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A Meta-Analysis of Learner Control In Computer-Based Learning Environments

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A META-ANALYSIS OF LEARNER CONTROL IN
COMPUTER-BASED LEARNING ENVIRONMENTS

June A. Parsons

A dissertation submitted to Nova University in
partial fulfillment of the requirements for the degree
of Doctor of Education

Nova University
Center for Computer and Information Sciences
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ABSTRACT

The objective of this meta-analysis was to integrate the results of a collection of primary research studies on learner control of computer-based environments. The scope for the meta-analysis was limited to inquiry on different learner-control models and their effect on student achievement as represented by posttest scores. Three specific research questions were defined:

1. What are the characteristics of the body of learner-control research which has examined the effect on achievement of learner control?

2. Is there a difference in the achievement of students who are provided with learner control and students who are provided with other control models?

3. Do specific moderator variables interact with learner control to produce different achievement effects?

Learner-control research was found to be characterized as using a posttest-only control group design in which students were exposed to one treatment session lasting a little less than an hour. Typically, the achievement of students who were provided with control over four instructional factors was compared with the achievement of students who were provided with control over two instructional factors. The Apple II

was the most frequently used hardware platform and science was selected most frequently as the topic of instruction. College students were most frequently selected for the subject pool.

The average effect of providing more learner control to students using computer-based courseware was to decrease achievement by .04 standard deviation, an amount generally considered negligible within the educational domain. The negligible effect suggested that achievement under learner control was essentially the same as achievement under other control options.

The topic of instruction and the researcher were found to be possible moderator variables. It was suggested that the moderating effects of these two variables might be associated with the quality of the courseware.

As a result of the analysis, learner control of computer-based learning environments was recommended as a viable pedagogy with the caveat that learner control is likely to produce student achievement which is similar to, but not better than student achievement under other control options.

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CHAPTER 1

INTRODUCTION

Statement of the Problem

It is not known how learner control of computer environments affects student achievement. Research findings to date have been characterized as inconsistent. Consequently, courseware designers and courseware evaluators seem not to have conclusive findings on which to base design or selection decisions. The problem addressed by this dissertation was that of the apparently contradictory findings which have been attributed to the body of learner-control research.

Problem Background

The increasing use of computers for instructional delivery in corporate training centers, in the military, in colleges and universities, in public and private schools, in Computer Information Systems curricula, as well as other curricula across grade levels and disciplines, has opened the door for learner-centered educational alternatives once thought "impractical" in a traditional classroom setting. Consequently, there has been renewed interest in alternative theories of education and their potential for implementation via computer. One area of interest

is in providing learner control of computer-based learning environments as a means of individualizing instruction and increasing instructional effectiveness.

Under the learner-control strategy, it is the student who makes the instructional decisions, rather than the instructor or the computer acting as a surrogate instructor. From the perspective of the educational software designer, learner-control software enables the student to manipulate control factors such as pacing, interaction, amount of material, review, number of exercises, sequence of instruction, or cognitive format.

Learner control of the pace of instruction enables the student to regulate the amount of time used to study a screen of information. Interaction enables the student to make entries which are evaluated and responded to by the software. Learner control of the review process enables students to backtrack through previously viewed instructional sequences. Learner control of the amount of material enables the student to ask for more examples to clarify concepts. Learner control of the number of exercises enables the student to practice until he or she has reached a personally defined level of mastery. Learner control of the instructional sequence enables the student to select the next topic of instruction. Learner control of

cognitive format enables the student to request alternative explanations suitable for different cognitive styles: graphics instead of text, deductive instead of inductive, multi-media instead of audio-only, and so forth.

Learner control was conceived as a means of individualizing instruction and it was assumed that providing a flexible learning environment would result in maximizing the learning potential of each student. In a review of learner-control research, Steinberg (1977) indicated that researchers hypothesized that learner control would improve student attitudes, reduce instructional time, and improve performance. Some, but not all of the research findings about learner control have supported these hypothesized benefits. This has lead reviewers and researchers to characterize the body of research on learner control as contradictory, inconclusive, ambiguous, and inconsistent. Carrier and Williams (1988) sum up the research in learner control by saying "Findings from empirical investigations of learner-control strategies do not lend unequivocal support for their use... Instead, as a whole these findings present a montage of inconsistencies, contradictions, and caveats" (p. 286).

But is this an accurate depiction of the learner-control research? Is the research contradictory,

ambiguous, inconclusive, and inconsistent, or are the apparently divergent findings, in fact, a product of sampling error or other research artifacts? Are there really no findings which can be translated into practice, or are there answers which are obscured by the volume of studies and methodological diversity? Might it be the case that learner control has neither a positive nor a negative effect on student achievement?

In light of these questions, it would seem appropriate to carefully examine this body of research to discover if there are answers yet to be culled from its depths.

Purpose and Rationale

The purpose of this dissertation was to examine the body of learner-control research in an attempt to accurately characterize its findings about the effect of learner control on student achievement. The questions considered in this study are important for the following reasons:

First, contradictory findings are not particularly useful to practitioners because they do not provide concrete principles which can be translated into practice. If the findings of the compendium of learner-control research are, indeed, contradictory, there is a mandate for further study in the area. If the findings are not contradictory, an examination of

the findings should resolve several questions important to software designers and teachers, such as: Is learner control effective in certain conditions, but not effective in others? Under what conditions, if any, is learner control effective? Is the effectiveness of learner control influenced by factors such as student age, instructional topic, software quality, student aptitude, student interest, or type of computer equipment?

Second, research integration and review is becoming increasingly important under the burgeoning volume of published research. Practitioners who would rather not base decisions on subjective judgement and intuitive rationale are faced with a serious information processing task. A practitioner considering the use of learner control and looking for answers about its effectiveness will need to locate the hundreds of studies catalogued in databases and referenced by other researchers, select those which are relevant, and then attempt to reconcile the findings. An accurate summary of learner-control findings would provide practitioners with a convenient reference source.

Third, resolution of learner-control issues may have economic implications. In addition to the primary pedagogical concern about learning effectiveness,

educational software designers generally find it useful to consider the economic issues associated with a design project. Educational software which utilizes learner control to provide individualized instruction may yield economic dividends. Lippert (1989) noted that the rule of thumb for development time of computer-aided instruction (CAI) is 200-300 hours for each hour of instruction. Intelligent computer-aided instruction (ICAI) which can diagnose a learner's ability and learning style, then use this diagnosis as the basis for individualized instructional delivery, would require more than 1500 developmental hours for each instructional hour. If students can monitor their own learning and make effective decisions to, in effect, individualize their own learning programs, the necessity for expensive ICAI systems may be reduced.

From the practitioner's perspective, the learner-control issue is inherent in the theoretical and economic realm of courseware development and selection. An accurate characterization of the body of learner-control research and clarification of the effectiveness of learner control would appear to be an important step in the utilization of computers in the educational setting.

Investigative Technique

In order to examine the body of learner-control research, this dissertation employed an investigative technique called meta-analysis which is a statistical analysis of the findings of many individual studies. Meta-analysis provides an alternative to traditional informal narrative techniques for research review and integration.

As Glass, McGaw, and Smith (1981) pointed out, narrative research reviews are often influenced by prejudice and stereotyping which would be unforgivable in primary research. The sheer volume of research, the disparate terminology, and the use of diverse outcome measures makes it difficult, if not impossible, to apply heuristic reasoning to produce an accurate narrative integration.

Meta-analysis methodology was developed to provide a more rigorous technique for integrating the results of a number of research studies. This methodology may, in addition, provide an appropriate tool for discovering if conflicting results in the literature are due to real differences or if they are, instead, due to sampling error or other study artifacts (Hunter, Schmidt, and Jackson, 1982, p. 19).

