receive.

The order of the control and helplessness questions was found only to affect participants' ratings of dissatisfaction with the test, such that participants who received the control-item questionnaire felt more dissatisfied, depressed, and sad than participants who received the helpless-item questionnaire. This represents a relief to researchers of past studies, as it shows that control-related variables were unaffected by the questionnaire item order. But it does provide a useful caution to researchers who wish to further examine the relationship between control and failure.

Overall, the results suggest that participants used the first item of the questionnaire as a means to identify the experimental purpose. For instance, participants who received the helpless-item questionnaire believed the study was investigating helplessness, which involves the negative endorsement of these items. As a result, the helpless-item participants showed higher depressive-like ratings than participants who received the control-item questionnaire (Graziano & Raulin, 1993; Orne, 1962).

It should be pointed out that ratings of both sadness and dissatisfaction were tempered by interactions with one or both of the actual control metrics. For example, when the make-a-difference metric was zero, control- and helpless-item participants felt similar dissatisfaction and sadness. When the make-a-difference metric was low, there was no difference in sadness, but control-item participants feel less dissatisfied than helpless-item participants. Finally, when the make-a-difference metric was high, control-item participants indicated greater dissatisfaction and sadness than helpless-item participants. These findings have two implications: It suggests that individuals can adopt a response style in their endorsement of dissatisfaction items, whereby participants who believe the study examines controllability (as based on the first questionnaire item) endorse the dissatisfaction items using their perceived control as a judge. But also, it suggests that on a dependent measures questionnaire, participants may be more reluctant to endorse a strong-sentimented variable such as sadness rather than a less extreme variable like dissatisfaction, since a large amount of make-a-difference controllability was needed before a difference in perceived sadness emerged between control- and helpless-item participants.

However, the opposite occurred in the interaction with the difference metric: The only difference between control-item and helpless-item questionnaire participants in perceived dissatisfaction occurred only when the difference metric was zero, and then the control-item participants felt more unhappy than the helpless-item participants. This interaction would suggest an association between the difference metric and participants' expression of dissatisfaction at having an equal likelihood of success from the two response options. By giving participants even 25% more correct answers from one response option, their dissatisfaction disappears. This once again suggests that the two metrics are measuring a slightly different (although not completely independent) constellation of variables.

Sex of participant. It has not been uncommon to observe sex differences in control and helplessness research (Alloy & Abramson, 1979; Dweck, Davidson, Nelson, & Enna, 1978; Dweck, Goetz, & Strauss, 1980; Sedek & Kofta, 1990). Analysis of the effects of the sex variable alone indicated that males felt more depressed than females, but females felt they failed to a greater degree than males. Numerous studies (Dweck & Bush, 1976; Dweck & Gilliard, 1975; Nicholls, 1975; Nickels et al., 1993; Sayers, Baucom, & Tierney, 1993) have indicated that females "show lower expectancies of success than do [males] across a wide variety of domains" (Dweck & Litch, 1980, p. 203). Many of the differences between males and females in the present study were tempered by interactions with the difference metric. To begin, despite a main effect for neither sex nor the difference metric for perceived responsibility, their combined effects yielded a significant difference, but only when the difference metric was high. That is, males felt no less responsibility than females, except when the stakes for success were very high. This suggests that if the potential outcome is either extreme success or extreme failure, females take more responsibility than males for the outcome of their choices. Moreover, only when the available responses led to an equal likelihood of success (DM=0%) did females feel more dissatisfied with the test than males. Perhaps females see situations like these as representing an inflexible or preset situation than if more successes were available. In short, when the stakes are high (DM=75%), females feel more responsible; but when the stakes are zero (DM=0%), females become frustrated and dissatisfied with the test, which indicates that females appear to be better consumers of the tasks they perform. Similar sex differences have been observed in sociobiological research of mate selection criteria, whereby males are more likely to fall in love, while females are more likely to fall out of love (Barash, 1979, 1982; Buss, 1987; Wilson, 1978).

Finally, males felt more depressed than females about the outcome of the test, but only when the stakes were moderate (DM=25%). This can be explained by the ambiguity males possibly perceive in the test. That is, tests with a zero stake of success (DM=0%) or a substanially greater stake of success (DM=75%) are easy to understand; but tests with a low stake of success (DM=25%) offer too little information on what exactly to do in order to achieve success. In this case, the stakes are too high to deem the final test score preset and uninfluenced by one's choice of responses; but the stakes are also too low to have a sufficient opportunity to obtain the desired number of successes (Keinan, 1994).