To use meta-analysis for an integration of findings on the effect of learner control in computer-

based learning environments, the quantitative results for each treatment group in a sample of independent learner-control studies were collected and subjected to statistical procedures, which, after eliminating sampling error, were designed to reveal what Hunter, Schmidt, and Jackson (1982) described as "the hidden facts that can be proven under the cumulative weight of previous studies" (p. 143).

Research Questions and Hypotheses

Using meta-analytic techniques, this dissertation attempted to integrate the findings of empirical studies on the effect of learner control on student achievement. Of particular interest was whether the learner-control research could appropriately be characterized as ambiguous, inconsistent, inconclusive and contradictory or whether it offered resolution on some learner-control issues. Studies were examined in which students in one treatment group were provided with control over more or different factors in the computer-based learning environment than students in another treatment group. The analysis addressed the following question sets:

1. What are the characteristics of the body of learner-control research which has examined the effect on student achievement of learner control?

1. a. How many studies have been published?

1. b. When were the studies performed?
1. c. Who were the major researchers on this topic?
1. d. What variables have been researched?
1. e. What types of research designs were employed?
1. f. What was the quality of the research?

This first set of research questions was descriptive in nature and therefore, no hypotheses were advanced.

2. Is there a difference in the achievement of students provided with learner control and students who are provided with other control models?

2. a. What is the effect on student achievement of providing learner control?

2. b. Do specific learner-control configurations have an effect on student achievement?

2. c. What is the effect on achievement of providing specific learner-control factors such as pace, interaction, review, or sequencing?

This second set of research questions had the following associated hypotheses:

Effect of Learner Control on Achievement

H₁ Providing learner control has an effect on student achievement.

$H_{(0)1}$ Providing learner control does not have an effect on student achievement.

Specific Learner-Control Configurations

H_2 Specific learner-control configurations have an effect on student achievement.

$H_{(0)2}$ Specific learner-control configurations do not have an effect on student achievement.

Learner-Control Factors

H_3 Providing specific learner-control factors has an effect on achievement.

$H_{(0)3}$ Providing specific learner-control factors does not have an effect on achievement.

3. Do specific moderator variables interact with learner control to produce different achievement effects?

3. a. Does student age or class in school interact with learner control to produce different effect sizes for student achievement?

3. b. Does the topic of instruction interact with learner control to produce different effect sizes for student achievement?

3. c. Does the type of computer equipment used for the study interact with learner control to produce different effect sizes for student achievement?

3. d. Do the instructional media interact with learner control to produce different effect sizes for student achievement?

3. e. Are the effect sizes for student achievement under learner control in published studies significantly different from those reported in unpublished studies or from those reported in dissertations?

3. f. Do effect sizes for student achievement under learner control differ by experimental design?

3. g. Do effect sizes for student achievement under learner control differ by study date?

3. h. Do effect sizes for student achievement under learner control differ by researcher?

3. i. Does the length of exposure to learner-control treatments have an effect on achievement?

This third set of research questions had the following associated hypotheses:

Student Age/Class in School

H₄ Student class in school interacts with learner control to produce different effect sizes for student achievement.

$H_{(0)4}$ Student class in school does not interact with learner control to produce different effect sizes for student achievement.

Topic of Instruction

H_5 The topic of instruction interacts with learner control to produce different effect sizes for student achievement.

$H_{(0)5}$ The topic of instruction does not interact with learner control to produce different effect sizes for student achievement.

Type of Computer Equipment

H_6 The type of computer equipment used for the study interacts with learner control.

$H_{(0)6}$ The type of computer equipment used for the study does not interact with learner control.

Instructional Media

H_7 The instructional media interact with learner control.

$H_{(0)7}$ The instructional media do not interact with learner control.

Publication Type

H_8 Effect sizes from published studies on learner control differ from those reported in unpublished studies or from those reported in dissertations.

$H_{(0)8}$ Effect sizes from published studies on learner control do not differ from those reported in unpublished studies or from those reported in dissertations.

Experimental Design

H_9 Effect sizes for student achievement under learner control differ by experimental design type.

$H_{(0)9}$ Effect sizes for student achievement under learner control do not differ by experimental design type.

Study Date

H_{10} Effect sizes for student achievement under learner control differ by study date.

$H_{(0)10}$ Effect sizes for student achievement under learner control do not differ by study date.

Researcher

H_{11} Effect sizes for student achievement under learner control differ by researcher.

$H_{(0)11}$ Effect sizes for student achievement under learner control do not differ by researcher.

Length of Treatment

H_{12} The length of treatment under learner control has an effect on student achievement.

$H_{(0)12}$ The length of treatment under learner control does not have an effect on student achievement.

Dissertation Scope, Assumptions, and Limitations

The scope of this dissertation was three-fold. First, it attempted to broadly characterize a collection of primary studies of learner control. Second, it examined the effect on achievement of providing learner control. Third, it examined the interaction between selected variables, learner control, and student achievement. The limitations and assumptions which further defined the scope of the dissertation are discussed in the next sections.

The Available Sample of Studies

The sample of studies used for this analysis was obtained through an exhaustive search of indexes, databases, and references as detailed in Chapter 3. Both published and unpublished studies were included. Though it is unlikely that all learner-control studies were located, appropriate statistical procedures were applied during the meta-analysis to correct for the possibility of sampling error.

Accurate Reporting

The data used for this dissertation were the reported outcome measures from a sample of primary research studies on learner control. It was assumed

that the researchers correctly reported the results of their studies, thereby providing accurate data for input into the meta-analysis.

Achievement as the Dependent Variable

The success of an educational technique may be measured by a variety of variables including achievement, attitude, and efficiency. Achievement was the only dependent variable examined by this dissertation. For purposes of the analysis, achievement was defined by student scores obtained on immediate posttests covering the concepts presented in the computer-based lesson. Only those learner-control studies which reported student achievement scores were included in the analysis.

Analysis Limited to Computer-based Environments

Learner-control issues are not restricted to computer learning environments. However, the scope of this meta-analysis was limited to computer-based environments and to specific aspects, here identified as control factors, of these environments which might be of particular interest to the courseware designer or to the instructor evaluating software. The seven control factors of computer-based learning environments which were examined are pacing, interaction, review, number of examples, amount of material, sequence, and cognitive format. Computer-based environments may

include additional media such as audio cassette, paper-based supplements, or video disk. Studies employing such multi-media environments were included in the data set as long as the primary controlling element of the learning environment was the computer.

A Comparison of Different Types of Learner Control

The purpose of this study was to examine the effect of providing learner control. In the discussion in Chapter 2, it will be seen that the issue is not the effectiveness of learner control versus the effectiveness of no learner control. Rather, it is a question of the effectiveness of different degrees or configurations of learner control. Hence, studies selected for this meta-analysis were required to be designed such that in one of the treatment groups, subjects were provided with control over more factors in the learning environment than subjects in another treatment group. Studies which examined the effect of a *single* mode of learner control on different student populations were not included.

Cognate Research Questions

There are several issues cognate to learner control, but not directly addressed by this meta-analysis. They are presented here to add perspective and to reinforce some of the limitations of using research findings for practical applications.

The studies considered in this meta-analysis focused on computer-based learning environments. It may, however, be appropriate to question whether computer-based environments are somehow unique in terms of the learner-control options they present and in terms of student response to those options. Are there elements of a computer-based learning environment which uniquely interact with learner-control factors, or is the theory of learner control similar across delivery methods?

Many of the studies included in this meta-analysis were carried out during the "dawn of the personal computer." Some of the studies pre-date the personal computer and were carried out in mainframe environments. Has the design of courseware been limited by hardware factors which, until improved, will require design compromises with potentially negative effects on the learning process? Is it appropriate to base future design decisions on the results of studies which used courseware units that were possibly crippled by hardware limitations?

Most of the studies included in this meta-analysis contained adequate descriptions of selecting subjects, defining treatment groups, administering treatments, and analyzing results. However, descriptions of the instructional instrument -- the courseware itself --

were sketchy at best. In most instances, it would not be possible to evaluate the quality of the courseware. How significant are the qualitative aspects of courseware design? Given optimal information on the type and level of learner control that is appropriate for an individual student, are there additional design wild cards which can affect the quality of the courseware and hence, the learning process?

Obviously, the questions surrounding learner control and courseware design are complex. The scope of this dissertation encompassed the fairly broad perspective of characterizing the body of learner-control research and a more narrowly defined objective to examine whether specific variables interacted with learner control in order to affect student achievement. It was expected that the findings would add to our understanding of learning theory.

Definitions of Terms

Adaptive computer control. Adaptive computer control is based on adaptive treatment interaction (ATI) theory and provides individualization of the learning environment by obtaining estimates of student learning needs during instruction, then using those estimates to make instructional prescriptions (Atkinson, 1976). An overview of five adaptive models can be found in Park and Tennyson (1983).

Computer-Aided Instruction (CAI). Though there are many nuances in the use of the term computer-aided instruction, in the context of this meta-analysis, CAI refers to the use of a computer to deliver subject matter content.

Computer-Based Learning Environments. This term is used within this dissertation to refer to a collection of hardware, software, and media components assembled for the purpose of providing students with learning opportunities.

Computer control. In the computer-based learning environment, the computer program may act as a surrogate teacher, offering information, providing practice problems, and evaluating learner performance. Computer control of an aspect of the learning environment means that the computer, rather than the student, makes the instructional decision. Computer control of pacing, for example, means that the computer displays a screen of information for a set period of time, rather than giving the student the option to press a key to indicate readiness to proceed.

Courseware. Courseware, as used in the context of this dissertation, refers to computer software which is used as an instructional delivery system. A synonym for courseware is educational software.

Hypertext. Hypertext is computer software which employs a three-dimensional model for text storage and access. In many implementations, hypertext is characterized by "buttons" or "hot spots" which the user can select to obtain additional context-sensitive reference material.

HyperCard. HyperCard is a software package which implements a limited version of hypertext.

Hypermedia extends the concept of hypertext to include additional media such as video, sound, and graphics.

Intelligent computer-aided instruction (ICAI). An emerging area of interest, ICAI would be classified as a special instance of adaptive computer control in which the computer attempts to analyze the needs of the individual learner and provide appropriate content, exercises, and feedback. A good discussion of developments in ICAI can be found in Self (1988).

Learner control. Merrill (1984) defined learner control as providing the learner, rather than the computer or the instructor, with the ability to make adaptive instructional decisions which result in learning activities being matched to the unique aptitudes and abilities of the individual student.

Meta-analysis. Meta-analysis is a quantitative methodology for integrating the results of many

empirical studies which address the same research question.

Non-adaptive computer control. Instruction based on the non-adaptive model is a philosophical descendant of Skinnerian learning theory. Also called the "child robot" model by Snow (1980), non-adaptive computer control is characterized by fixed tasks, fixed treatment, computer control of pacing, no remediation, and educational goals imposed by the institution.

Programmed instruction. A variation of the child-robot model can be found in early teaching machines, which were not necessarily computers, featuring programmed instruction. Programmed instruction was an attempt to provide individualization by giving the student control over the pacing of the lesson, but not over other factors relevant to instruction.

Summary

Learner control was described as a potentially effective means of individualizing instruction. However, the findings of learner-control research have been characterized as inconsistent, contradictory, ambiguous, and inconclusive. Practitioners, looking for concrete principles on which to base courseware design, may not find this characterization particularly useful. Hunter, Schmidt, and Jackson (1982) have suggested that in some cases, apparent contradictions

in the research are the result of sampling error or other study artifacts. This opens the possibility that some of the apparent contradictions in the learner-control research do not exist. To examine this possibility, this dissertation utilized a quantitative review technique called meta-analysis.

The scope of the dissertation was three-fold. First, it attempted to broadly characterize a collection of primary studies of learner control. Second, it examined the effect on achievement of providing learner control. Third, it examined the interaction between selected variables, learner control, and student achievement.

The chapters which follow elaborate on the findings of learner-control research, present background material on the use of meta-analysis as a tool for research integration, detail the design of the present study, present the results of the analysis, and discuss the implications of these results.

CHAPTER 2
REVIEW OF THE LITERATURE

Introduction

In this chapter, two topics are presented. The first section of the chapter is a discussion of learner-control research, covering research assumptions, experimental designs, and specific instances of the inconsistent findings which appear to characterize this body of research. The purpose of this section is to point out the pervasiveness of the notion that learner-control research is inconsistent, contradictory, ambiguous, and inconclusive. A variety of variables associated with learner-control research are introduced in order to illustrate the complexity of the research domain and the rather difficult task which faces the reviewer armed only with traditional narrative methodology. Hence, the material in the first section of this chapter is designed to support the suggestion made in Chapter 1 that meta-analytic techniques might help integrate disparate learner-control study findings into a more coherent picture.

The second section of this chapter is a discussion of meta-analysis which includes the historical rationale for the use of meta-analysis as a tool for research integration, the increasing use and acceptance of meta-analytical methodology, an introduction to the

basic methodology of meta-analysis, a summary of the major types of meta-analysis, and a discussion of the issues and methodological concerns associated with the use of meta-analysis. The purpose of this section is to provide the reader with conceptual background pertaining to the methodology used in this dissertation as it is detailed in Chapter 3.

Learner-Control Studies

The Basic Framework

According to Merrill (1984), the term learner-controlled instruction first appeared in 1961 as the title of a book authored by Mayer and McCann (1961). As then used, the term referred to the opportunity for students to sequence the objectives within a particular course in any order desired.

Since its introduction, the term learner control has expanded to include characteristics other than sequencing, such as pacing, selection, and format. Snow (1980) suggested that learner control referred to a continuum of control strategies by which the learner could exercise control over the characteristics of the learning environment and pointed out that learners always exercise some degree of control over their own overt and covert learning activities; even in a traditional lecture session, for example, the student

may select or reject material by electing to listen, take notes, day dream, and so forth.

From the perspective of the teacher, instructor, or trainer, learner control may be defined as a continuum of instructional strategies in which the learner is provided with the option for controlling one or more of the parameters of the learning environment.

Early studies of learner control in non-computer environments appeared to provide a general endorsement for the learner-control concept (Mager, 1961; Mager & Clark, 1963; Campbell, 1964), but a study by Fry (1972) disputed earlier findings, pointing out that for some students, learner control reduced achievement.

Learner-control studies using computers as the delivery medium appeared in the early 1970s (Judd, Bunderson, & Bessent, 1970; Oliver, 1971; Atkinson, 1972; Hansen, 1974; Tobias & Duchastel, 1974; Fisher, Blackwell, Garcia, & Greene, 1975; Seidel, 1975). In a 1977 review of learner control in computer assisted instruction, Steinberg (1977) indicated that there were strong a priori assumptions on the part of many researchers that learner control would benefit students by reducing anxiety, increasing task engagement, increasing learning speed, improving attitudes, and improving performance. However, the results of some empirical studies (Judd et al., 1970; Fisher et al.,

1975) did not support the assumption that learner control would improve performance, and instead showed that students under learner control had lower achievement scores than students under computer control.

It was suggested that these early studies did not take into account the appropriate constellation of variables which might impact the effectiveness of learner control. Snow (1980) stated that "individual characteristics not under control of the student will determine, to a significant extent, what and how much that individual will learn in a given instructional setting" (p. 152). Subsequently, researchers introduced a variety of variables, interactions, and outcome measures to the learner-control experimental designs.

The experimental variables in learner-control studies may be classified as belonging to one of two groups: student variables or courseware variables.