Nonsignificant differences in motor speed. The present study found no significant differences across the independent variables (i.e., difference metric, make-a-difference metric, questionnaire type, sex, and section) and their interactions for the speed at which participants filled in the IBM answer sheets. This result is not surprising in the assessment of Nickels' make-a-difference metric, since no research to date has documented a performance deficit (Cramer & Nickels, 1988; Nickels et al., 1992, Experiment 2; Nickels & Cramer, 1993). But the result is surprising for investigators of Seligman's difference metric, since a lack of controllability is theoretically comprised of performance deficits (Peterson et al., 1993; Seligman, 1975). Some studies have found performance deficits following learned helplessness training in participants' motivation to escape aversive noise (e.g., Alloy & Abramson, 1979; Glass, Reim, & Singer, 1971; Glass & Singer, 1972; Hiroto & Seligman, 1975; Klein & Seligman, 1976; Langer, 1975; Miller & Seligman, 1976) and in the solution of anagrams (e.g., Benson & Kennelly, 1976; Gatchel & Proctor, 1976; Hiroto & Seligman, 1975; Klein, Fencil-Morse, & Seligman, 1976). But other studies have found enhanced performance following learned helplessness training (e.g., Hanusa & Schultz, 1977; Roth & Kubal, 1975; Tennen & Eller, 1977; Wortman, Panciera, Shusterman, & Hibscher, 1976). The absence of motor speed differences in the present study may have resulted because participants received no information regarding their progress (i.e., outcome feedback), typically provided in traditional difference metric research. Although Nickels et al. (1993) manipulated outcome feedback (to levels of high success feedback, low success feedback, and no success feedback) to assess its contribution to controllability measures, they failed to take a performance measure, so it remains to be tested whether outcome predictability is necessary for participants to acquire a performance deficit. Future research might consider a more detailed examination of the specific factors necessary for these deficits to emerge.

Lack of participant satisfaction. The present study found no differences in participants' ratings of satisfaction with the test following the manipulation of either metric. This result is unusual given the volumes of research which document a variety of benefits derived from the belief in perceived control, regardless of whether or not that belief is veridical (Alloy & Abramson, 1979; Burger, 1989; Langer, 1975; Langer & Rodin, 1977; Perlmuter & Monty, 1977). This suggests that the metrics are important in the assessment of actual control but fail to accurately predict whether participants will perceive their control as something beneficial. It would appear that more than just control is needed for participants to feel happy. One experimental constant included in the present study was the number of choices (24 selections of either A or B) that participants made; this was regulated because research by Langer (1975) and Rodin and Langer (1977) found greater satisfaction with an increase in the number of significant choices one makes over the outcome of an event. Perhaps an information variable such as prediction by itself or in tandem with control could bring about more satisfaction. Future research might consider the addition of either pre- or post-selection information as a key factor to participants' perceptions of satisfaction.

References

Abramson, L. Y., & Alloy, L. B. (1981). Depression, nondepression, and cognitive illusions: Reply to Schwartz. Journal of Experimental Psychology: General, 110, 436-447.

Abramson, L. Y., Seligman, M. E. P., & Teasdale, J. (1978). Learned helplessness in humans. Critique and reformulation. Journal of Abnormal Psychology, 87, 49-74.

Alloy, L. B., & Abramson, L. Y. (1979). Judgment of contingency in depressed and nondepressed: Sadder but wiser? Journal of Experimental Psychology: General, 108, 441-485.

Alloy, L. B., & Abramson, L. Y. (1982). Learned helplessness, depression, and the illusion of control. Journal of Personality and Social Psychology, 42, 1114-1126.

Alloy, L. B., & Abramson, L. Y. (1988). Depressive realism: Four theoretical perspectives. In L. B. Alloy (Ed.), Cognitive processes in depression (pp. 223-265). New York: Guilford Press.

Alloy, L. B., Abramson, L. Y., & Kossman, D. (1985). The judgment of predictability in depressed and nondepressed college students. In F. R. Brush & J. B. Overmier (Eds.), Affect, conditioning and cognition: Essays on the determinants of behaviour (pp. 229-246). Hillsdale, NJ: Erlbaum.

Alloy, L. B., Abramson, L. Y., & Viscusi, D. (1981). Induced mood and the illusion of control. Journal of Personality and Social Psychology, 41, 1129-1140.

Averill, J. R. (1973). Personal control over aversive stimuli and its relationship to stress. Psychological Bulletin, 80, 286-303.