Student variables are associated with characteristics or traits inherent in individual learners. Many of these learner characteristics are considered difficult, or impossible to alter by external forces and hence, may be used as independent variables in learner-control research. Student variables include cognitive style, age, intrinsic

motivation, inquisitiveness, personality, and aptitude. Studies which focus on student variables often examine a research question of the general format: Given environment X, how do outcomes differ for students with trait Y or Y¹? For example, in a simple experimental design, two groups of students, differentiated on the basis of a single student-variable such as personality type, would be exposed to the same learner-controlled computer environment. Outcome measures would then be analyzed to determine if the student variable did, in fact, appear to influence the outcome.

Courseware variables are associated with the design of a particular educational software. The software designer has control over the specification of these variables which, from a global design perspective, might include the use of color, the inclusion of animation, the size and design of the screen font, the density of text on the screen, and the choice of input device. Within the scope of learner-control research the courseware variables which are generally studied include pacing, amount of feedback, review capability, sequencing, amount of material, number of exercises, and style of presentation.

Research on courseware variables is often framed within the general question: Given a naturally occurring group of students, is software design A or A¹

more effective? In a simple experimental design for studying a learner-control courseware variable, it would be assumed that two groups of students are homogenous for experimental purposes. One group of students would use courseware which allowed learner control of the courseware variable. Another group of students would use courseware which did not provide learner control of the variable. Differences in the outcome measure would be attributed to the difference in type of learner control.

Some studies have examined the interaction between different levels of learner control and one or more student variables. A typical 2 X 2 experimental design might analyze high-aptitude versus low-aptitude students interacting with learner-controlled courseware and program-controlled courseware.

It is designs of the latter two types which are the focus of this meta-analysis, specifically because they test different levels of learner control.

Past Findings: "A Montage of Inconsistencies"

In a quote cited earlier, Carrier and Williams (1988) described the findings from empirical investigations of learner-control strategies as "a montage of inconsistencies, contradictions, and caveats" (p. 286). This is an expression of a recurring theme in the learner-control literature.

Commentators, reviewers, and researchers have continually referred to the inconsistent nature of findings in learner-control research.

In 1977, Steinberg included the following statement in her review of student control in computer-assisted instruction:

Because the database is inadequate and the experimental results are highly variant, it is not possible to make generalizations regarding the locus of control in CAI. Some topics were investigated by just one study. Other topics were studied by several researchers, but the results were contradictory." (p. 88)

After Steinberg's review, the database of research expanded, but resolution of the basic research question still appeared elusive. In 1985, Goetzfried and Hannafin stated that "the locus of instructional control in CAI design, whether to provide learner, computer, or combined control, has been a recurring, but as yet unresolved issue" (p. 273). The same year, Holmes, Robson and Steward (1985) observed that "the studies of the effects of giving [content] control that have been undertaken have shown mixed results" (p. 101).

The same refrain was heard in 1986. In the introduction to a study of learner control and prior

understanding in computer assisted video instruction, Gay (1986) wrote, "The results from experimental studies have been contradictory" (p. 225). Duchastel (1986) voiced the frustration of many practitioners who have attempted to apply the findings of empirical research, "As with most complex issues, generalizations regarding basic conclusions [in learner control] are difficult to come by" (p. 380).

In 1988, Rowland and Stuessy somewhat casually described the learner-control research as "a mixed bag". The next year, Kinzie and Sullivan (1989) wrote what was by then the defacto doctrine, "Research indicates mixed results for learner control as it relates to achievement" (p. 6).

Specific examples of these mixed results are numerous. The following discussion is not intended to be an exhaustive review of learner-control research, but rather an illustration of some of the apparent contradictions in the learner-control research literature. The intent of this section is twofold. First, it is designed to establish the need for rigorous research integration techniques such as those provided by meta-analysis. Second, it will provide a framework of research variables which will be utilized in the meta-analysis as detailed in Chapter 3.

A study by Tennyson (1981) and a study by Carrier and Williams (1988) will provide a continuing example for this section illustrating the complex and apparently contradictory results of learner-control research. The results of the two example studies appear to be quite different. Learner control produced the best student achievement of the three treatment groups studied by Carrier and Williams. In contrast, learner control produced the poorest achievement of the three treatment groups studied by Tennyson.

There are several between-study factors which might account for this apparent discrepancy, including student grade level, interval of treatment, topic of instruction, and the nature of the treatment with which learner control is being compared.

Student Class in School

Most learner-control studies report the subjects' class in school, rather than age. This is likely the result of using naturally occurring classes as subject pools. An inspection of the two sample studies indicates that the students studied by Tennyson (1981) were in twelfth grade; students in the Carrier and Williams study (1988) were in sixth grade.

In a list of tentative guidelines for determining the locus of instructional control, Hannafin (1984) posited that older students would perform more

effectively under learner control than younger students because "older and more able learners may have more effective and refined cognitive strategies to apply during instruction, and are likely to be better at estimating the accuracy of learning, the presence of confusion, and the need for additional instruction than younger and less able learners" (p. 8). A 1975 study (Fisher et al.) of fifth grade students using math courseware provided support for this idea; students in the learner-control treatment did not perform as well as students under program control. However, other research indicates that learner control can be effective for younger students. Elementary school children in grade 6 were the subjects of two studies (Holmes, Robson, & Steward, 1985; Carrier, Davidson, & Williams, 1985) in which the learner-control groups performed better than program control groups.

At the university level, a similar set of apparently contradictory findings exists. Ross and Rakow (1981), Atkinson (1972), and Steinberg, Baskin, and Hofer (1986), all using college students as subjects, reported better performance under learner control than program control. However, the college students in studies by Gay (1986) and Schloss, Wisniewski, and Cartwright (1988) did not perform as well as computer-control groups.

Number of Treatments and Treatment Duration

Holmes et al. (1985) noted, "It is likely that, when initially confronted with a learner-control facility, students may be distracted from learning the subject matter itself by having to cope with the additional task of making decisions about the instruction" (p. 106). Since it may take time for students to become familiar with the computer-based learning environment and with strategies for optimizing learner-control features, the positive effects of learner control may only appear after the novelty effect has worn off and after the students have adjusted to the protocol of the learning environment. Perhaps, then, positive results from learner-control treatments can only be detected from long-term studies.

The Tennyson (1981) study which showed superiority for computer control provided students with only one exposure to the instructional treatments. The Carrier and Williams (1988) study which showed superiority for learner control provided students with three exposures to the computer-based instruction. This might appear to support the hypothesis that success of learner control is somehow related to the number of treatment intervals or length of treatment. However, other studies do not support this tentative hypothesis.

Kinzie, Sullivan, and Berdel (1988) and Gray (1987) examined a single-exposure treatment and found achievement under learner control to be significantly better than computer control.

Lopez and Harper (1990) also examined a single-exposure treatment, but found no significant difference between learner control and computer-control treatments. Avner, Moore, and Smith (1980) studied treatments which lasted an entire semester and found mixed results: Learner-control treatments appeared to be less effective for tasks which required following instruction, but more effective for tasks which required decision making. Holmes et al. (1985) also studied treatments spanning an entire semester but found that providing learner control produced no significant difference in achievement.

Instructional Topic

Learner-control researchers have designed courseware for a number of different subject areas. Gray (1987) suggested that "[learner control] might be particularly relevant in instructional topics in which there is less emphasis on drill, less movement from simple to the complex, and a single correct answer - sociology, for example, as opposed to math, reading, or the natural sciences" (p. 54). Returning to the example studies, the topic of Tennyson's (1981)

courseware was physics. Specifically, his courseware introduced the concepts of force, power, velocity, and speed. Carrier and Williams (1988) used courseware which introduced four propaganda techniques used in advertising: bandwagon, uniqueness, testimonial, and transfer. Though the researchers did not specify the details of the courseware in their published reports, the nature of the topics suggests that Tennyson's courseware would be structured with more emphasis on progression from simple to complex, might require some mathematics ability, and would likely include more drill than would the courseware used by Carrier and Williams. This would lend support to Gray's hypothesis. However, analysis of additional studies in which the instructional topic was within the domain of mathematics does not show consistent support for the hypothesis.

Lee and Wong (1989) studied 11th-grade students using courseware designed to teach the conversion of mole to mole, mole to mass, mass to mole, and mass to mass units in gravimetric stoichiometry. Students in the computer-control treatment scored significantly better than students in the learner-control treatment. Goetzfried and Hannafin (1985) studied 7th-grade students using courseware to learn math rules about divisibility by two, three, and five. No significant

differences in achievement were found between the learner-control and computer-control treatment groups.

Fisher et al. (1975) studied fourth and fifth-grade students who were using an arithmetic drill and practice courseware. Students in the learner-control treatment posted lower scores than students in the computer control treatment. Ross, Morrison, and O'Dell (1990) studied undergraduate students who were using courseware designed to teach basic statistics. No significant effect on achievement was found between treatment groups.

Treatment Groups

Another between-study factor to consider is the nature of the treatment group with which the learner-control treatment is compared. Returning once again to the example studies, the three treatment groups in the Carrier and Williams (1988) study were not the same as the treatment groups in the Tennyson (1981) study. Tennyson compared the learner-control treatment with an adaptive-control treatment and a learner-adaptive-control treatment. The adaptive-control treatment utilized an instructional algorithm which prescribed the progression of the lesson based on the student's pretest and on-task performance. The learner-adaptive-control treatment used the same instructional algorithm as the adaptive-control treatment, but only advised the

student about lesson progression and the number of examples which were likely to be needed to reach mastery. Students under this treatment were free to follow or ignore the advice. The learner-adaptive-control treatment and the adaptive-control treatment posttest scores were statistically equivalent. However, the learner-control treatment posttest scores were significantly lower than for the other treatments.

Carrier compared the learner-control treatment with two treatments which were not adaptive -- treatments which might be called linear. In the first linear treatment, students were shown one concept definition, one expository instance, and worked one exercise for each concept. In the second linear treatment, students were shown the concept definition, a paraphrased definition, four expository instances, and worked four practice problems for each concept. Under learner control, students could select the number and type of treatment screens, as well as the number of practice problems. Results showed significant differences on posttest scores among all treatments, with the learner-control group having a higher mean score than either of the two linear treatments.

After considering the results of the two example studies, it would be tempting to hypothesize a continuum of effectiveness in which learner control is

more effective than linear control, but less effective than adaptive control. A study by Goetzfried and Hannafin (1985) did not support this hypothesis in its entirety. In the Goetzfried and Hannafin study the linear treatment produced better achievement results than the learner-control-with-advisement treatment, though a third treatment under adaptive-control produced better results than either learner or linear control.

Kinzie and Sullivan (1989) examined the effectiveness of learner-control and linear-control treatments. In this study, no significant difference in achievement was found between the two treatment groups, though the researchers found a strong preference for learner control.

Other studies have failed to show differences between learner control and other types of control. Schloss et al. (1988) studied 50 college students using courseware on special education. Subjects were assigned to one of four groups. For all groups, the courseware contained 90 information screens and 60 multiple choice test question screens. The choice-loop-no-feedback group used a version of the courseware which accepted student input in response to the multiple-choice questions and asked whether the student wanted to review information for an item answered

incorrectly. The forced-loop-no-feedback treatment was the same, except that incorrect answers automatically produced a review screen and another chance to answer correctly. The choice-loop-feedback treatment and forced-loop-feedback treatment were similar to their no-feedback counterparts, except that upon completion of each set of five questions, the cumulative number of items correct was displayed. In this study, no significant difference was found in the achievement scores for the forced-loop (computer-control) and choice-loop (learner-control) treatments, though significant effect was found for the feedback versus the no-feedback treatments.

Another study which failed to show significant achievement differences between learner control and other types of control was carried out by Holmes et al. (1985). The learner-control treatment allowed students to select one to six displays on each concept and allowed students to request explanatory solutions to incorrectly answered problems. The learner-control-with-pre-advice treatment was the same, except for the addition of a 25-minute pre-instructional session designed to familiarize students with the decision-making required to optimize learning under the learner-control environment. No significant difference in achievement scores was found between the learner

control and the learner-control-with-pre-advice treatments. A third treatment group in this study, which used an instructional algorithm, failed to produce achievement scores significantly different from the learner-control treatments. A fourth treatment group, in which the display and selections were made randomly by the computer, produced significantly lower achievement scores than for the other three treatments.

Another study which failed to show significant differences between control models was a recent study by Murphy and Davidson (1991) which found no significant differences in achievement or retention for students who were provided with learner control, adaptive control, or learner control with the addition of advisement.

Examples of apparent contradictions in the findings of learner-control research are numerous, as evidenced by the preceding discussion. However, it may be premature to conclude that learner control is effective in some situations, but not effective in other situations. The source of the contradictory findings may not be pedagogic factors, but instead may be discrepancies in the body of research. There are a number of research-related factors that could account for the inconsistency of findings from learner-control research, including reporting errors, lack of

standardization of the courseware, lack of agreement on what constitutes learner control, and sampling error. The first three research-related facets of the learner-control issue which may contribute to the apparent diversity of findings are discussed in the next three sections. Sampling error is discussed in the meta-analysis section of this chapter.

Reporting Errors

In 1986, the Journal of Educational Psychology published a study by Gay, reporting the results of research on the interaction between learner control and prior understanding in computer-assisted video instruction. In the study, 40 low-conceptual-understanding college students used biology courseware under either a learner-control or computer-control mode. Another group of 40 high-conceptual-understanding students used the same courseware, again under either a learner-control or computer-control mode. Gay reported a significant interaction between treatment and prior conceptual understanding: $F(1,79) = 10.53, p < .001, MSe = 37.81$ as measured by posttest scores. A significant interaction was also reported between treatment and prior conceptual understanding on task time: $F(1, 79) = 10.53, p < .01, MSe = 1280.2$. The results of Gay's research were later re-analyzed by Lee

and Wong (1989) who made the following correction: "The interaction Gay (1986) intended to show was incorrectly defined and stated. The analysis reported needed to be corrected to read $F(1, 76) = 10.56, p < .025, MSe = 3.58$ for posttest scores and $F(1, 76) < 1.0, MSe = 94.05$ for time on task" (p. 368). In this example, the initial research reported, incorrectly, the degrees of freedom.

Inaccuracies in reporting are likely to account for only a small number of the inconsistencies which appear in the learner-control research, but from the perspective of the practitioner or reviewer, these inaccuracies are not inconsequential in the attempt to piece together the learner-control puzzle.

Lack of Standards for Courseware

When discussing the results of their study on learner control and achievement in science computer-assisted instruction, Kinzie, Sullivan, and Berdel (1988) noted that "the differences in results may be due to variations in the instructional design of the CAI across studies, specifically in the types of instructional support and control offered" (p. 302). Characteristics of the courseware design which might have an effect on experimental results include the quality of the user interface and the dialogue format, specifically, response time, information density, use of color, style of navigation, use of graphics,

availability of help, and error handling. The courseware design may be limited by machine architecture and capacity. Note the equipment and user interface description from the methodology section of Atkinson's 1972 study on optimizing the learning of a second language vocabulary:

The control functions were performed by programs run on a modified PDP-1 computer operating under a time-sharing system. Eight teletypewriters were housed in a soundproof room and faced a projection screen mounted on the front wall.

...S typed 1 of 12 numbered keys during the inspection period to indicate to the computer which item he wanted to be tested on. At the end of the inspection period, S was required to type out the English translation for the designated German word and then strike the "slash" key, or if unable to provide the translation to simply hit the "slash" key. After the "slash" key had been activated the computer typed out the correct translation and spaced down two lines in preparation for the next trial. (p. 125)

Another 1972 study (Fry) creatively simulated what would now be called a multi-media-hypertext

environment. Again, the methodology is instructive, but note the "response time" problem:

...each subject was given a freshly shuffled deck of cards. Each card in the deck contained one significant question about computers to which the subject might wish to know the answer. For each card there was a corresponding videotape segment, that, in effect answered the question on that card. Therefore each subject could decide the sequence in which he wanted the questions answered. The experimenter, in another room, located the appropriate videotape segments and played them to the subject over a television monitor....