Averill, J. R., & Rosenn, M. (1972). Vigilant and non-vigilant coping strategies and psychophysiological stress reactions during the anticipation of electric shock. Journal of Personality and Social Psychology, 23, 128-141.

Badia, P., & Culbertson, S. (1972). The relative aversiveness of signalled vs. unsignalled escapable and inescapable shock. Journal of Experimental Analysis of Behavior, 17, 463-471.

Barash, D. P. (1979). The whisperings within: Evolution and the origin of human nature. New York: Penguin.

Barash, D. P. (1982). Sociobiology and behavior (2nd ed.). New York: Elsevier.

Benassi, V. A., & Mahler, H. I. M. (1985). Contingency judgments by depressed college students: Sadder but not always wiser. Journal of Personality and Social Psychology, 49, 1323-1329.

Benson, J. S., & Kennelly, K. J. (1976). Learned helplessness: The result of uncontrollable reinforcements or uncontrollable aversive stimuli? Journal of Personality and Social Psychology, 34, 138-145.

Burger, J. M. (1989). Negative reactions to increases in perceived personal control. Journal of Personality and Social Psychology, 56, 246-256.

Burger, J. M. (1992). Desire for control: Personality, social, and clinical perspectives. New York: Plenum.

Burger, J. M., & Arkin, R. M. (1980). Prediction, control, and learned helplessness. Journal of Personality and Social Psychology, 38, 482-491.

Burger, J. M., & Cooper, H. M. (1979). The desirability for control. Motivation and Emotion, 3, 381-393.

Buss, D. M. (1987). Sex differences in human mate selection criteria: An evolutionary perspective. In C. Crawford, M. Smith, & D. Krebs (Eds.), Sociobiology and psychology: Ideas, issues, and applications (pp. 335-351). Hillsdale, NJ: Erlbaum.

Cattell, R. B. (1966). The scree test for the number of factors. Multivariate Behavioural Research, 1, 245-276.

Cliff, N. (1988). The eigenvalues-greater-than-one rule and the reliability of components. Psychological Bulletin, 103, 276-279.

Cramer, K. M., & Nickels, J. B. (1988). The illusion of uncontrol: The other side to the illusion of control. Unpublished manuscript. University of Manitoba, Winnipeg.

Cramer, K. M., & Nickels, J. B. (1994). Make-a-difference metric and outcome predictability as explanations for depressive realism. Unpublished manuscript. University of Manitoba, Winnipeg.

Cramer, K. M., Nickels, J. B., & Gural, D. M. (1994). Lack of prediction, success and impact as three explanations of perceived helplessness. Unpublished manuscript. University of Mantioba, Winnipeg.

Dambrot, F. (1980). Test item order and academic ability or should you shuffle the test item deck. Teaching of Psychology, 7, 94-96.

Darlington, R. B., & Carlson, P. M. (1987). Behavioral statistics: Logic and methods. New York: The Free Press.

DeCola, J. P., Rosellini, R. A., & Warren, D. A. (1988). A dissociation of the effects of control and prediction. Learning and Motivation, 19, 269-282.

Dresel, K. M. (1985). Effects of the type A behaviour pattern, depression, and the duration of noncontrol on the illusion of control. Unpublished doctoral dissertation. University of Manitoba, Winnipeg.

Dweck, C. S., & Bush, E. S. (1976). Sex differences in learned helplessness: I. Differential debilitation with peer and adult evaluators. Developmental Psychology, 12, 147-156.

Dweck, C. S., Davidson, W., Nelson, S., & Enna, B. (1978). Sex differences in learned helplessness: (II) The contingencies of evaluative feedback in the classroom and (III) An experimental analysis. Developmental Psychology, 14, 268-276.

Dweck, C. S., & Gilliard, D. (1975). Expectancy statements as determinants of reactions to failure: Sex differences in persistence and expectancy change. Journal of Personality and Social Psychology, 32, 1077-1084.

Dweck, C. S., Goetz, T. E., & Strauss, N. L. (1980). Sex differences in learned helplessness: (IV) An experimental and naturalistic study of failure generalization and its mediators. Journal of Personality and Social Psychology, 38, 441-452.

Dweck, C. S., & Licht, B. G. (1980). Learned helplessness and intellectual achievement. In J. Garber and M. E. P. Seligman (Eds.), Human helplessness: Theory and applications (pp. 197-221). New York: Academic Press.

Gatchel, R. J., Paulus, P. B., & Maples, C. W. (1975). Learned helplessness and the self-reported affect. Journal of Abnormal Psychology, 84, 732-734.

Gatchel, R. J., & Proctor, J. D. (1976). Physiological correlates of learned helplessness in man. Journal of Abnormal Psychology, 85, 27-34.

Geer, M., & Maisel, E. (1971). Evaluating the effects of the prediction-control confound. Journal of Personality and Social Psychology, 23, 314-319.

Gibbon, J., Berryman, R., & Thompson, R. L. (1974). Contingency spaces and measures in classical and instrumental conditioning. Journal of the Experimental Analysis of Behaviour, 21, 585-605.

Glass, D. C, Reim, B., & Singer, J. R. (1971). Behavioural consequences of adaptation to controllable and uncontrollable noise. Journal of Experimental Social Psychology, 7, 244-257.

Glass, D. C., & Singer, J. R. (1972). Urban stress: Experiments on noise and social stressors. New York: Academic Press.

Graziano, A. M., & Raulin, M. L. (1993). Research methods: A process of inquiry (2nd ed.). New York: Harper Collins.

Guttormson, L. E. (1984). Differentiation of real prediction and real control by degree. Unpublished Master's thesis. University of Manitoba, Winnipeg.

Hammond, L. J., & Paynter, Jr., W. E. (1983). Probabilistic contingency theories of animal conditioning: A critical analysis. Learning and Motivation, 14, 527-550.

Hanusa, B. H., & Schultz, R. (1977). Attributional mediators of learned helplessness. Journal of Personality and Social Psychology, 35, 602-611.

Hiroto, D. S. (1974). Locus of control and learned helplessness. Journal of Experimental Psychology, 102, 187-193.

Hiroto, D. S., & Seligman, M. E. P. (1975). Generality of learned helplessness in man. Journal of Personality and Social Psychology, 31, 311-327.

Hokanson, J. E., DeGood, D. E., Forrest, M. S., & Brittain, T. M. (1971). Availability of avoidable behaviours in modulating vascular-stress responses. Journal of Personality and Social Psychology, 19, 60-68.

Jackson, R. L., & Minor, T. R. (1988). Effects of signalling inescapable shock on subsequent escape learning: Implications for theories of coping and "learned helplessness." Journal of Experimental Psychology: Animal Behaviour Processes, 14, 390-400.

Jenkins, H. M., & Ward, W. C. (1965). The judgment of contingency between responses and outcomes. Psychological Monographs, 79, (1, Whole No. 594).

Kaiser, H. F. (1970). A second-generation Little Jiffy. Psychometrika, 35, 401-415.

Keinan, G. (1994). Effects of stress and tolerance of ambiguity on magical thinking. Journal of Personality and Social Psychology, 67, 48-55.

Klein, D. C., Fencil-Morse, E., & Seligman, M. E. P. (1976). Learned helplessness, depression, and the attribution of failure. Journal of Personality and Social Psychology, 85, 11-26.

Klein, D. C., & Seligman, M. E. P. (1976). Reversal of performance deficits in learned helplessness and depression. Journal of Abnormal Psychology, 85, 11-26.

Langer, E. J. (1975). The illusion of control. Journal of Personality and Social Psychology, 32, 311-328.

Langer, E. J., & Rodin, J. (1976). The effects of control-relevant personal responsibility for the aged: A field experiment in an institutional setting. Journal of Personality and Social Psychology, 34, 191-198.

Maier, S. F., & Keith, J. R. (1987). Shock signals and the development of stress-induced analgesia. Journal of Experimental Psychology: Animal Behaviour Processes, 13, 226-238.

Maier, S. F., & Seligman, M. E. P. (1976). Learned helplessness: Theory and evidence. Journal of Experimental Psychology: General, 105, 3-45.

Maier, S. F., Seligman, M. E. P., & Solomon, R. L. (1969). Pavlovian fear conditioning and learned helplessness. In B. A. Campbell & R. M. Church. (Eds.), Punishment and aversive behaviour. New York: Appleton-Century-Crofts.

Maier, S. F., & Testa, T. J. (1975). Failure to learn to escape by rats previously exposed to inescapable shock is partly produced by associative interference. Journal of Comparative Physiological Psychology, 88, 554-564.

Martin, D. J., Abramson, L. Y., & Alloy, L. B. (1984). Illusion of control for self and others in depressed and nondepressed college students. Journal of Personality and Social Psychology, 46, 125-136.

Matute, H. (1994). Learned helplessness and superstitious behaviour as opposite effects of uncontrollable reinforcement in humans. Learning and Motivation, 25, 216-232.