In addition, these subjects were given the opportunity to ask questions about each tape segment after it had been shown. If asked, the experimenter would appear on the monitor and answer each inquiry posed to him. (p. 460)

More recent studies, it might be assumed, would use a different implementation. However, many research reports lack adequate descriptions of the mechanics of the courseware. Because the learner-control treatments use a different version of the software from treatments under computer control, it is possible that some experiments tested the efficacy of a particular

software interface, rather than a learner-control factor. For example, if learners were allowed to control the sequence of instruction, it would be important to know how this was done. Was a series of menus used? Were the menu items self descriptive? Was the menu series easy to use? Was it easy to navigate between menus? A poorly designed learner-control version of the software could cause the learner-control treatment to appear inferior to the computer-control treatment. Better software design might eliminate this difference.

Lack of Agreement on what Constitutes Learner Control

It is an oversimplification to view the research on learner control as a comparison between learner control and computer control. A more accurate perspective might be to regard this research as a comparison of different *configurations* of learner control.

The term learner control has been applied inconsistently to treatment groups across studies. For example, Kinzie and Sullivan (1989) investigated the effects of learner control and program control on the achievement and continuing motivation of high school students. The program-control treatment allowed the students to proceed through the material at their own pace, and gave them feedback following interactions.

Table 1
Terminology Used to Refer to Treatment Groups from
Selected Studies in which Students Controlled Pace and
Were Given Feedback Following Interactions

RESEARCHER	TREATMENT GROUP TERMINOLOGY
Kinzie & Sullivan (1989)	Program control
Carrier et al. (1984)	No-options
Fisher et al. (1975)	Yoked control
Holmes et al. (1985)	Random
Gay (1986)	Program controlled treatment
Tennyson et al. (1985)	Learner controlled discrimination
Tennyson (1981)	Adaptive control

In a study by Tennyson, Park, and Christensen (1985), the group which performed under similar parameters was called the *learner-control* treatment. This ambiguity in the use of basic terminology was by no means an isolated instance. Table 1 shows the diversity of terminology used to refer to treatment groups in which students controlled pace and were given feedback following interactions.

Under these circumstances, a statement such as "learner control was shown to be inferior to program control" has little meaning. Lee and Wong (1989) made just this point, "It is evident that the features of the control strategies were not defined and studied in the same sense across the different studies... Before hastening to set up guidelines for using locus of control strategy or intelligent computer-aided learning design, it seems prudent to examine the nature of learner control conceptually and empirically" (p. 368).

Table 2
Degree of Learner Control for Imposed and Elected
Educational Goals and Treatments

I.	a.	Complete independence, self direction and self evaluation. Library resources available.
	b.	As above, but with periodic external checks, e.g. tests.
	c.	As above, but with resource consultants, peer discussion, and counseling available
II.	a.	Imposed tasks, but with learner control of sequence, scheduling, and pace of learning. Alternative instructional treatments available for choice by learner.
	b.	As above, but alternative instructional treatments are imposed by optimization rules.
	c.	As above, but without alternative instructional treatments.
III.	a.	Fixed tasks with learner control of pace. Remediation available.
	b.	As above, but without learner control of pace.
	c.	As above, without remediation.

Note. From Snow, 1980, p. 154.

Learner-control taxonomies. Snow (1980) proposed a taxonomy for learner control which is summarized in Table 2. Though often cited by learner-control researchers, Snow's concept of learner control as a continuum has limitations in terms of practical application. For example, Group III of Table 2, shows a progression from (a) learner control of pace with remediation to (b) computer control of pace with remediation to (c) computer control of pace without remediation. This progression does not provide for the case in which the learner controls pace but there is no remediation.

Carrier and Williams (1988) also recognized the basic conceptual problem underlying learner control research: "The lack of adequate models of learner-control strategies that describe the conditions under which the granting of control will be beneficial is a stumbling block for the interpretation of results from existing research and the formulation of hypotheses for future research" (p. 287). Carrier's model of factors affecting learner control contains a category, Type of Decisions Allowed, which includes pacing, sequence, review, and elaborative material. These concepts dovetail with the main concepts presented by Snow, but the notion of a strict continuum has been omitted.

Gay (1986) added another element to the list of learner-control variables; student choice of mode of presentation. Though Gay specifically mentioned delivery modes such as video, audio, graphics, and text, in a broad sense this might be extended to encompass any aspect of cognitive delivery including deductive, inductive, and socratic methodologies.

The control-factor framework. In order to compare "apples with apples and oranges with oranges" so to speak, it was necessary for this analysis to have a standard classification for learner-control treatment groups. Based on the work of Snow (1980), Carrier and Williams (1988), and Gay (1986) seven learner-control

factors were identified: pace, interaction, review, number of exercises, amount of material, sequence, and cognitive format.

Pace refers to the speed at which new concepts are introduced. When the learners control pace, there is generally some mechanism by which learners indicate they are ready to move on. Often this is as simple as pressing the return key to proceed to the next screen.

Interaction refers to the mode of dialogue between the learner and the computer program. Learner interaction exists when the learner makes an entry and receives a context-sensitive response.

Review refers to the provision to return to previously viewed screens. In computer-controlled environments, review is often provided after a student responds incorrectly to a segment or unit question. Generally, such review is mandatory. Under the learner-control strategy, review is available, but is provided only when requested by the student.

Number of exercises refers to the number of practice problems available. In many types of computer-based learning environments, the student is provided with practice exercises. When learners control the number of exercises, they may request additional exercises until they feel they have achieved mastery.

Amount of material refers to the availability of instructional explanations. When learners control the amount of material, they may request additional explanations about concepts. It is important to distinguish this supplementary material from review material. When learners request additional material, they are asking for new material. Learners requesting review material are requesting a re-display of previously viewed material.

Sequence refers to the order of material. Control of sequence may exist on several levels: for concepts, modules, or topics. When learners control the sequence, they can select the material they would like to work with next.

Cognitive format refers to the way in which material is presented. Typical options include graphics, text-based, or audio formats in addition to inductive or deductive presentations. When learners control the cognitive format, they may request the format or formats of presentation for the instructional material.

Using the control factor framework, courseware can be classified as providing the learner with control over one or more of the seven control factors. In this way, treatment groups, for example, in which the learner controls the pace of the instruction and

nothing else, can be classified as such and compared with other treatment groups within the same or different classifications, as needed.

Meta-Analysis

Purpose and Use of Meta-Analysis

The Purpose of Meta-Analysis

Meta-analysis was developed as an alternative to traditional non-quantitative, heuristic methods for reviewing and integrating research. It is the perspective of meta-analysis that the methods employed in traditional narrative review lack rigor. Glass et al. (1981) explained in the seminal text, Meta-Analysis in Social Research:

Styles of research integration have been shaped by the size of the research literature. In the 1940s and 1950s, a contributor to the Review of Educational Research or Psychological Bulletin might have found one or two dozen studies on a topic. A narrative integration of so few studies was probably satisfactory. By the late 1960s, the research literature had swollen to gigantic proportions. Although scholars continued to integrate studies narratively, it was becoming clear that chronologically arranged verbal descriptions of research failed to portray the

accumulated knowledge. Reviewers began to make crude classifications and measurements of the conditions and results of studies. Typically, studies were classified in contingency tables by type and by whether outcomes reached statistical significance. Integrating the research literature of the 1980s demands more sophisticated techniques of measurement and statistical analysis. The findings of multiple studies should be regarded as a complex data set, no more comprehensible without statistical analysis than would be hundreds of data points in one study. Contemporary research should be more technical and statistical than it is narrative. (p. 12)

The pitfalls of narrative integration are numerous. Hunter et al. (1982) provided an illustration by presenting the results of 30 studies on the correlation between organizational commitment and job satisfaction (Table 3), then stepped through the contingency table methodology of research review. The typical reviewer looking at these data would note that 19 of the 30 studies found a significant correlation, but 11 of the 30 studies found no correlation. Delving for an explanation of these inconsistent findings, the reviewer might look at the population being studied and note that in studies which examined the gender

variable, male populations showed a correlation in 8 of 15 studies, but showed no correlation in 7 of 15 studies. Females showed a correlation in 11 of the 15 studies.