Miller, I. W., & Norman, W. H. (1979). Learned helplessness in humans: A review and attribution theory model. Psychological Bulletin, 86, 93-118.

Miller, N. E., & Weiss, J. M. (1969). Effects of somatic or visceral responses to punishment. In B. A. Campbell & R. M. Church (Eds.), Punishment and aversive behaviour. New York: Appleton-Century-Crofts.

Miller, S. M. (1979). Controllability and human stress: Method, evidence, and theory. Behaviour Research and Therapy, 17, 287-306.

Miller, W. R., & Seligman, M. E. P. (1973). Depression and the perception of reinforcement. Journal of Abnormal Psychology, 82, 62-73.

Miller, W. R., & Seligman, M. E. P. (1975). Depression and learned helplessness in man. Journal of Abnormal Psychology, 84, 228-238.

Miller, W. R., & Seligman, M. E. P. (1976). Learned helplessness, depression, and the perception of reinforcement. Behaviour Research and Therapy, 14, 7-14.

Mineka, S., Cook, M., & Miller, S. (1984). Fear conditioned with escapable and inescapable shock: The effects of a feedback stimulus. Journal of Experimental Psychology: Animal Behaviour Processes, 10, 168-181.

Nicholls, J. G. (1975). Causal attributions and other achievement-related cognitions: Effects of task outcome, attainment value and sex. Journal of Personality and Social Psychology, 31, 379-389.

Nickels, J. B. (1980). That confounded confounding of prediction and control: Is predictionless control possible? Unpublished manuscript. University of Manitoba, Winnipeg.

Nickels, J. B., & Cramer, K. M. (1993). Is foreknowledge of control necessary for perceptions of control. Unpublished manuscript. University of Manitoba, Winnipeg.

Nickels, J. B., Cramer, K. M., & Gural, D. M. (1992). Toward unconfounding prediction and control: Predictionless control made possible. Canadian Journal of Behavioural Sciences, 24, 156-170.

Nickels, J. B., Gural, D. M., & Cramer, K. M. (1993). Choices exercised under prediction and blind control cues: Will the person with real control please stand up? Unpublished manuscript. University of Manitoba, Winnipeg.

Orne, M. T. (1962). On the social psychology of the psychological experiment with particular reference to the demand characteristics and their implications. American Psychologist, 17, 776-783.

Overmier, J. B., & Seligman, M. E. P. (1967). Effects of inescapable shock upon subsequent escape and avoidance learning. Journal of Comparative Physiological Psychology, 63, 23-33.

Peterson, C., & Villanova, P. (1988). An expanded Attributional Style Questionnaire. Journal of Abnormal Psychology, 97, 87-89.

Perlmuter, L. C., & Monty, R. A. (1977). The importance of perceived control: Fact or fantasy? American Scientist, 65, 759-765.

Peterson, C. (1993). Helpless behavior. Behavior Research and Therapy, 31, 289-295.

Peterson, C., Maier, S. F., & Seligman, M. E. P. (1993). Learned helplessness: A theory for the age of personal control. London, GB: Oxford University Press.

Rodin, J., & Langer, E. J. (1977). Long-term effects of a control-relevant intervention with the institutionalized aged. Journal of Personality and Social Psychology, 37, 897-902.

Rogers, J. L., Howard, K. I., & Vessey, J. T. (1993). Using significance tests to evaluate equivalence between two experimental groups. Psychological Bulletin, 113, 553-565.

Rosellini, R. A., DeCola, J. P., & Warren, D. A. (1986). The effect of feedback stimuli on contextual fear depends upon the length of the minimum ITI. Learning and Motivation, 17, 229-242.

Rosellini, R. A., Warren, D. A., & DeCola, J. P. (1987). Predictability and controllability: Differential effect upon contextual fear. Learning and Motivation, 18, 392-420.

Roth, S. (1980). A revised model of learned helplessness in humans. Journal of Personality, 48, 103-133.

Roth, S. & Kubal, L. (1975). Effects of noncontingent reinforcement on tasks of differing importance: Facilitation and learned helplessness. Journal of Personality and Social Psychology, 32, 680-691.

Rotter, J. B. (1966). Generalized expectancies for internal versus external control of reinforcement. Psychological Monographs, 80, (1, Whole No. 609).

Ryan, T. A. (1960). Significance tests for multiple comparison of proportions, variances, and other statistics. Psychological Bulletin, 57, 318-328.

Sayers, S. L., Baucom, D. H., & Tierney, A. M. (1993). Sex roles, interpersonal control, and depression: Who can get their way? Journal of Research in Personality, 27, 377-395.