The reviewer might look at the difference between studies with blue-collar subjects and those with white-collar subjects. Correlation between occupational commitment and job satisfaction would be found in 79% of the blue-collar studies, but in only 50% of the white-collar studies. So the reviewer might continue, examining other interaction variables such as race, age, size of company, and geographical location, finally concluding something like the following:

Organizational commitment and job satisfaction are correlated in some organizational settings but not in others. In work groups in which all workers are over thirty, the correlation between commitment and satisfaction was never significant. In young or mixed-age work populations, commitment and satisfaction are always correlated in large organizations. For younger or mixed-age work populations in small organizations, correlation was found in nine of thirteen studies with no organizational feature capable of perfectly accounting for those cases in

Table 3
Correlations Between Organizational Commitment and
Job Satisfaction

STUDY	N	R	SEX	ORG. SIZE	WHITE/ BLUE COLLAR	RACE	UNDER/ OVER	NORTH SOUTH
1	20	.46*	F	S	WC	B	U	N
2	72	.32**	M	L	BC	Mix	Mix	N
3	29	.10	M	L	WC	W	O	N
4	30	.45**	M	L	WC	W	Mix	N
5	71	.18	F	L	BC	W	O	N
6	62	.45**	F	S	BC	W	U	N
7	25	.56**	M	S	BC	Mix	U	S
8	46	.41**	F	L	WC	W	Mix	S
9	22	.55**	F	S	WC	B	U	N
10	69	.44**	F	S	BC	W	U	N
11	67	.34**	M	L	BC	W	Mix	N
12	58	.33**	M	S	BC	W	U	N
13	23	.14	M	S	WC	B	O	S
14	20	.36	M	S	WC	W	Mix	N
15	28	.54**	F	L	WC	W	Mix	S
16	30	.22	M	S	BC	W	Mix	S
17	69	.31**	F	L	BC	W	Mix	N
18	59	.43**	F	L	BC	W	Mix	N
19	19	.52*	M	S	BC	W	Mix	S
20	44	-.10	M	S	WC	W	O	N
21	60	.44**	F	L	BC	Mix	Mix	N
22	23	.50**	F	S	WC	W	Mix	S
23	19	-.02	M	S	WC	B	O	S
24	55	.32**	M	L	WC	W	Mix	UKN
25	19	.19	F	S	WC	B	O	N
26	26	.53**	F	S	BC	B	U	S
27	58	.30*	M	L	WC	W	Mix	S
28	25	.26	M	S	WC	W	U	S
29	28	.09	F	S	BC	W	O	N
30	26	.31	F	S	WC	Mix	U	S

* $p < .05$ ** $p < .01$

Note. From Hunter et al., 1982, p. 14.

which correlation was not found. (Hunter & Schmidt, 1990, pp. 17-18)

Though the reviewer's conclusions would appear reasonable, Hunter and Schmidt (1990) pointed out that every conclusion in the review was false. In fact, the

so-called studies and their results were constructed by a Monte Carlo run in which the population correlation was always assumed to be .33 and hence the apparent variations in the results of the studies were entirely the result of sampling error. Hunter and Schmidt continued with this example to demonstrate how meta-analysis of this set of studies would reveal that the relation between organizational commitment and job satisfaction was constant across gender, race, job level, age, geographical location, and size of company. The statistical estimate of correlation using meta-analysis was .331 -- very close to the actual value of .33 -- and as Hunter and Schmidt pointed out, all reviewers applying this quantitative methodology should arrive at the same conclusion.

The Hunter-Schmidt example illustrates how sampling error can be mistaken for contradictory, inconclusive, ambiguous, or inconsistent findings within a body of research. This suggests the possibility that in the learner-control research, often characterized as contradictory, etc., sampling error, rather than true differences, might account for the apparent contradictions. Further, the quantitative methodology offered by meta-analysis may reveal the sampling error problem, if one exists.

Meta-analytic methodology has been proposed as a technique for research review and integration which is superior to the traditional narrative and contingency table techniques. The noted Nobel Laureate, Herbert A. Simon (Simon & Kaplan, 1989), neatly summed up, "A meta-analytic review allows us to answer questions both more objectively and more precisely than a standard review of comparable scope allows" (p. 34).

Increasing Use of Meta-Analysis

Much of the pioneering work on meta-analysis can be attributed to Glass, who in 1976 published the article, "Primary, Secondary and Meta-Analysis of Research," in the Educational Researcher and in 1981 published the book, Meta-Analysis in Social Research (Glass et al., 1981), under the auspices of the American Psychological Association. In the five-year time span bracketed by the publication of Glass' 1976 article on meta-analysis and his book, significant movement was made to apply the technique. A tabulation of references in Meta-Analysis in Social Research with "meta-analysis" contained in the title revealed 12 published articles discussing the pros and cons of meta-analysis, 4 published meta-analyses, 9 miscellaneous presentations or working papers describing completed meta-analyses, and 8 dissertations (University of Michigan, University of Colorado,

University of Denver, Illinois Institute of Technology).

Table 4
Number of Articles in Psychological Abstracts from 1967-1984 Key-Worded as "Meta-Analysis"

	1977	1978	1979	1980	1981	1982	1983	1984
Number	2	4	6	9	18	32	55	63

Note. From Guzzo et al., 1986.

The use of meta-analysis has increased in recent years. Guzzo, Jackson, and Katzell (1986) tabulated the number of journal articles and dissertations in Psychological Abstracts that were key-worded as "meta-analysis." Their results are summarized in Table 4.

The number of meta-analyses that have been conducted to date is open to question. Hunter and Schmidt (1990) estimated that by 1987 there were over 500 meta-analyses on the single topic of the validity of personnel selection procedures. Psychological abstracts lists a total of 207 entries key-worded as meta-analysis through 1987. Abrami et al. (1988) mentioned the "scores of quantitative reviews" which appeared in the ten years following Glass' initial work, but did not indicate the exact number. Rothstein and McDaniel (1989) referred to the proliferation of meta-analytic techniques and applications, but did not

provide supporting data. Though the number of meta-analyses performed to date is uncertain, there appears to be a growing acceptance of the usefulness of meta-analysis as an alternative to narrative review techniques. As Bangert-Drowns (1986) stated "Meta-analysis is not a fad. It is rooted in the fundamental values of the scientific enterprise: replicability, quantification, causal and correlational analysis... The potential benefits of meta-analysis method seem enormous" (p. 398).

The Basic Methodology of Meta-Analysis

The basic concept of meta-analysis is quite straight-forward. The outcome measures from a collection of primary research studies are converted into standardized scores called effect sizes. These effect sizes can then be averaged or otherwise manipulated to assess outcomes across studies. Detailed discussion of the statistical procedures used by meta-analysis is presented later in this chapter and in Chapter 3.

Though Glass is credited with much of the initial work in developing meta-analysis, several additional approaches have evolved. Bangert-Drowns (1986) has identified five meta-analytic methods: (a) the classical Glassian approach: (b) study effect meta-analysis, (c) combined probability, (d) approximate

data pooling, and (e) psychometric meta-analysis. Of the five methodologies, the classical Glassian approach and psychometric meta-analysis appear to be the most completely documented. In the next sections, these two methodologies are discussed and compared in order to provide a foundation for understanding the methodology used in this dissertation as detailed in Chapter 3.