Sedek, G., & Kofta, M. (1990). When cognitive exertion does not yield cognitive gain: Toward an informational explanation of learned helplessness. Journal of Personality and Social Psychology, 58, 729-743.

Seligman, M. E. P. (1975). Helplessness. San Francisco: Freeman.

Seligman, M. E. P., & Maier, S. F. (1967). Failure to escape traumatic shock. Journal of Experimental Psychology, 74, 1-9.

Seligman, M. E. P., Maier, S. F., & Geer, J. (1968). The alleviation of learned helplessness in the dog. Journal of Abnormal Psychology, 73, 256-262.

Seligman, M. E. P., Maier, S. F., & Solomon, R. L. (1971). Unpredictable and uncontrollable aversive events. In F. R. Brush (Ed.), Aversive conditioning and learning. New York: Academic Press.

Staub, E., Tursky, B., & Schwartz, G. E. (1971). Increasing pain tolerance by information about aversive stimuli. Journal of Personality and Social Psychology, 21, 198-203.

Tabachnik, B. G., & Fidell, L. S. (1989). Using multivariate statistics (2nd ed.). New York: Harper & Row.

Tang, K. S., & Critelli, J. W. (1990). Depression and judgment of control: Impact of a contingency on accuracy. Journal of Personality, 58, 717-727.

Tennen, H., & Eller, S. J. (1977). Attributional components of learned helplessness and facilitation. Journal of Personality and Social Psychology, 35, 265-271.

Thompson, S. C. (1981). Will it hurt less if I can control it? A complex answer to a simple question. Psychological Bulletin, 78, 237-240.

Vázquez, C. (1987). Judgment of contingency: Cognitive biases in depressed and nondepressed subjects. Journal of Personality and Social Psychology, 52, 419-431.

Volpicelli, J. R., Ulm, R. R., & Altenor, A. (1984). Feedback during exposure to inescapable shocks and subsequent shock-escape performance. Learning and Motivation, 15, 279-286.

Ward, W. C., & Jenkins, H. M. (1965). The display of information and the judgment of contingency. Canadian Journal of Psychology, 19, 231-241.

Weiss, J. M. (1971). Effects of punishing the coping response (conflict) on stress pathology in rats. Journal of Comparative Physiological Psychology, 77, 14-21.

Wilson, E. O. (1978). On human nature. New York: Bantam.

Wortman, C. B. (1975). Some determinants of perceived control. Journal of Personality and Social Psychology, 31, 282-294.

Wortman, C. B., Panciera, L., Shusterman, L., & Hibscher, J. (1976). Attributions of causality and reactions to uncontrollable outcomes. Journal of Experimental Social Psychology, 12, 301-316.

Zwick, W. R., & Velicer, W. F. (1986). Comparison of five rules for determining the number of components to retain. Psychological Bulletin, 99, 432-442.

# Appendix A

## Participants' Instruction Booklet

PAGE 1

# I N S T R U C T I O N   S H E E T S

Please do NOT turn the page

until instructed

PAGE 2

**SAMPLE 24-QUESTION ANSWER KEY**

| Question | Option-A | Option-B | |
|---|---|---|---|
| 1 | (A) | B | A is the correct answer |
| 2 | A | (B) | B is the correct answer |
| 3 | (A) | (B) | Both A and B are correct |
| 4 | A | B | Neither A nor B is correct |
| . | . | . | |
| . | . | . | |
| 24 | . | . | |

**<u>STOP</u>: Please do <u>not</u> turn the page.**

**Sit quietly and wait for further instructions.**

In your particular case, you should know 2 things about the answer key:

**You will get 18 more correct answers**
**by selecting one of the options (either A or B)**
**rather than the other option.**
**We cannot tell you which option has the larger number**
**of correct answers.**

Please note that if you always select the option with the larger number of correct answers, you get a high final test score. However, if you always select the option with the smaller number of correct answers, you will get a low final test score. Because you do not know which option gives more correct answers, a good strategy is to try a relatively equal selection from both A and B.

**6 of the questions have only 1 correct answer.**
**The remaining questions have either**
**0 or 2 correct answers.**
**We cannot tell you which questions have 0**
**and which have 2 correct answers.**

That is, on 6 questions your choice of A or B makes a difference in whether you get the correct answer.

You will later see that we have labelled each of the 24 questions on your sheet so you know on which questions your choice makes a difference (i.e., only 1 correct answer).

**Please continue on to the next page.**

**FORM-A** **PAGE 4**

You may now begin to select either A or B for each question, trying to get the correct answer. **Remember, you get <u>18 more correct answers</u> by selecting one option (A or B) rather than the other option.**

FOR EACH OF THE 24 QUESTIONS BELOW, CIRCLE EITHER 'A' or 'B'

| Question | OPTION-A | OPTION-B | YOUR CHOICE... |
|---|---|---|---|
| 1 | A | B | makes a difference |
| 2 | A | B | makes NO difference |
| 3 | A | B | makes NO difference |
| 4 | A | B | makes a difference |
| 5 | A | B | makes a difference |
| 6 | A | B | makes NO difference |
| 7 | A | B | makes NO difference |
| 8 | A | B | makes NO difference |
| 9 | A | B | makes NO difference |
| 10 | A | B | makes NO difference |
| 11 | A | B | makes NO difference |
| 12 | A | B | makes a difference |
| 13 | A | B | makes NO difference |
| 14 | A | B | makes NO difference |
| 15 | A | B | makes NO difference |
| 16 | A | B | makes a difference |
| 17 | A | B | makes NO difference |
| 18 | A | B | makes a difference |
| 19 | A | B | makes NO difference |
| 20 | A | B | makes NO difference |
| 21 | A | B | makes NO difference |
| 22 | A | B | makes NO difference |
| 23 | A | B | makes NO difference |
| 24 | A | B | makes NO difference |

Please circle only one option for each question.

On how many of the above 24 questions did your choice of A or B make a difference?

______________
0 - 24

Please continue on to the next page.

PAGE 5

# QUESTIONNAIRE

**AGE (years):** ___________ **SEX (M/F):** _______

1. To what extent have you exercised <u>control</u> over the magnitude of your final test score? (Circle the appropriate number).

minimum amount | 1 2 3 4 5 6 7 | maximum amount

2. To what extent have you been <u>helpless</u> to affect the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

3. To what extent do you consider yourself to be a <u>success</u> because of the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

4. To what extent do you consider yourself to be a <u>failure</u> because of the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

5. To what extent <u>can you predict</u> the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

6. To what extent <u>are you responsible</u> for the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

7. To what extent <u>did you influence</u> the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

8. To what extent is it <u>hopeless</u> for you to anticipate the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

9. To what extent <u>do you believe you know</u> the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

**Please continue on to the next page.**

10. To what extent will you be <u>satisfied</u> with the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

11. To what extent do you deserve <u>credit or blame</u> for the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

12. To what extent will you feel <u>depressed</u> about the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

13. To what extent will you be <u>happy</u> with the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

14. To what extent do you feel <u>luck contributed to</u> the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

15. To what extent have you had an <u>opportunity</u> to affect the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

16. To what extent will you be <u>dissatisfied</u> with the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

17. To what extent do you feel <u>certain</u> about the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

18. To what extent will you be <u>pleased</u> with the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

19. To what extent will you be <u>sad</u> about the magnitude of your final test score?

minimum amount | 1 2 3 4 5 6 7 | maximum amount

**Please continue on to the next page.**

20. On how many of the 24 questions do you think you exercised control over the magnitude of your final test score?

__________
0 - 24

21. On how many of the 24 questions did your choice of A or B make a difference in whether or not you got the correct answer? (Please check back to page 3 in the booklet to get the answer).

__________
0 - 24

22. How many more correct answers would you get by constantly selecting one option (either A or B) rather than the other option? (Please check back to page 3 in the booklet to get the answer).

__________
0 - 24

23. Which option do you think had more correct answers? (Circle one of the three selections).

(A) (B) (Both the same)

24. How many correct answers do you think you would have earned if you had constantly selected option-A?

__________
0 - 24

25. How many correct answers do you thing you would have earned if you had constantly selected option-B?

__________
0 - 24

26. How many correct answers do you think you got?

__________
0 - 24

**STOP: Please make sure you have answered every question. When you have finished, please close your booklet and wait for further instructions.**

Appendix B

Experimenter Instructions

My name is Ken Cramer. I am the principle researcher in two separate studies that we will conduct today during the 1 hour research period. In the first study, we are trying to develop a new type of multiple choice test. In the second experiment, we are trying to find out how fast most people can perform a motor task, where you blacken all bubbles of several IBM answer sheets. The packet you have contains all you will need to participate in this experiment.

Multiple Choice Test

In the first study, we are interested in a new format of multiple choice test. While most multiple choice test questions have only one correct answer, we wish to examine the case where questions can also have either no correct answer or two correct answers. You will be asked later in the experiment to take this new multiple choice test. At this point, I would like you to do three things: (1) remove the INSTRUCTIONS SHEETS from your packet, (2) wait until I tell you to turn to page 2, and (3) leave everything else in the packet.

Many undergraduate courses at the university level use multiple-choice tests to evaluate student performance. This format usually involves one correct answer out of four or five options per question. A problem with this format is that at higher levels of learning (and in life, too,) many questions have any number of correct answers--or maybe even no correct answers. You are part of a group of students participating in a study to explore alternative formats to the traditional multiple-choice test. In this study, you are asked to take a test in which any question may have one, more than one, or no correct answers. Your task will be to get as many correct answers as you can.

We know that with the traditional multiple-choice test, students do NOT do well when they neglect to study for a test. The question is, "How well can students do on this new test format when they have not studied the material evaluated by the test?" In order to simulate the situation in which students have not studied the material at all, we have left out the actual wording of each question. You will be able to see only the question numbers and the two letters (A and B) representing the two choices for each question.

In each of your packets, we have included the test answer key in a sealed envelope, which you will be asked to open after the test is completed. We will use this key to determine how many correct answers you got (i.e., your total score). We will need to verify that your score is correctly calculated. To avoid the confusion of all 300 students coming up at once, we will do it in order or your score. The higher your score, the sooner you will be called up and leave the experiment.

Now open your instruction booklet to page 2. In this experiment, you are asked to circle 1 and only 1 option for each question. Page 2 shows an example of the 24-question answer key. Sample questions 1 and 2, like most multiple choice tests, represent instances when the answer sheet has only 1 correct answer. That is, for Sample question 1, you will get the correct answer if you select A, because A is the correct answer on the answer key. For Sample question 2, the same will occur if you select B, because B is the correct answer on the answer key. In each of these cases, your choice of A or B <u>makes a difference</u> in whether or not you get the correct answer.

The novel aspect of the new multiple choice test, however, is demonstrated in sample questions 3 and 4. For Sample question 3, you will <u>always</u> get the correct answer, because <u>both</u> A and B are correct on the answer key. However, for Sample question 4, you will <u>never</u> get the correct answer, because neither A nor B is correct on the answer key. You will get the same outcome regardless of your selection of A or B. That is, your <u>choice</u> of A or B <u>makes no difference</u> in whether or not you get the correct answer. You should know that for these questions, you should still select either A or B, since not answering will count against you.

As I have already said, you will be able to check your answers against the answer key. At this point, however, please turn to page 3 in your instruction booklet, read these instructions carefully to yourself, and begin the test by following the instructions.

Motor Task

At this point, I would like all of you to follow these 3 instructions: (a) place the instruction booklet inside the packet, (b) remove the IBM answer sheets and the sealed envelope from the packet (do not open the envelope) and (c) return the packet under your seat. When you have finished, please look to the front of the class.

You will be able to examine the answer key after we complete the second experiment involving the motor task; that way, both experiments will be completed and you will be called up individually with higher score participants being called before lower score participants. When I say "BEGIN," please blacken **as quickly as you can** EACH bubble on the answer sheet. Be sure to blacken the bubbles on the answer sheet question by question. For example you are to blacken each of the 5 bubbles in question 1 before proceeding to blacken each of the 5 bubbles in question 2, and so on. Computer-scoring will NOT count a partially blackened bubble but will count stray marks against you, so **be sure to completely fill in each bubble separately.** Otherwise, you may get a lower score than you deserve. Go on to a new answer sheet once you have completely filled in a sheet. When I say "STOP," I will ask you to open your sealed envelope. Please work as fast as you can during each minute of the task. Are there any questions before we start?

All right. Ready? Begin! [SET TIMER FOR 1 MINUTE]

Please stop.

I can now reveal that there will be NO need for any of you to do any further work in order to get credit for your participation in this study. All the data to be gathered have now been gathered. However, before you leave:

(a) please return the IBM answer sheets to the packet,

(b) seal the envelope so none of the material will be lost, and,

(c) write your name and student number on the outside of the envelope (so you can get credit for your participation).

Those of you who wish to see how you did on the multiple choice test are asked to wait until the others have left. Those not wishing to check their multiple choice test scores should now pass their packets to the end of the row, where we will pick them up.

Data Pick-Up

The results of the study should be available soon. When they are available, I will share them with you. Thank you very much for your participation. If you have any questions or want to score your own test, please stay in your seat. Thanks again! Those who have already handed in their packets may now leave.