Glassian Meta-Analysis

The purpose of Glassian meta-analysis is to review and summarize what the literature says about a particular research question. The unit of analysis is the study finding. Effect sizes are not weighted by the number of subjects in the study and a study may contribute more than one effect size to the analysis. The conclusions from such a meta-analysis are descriptive in nature; average effect sizes and comparisons of effect sizes in pre-established categories.

The Glassian methodology for meta-analysis may be summarized as follows:

1. Locate the population of primary studies on a specific research question.
2. Code each study according to a list of methodological and substantive characteristics. Methodological factors may include study date, publication form, assignment to groups, reactivity of

outcome measure, and internal validity. Substantive factors are specific to the problems studied and in learner-control research may include subject age, gender, aptitude, degree of learner control, type of computer, subject matter, and duration of treatment.

3. Calculate effect size for each study. The effect size is the difference between the means in standard score form. If the data are available, the mean from treatment C (control group) would be subtracted from the mean of treatment E (experimental group). The difference of the two means would then be divided by the standard deviation of treatment C¹:

¹ Glass et al. (1981, pp. 106 - 107) discussed their preference for the use of s_c , rather than s_E or $(s_E + s_c) / 2$ by means of an example in which methods A, B, and Control are compared in a single experiment, yielding the following results:

	Method A	Method B	Control
Means	50	50	48
Standard Deviations	10	1	4

Note that the treatment has apparently had an effect on the standard deviations of the experimental groups as well as on means of the experimental groups. If the effect sizes are calculated using the standard deviation of the method, the effect size of A equals .20 and the effect size of B equals 2.00. This, suggested Glass et al., is a misleading difference in view of the equality of means on the dependent variable. Misleading differences such as in this example may appear whether the treatment increases the standard deviation as in Method A or decreases the standard deviation as in method B. The problem lies in the magnitude of the difference between the standard deviations of the treatment methods. Using the control treatment standard deviation would yield effect sizes of .50 for both methods.

$$\Delta = (\bar{X}_E - \bar{X}_C) / s_C$$

*where Δ is the effect size,
 \bar{X}_E is the mean of the experimental group,
 \bar{X}_C is the mean of the control group,
 s_C is the standard deviation of the control group.*

Glass et al. (1981) provided alternative procedures for calculating effect size if the mean and standard deviation are not available, but other statistics such as t or F are reported.

4. Analyze the effect sizes for possible interaction from the coded experimental variables. The quantified effect sizes can be subjected to most of the standard methods of tabulating and describing data: frequency distributions, averages, measures of variability, etc.

Psychometric Meta-Analysis

As Glass went to press in 1980, the American Psychological Association had initiated a project intended to encourage innovations in methodology for organizational research. Eighteen scholars were recruited from a variety of disciplines and organized into six task groups. One group, with members including John E. Hunter, professor of Psychology and Mathematics at Michigan State University; Frank L. Schmidt, Research Psychologist at George Washington University; and Gregg B. Jackson, free-lance social science

research consultant in Washington, D.C., produced a monograph titled "Meta-Analysis" which was published by the American Psychological Association in 1982. In 1990, Hunter and Schmidt expanded their earlier work and published Methods of Meta-Analysis: Correcting Error and Bias in Research Findings.

Hunter and Schmidt proposed a methodology, termed psychometric meta-analysis, which extended Glassian-style meta-analysis to deal with variations in study effect sizes due to sampling error and other artifacts. The purpose of psychometric meta-analysis is to estimate population treatment effects. Consequently, this approach is inferential, whereas the Glassian approach is more descriptive in purpose. The unit of analysis in psychometric meta-analysis is the subject, rather than the study. The effect size, d , is calculated using the formula:

$$d = (\bar{X}_E - \bar{X}_C) / s_w$$

*where \bar{X}_E is the mean of the experimental group,
 \bar{X}_C is the mean of the control group,
and s_w is the pooled within-group standard deviation.*

Note that the pooled within-group standard deviation used in the denominator differs from the standard deviation of treatment group C (control group) used in the denominator by Glass. Hunter and Schmidt prefer this value in the denominator because the pooled

within-group standard deviation has less sampling error than the control-group standard deviation (Hunter & Schmidt, 1990, pp. 276 - 278).

Hunter and Schmidt (1990) suggested calculating the mean effect size across studies, before coding the studies for selected properties (moderator variables) that vary across studies. If the mean effect size is determined to be zero, it can be concluded that the treatments being compared did not produce different values on the outcome measure. If the mean effect size is determined to be non-zero, then it can be concluded that the treatments being compared produced a difference in the standardized value of the outcome variable. The magnitude of this difference is indicated by the mean effect size.

Hunter and Schmidt (1990) summarize:

After estimating mean true effect size, the hypothesis that observed S^2_{ES} is due to statistical artifacts is tested using the methods developed by Smith and Hunter. This is the hypothesis that the variance of actual (true) effect sizes is zero, i.e., $S^2_{ESA} > 0$. If this hypothesis is rejected, the reviewer concludes that the true ES is constant across the many factors varying in the studies reviewed. Estimated ESA is then the final and only product of the review.

If the hypothesis that $S^2_{ESA} > 0$ cannot be rejected, then selected properties that vary across studies are coded and correlated with study ESs as suggested by Glass. (pp. 485-486)

Issues in Meta-Analysis

Critique and commentary on meta-analysis includes two major themes. The first theme is a critique of the general philosophy of meta-analysis as an appropriate alternative to narrative review techniques traditionally used for research integration. The second theme is commentary on the specific procedures and statistical techniques used for meta-analysis. The purpose of this section is to highlight the major controversies within each of these themes.

Critique of Meta-Analytic Philosophy

Meta-analysis has its critics. Eysenck (1978) called it "an exercise in mega-silliness." Gallo (1978) called it "a mixed meta-phor." At issue is the legitimacy of meta-analysis as an appropriate tool for research integration; as an addition to, or substitute for heuristically-based narrative techniques. Chow (1986) summed up the reoccurring "apples and oranges" argument against the use of meta-analysis:

Often the diverse experiments bearing on a theory are "converging operations," "conceptual replications," or "constructual replications"

devised to test various aspects of the theory. The important points about these converging operations are that (a) they are not literal replications of the same experiment, (b) the experimental conditions and procedures used in the converging operations (i.e., the individual experiments) are often very different, (c) the experimental task may be very different from the original phenomenon for which the theory is proposed, and (d) it is inevitable that some auxiliary assumptions have to be made implicitly.... These differences cannot be ignored by appealing to a super-category. If data from these experiments are aggregated in the way suggested by the meta-analysts, apples are being mixed with oranges with no defensible justification. (p. 266)

Advocates of meta-analysis have responded to the "apples and oranges" criticism from a variety of perspectives. Glass et al. (1981, p. 218) argued that a meta-analysis of only "apples" would be pointless. In other words, the concept of comparing studies that are the same in all respects is unproductive because they would all have the same results within statistical error. In an additional argument, Glass et al. (1981, p. 220) drew a parallel between the different studies

used as the basis for meta-analysis and the different persons used as the basis for primary research:

The same critics who object to pooling the findings of studies 1, 2, ..., 10 see nothing at all objectionable in pooling the results from persons 1, 2, ..., 100 in their own research. (p. 276)

Bangert-Drowns (1986) suggested that the significance of the "apples and oranges" criticism must be considered within the context of the research question. A broad research question such as "Does CAI affect student achievement?" may not require a database of homogeneous studies, whereas a more specific research question such as "What is the relative effectiveness of different types of CAI?" would suggest the need for finer-grained analysis and the selection of studies with a greater degree of homogeneity.

In a previous section, the accuracy and replicability problems with narrative research were discussed along with the theoretical advantage of more quantitative methods. However, it is not yet clear in practice whether meta-analysis offers a more effective methodology. Abrami, et al. (1988) compared six quantitative reviews of the research on the validity of student ratings of instructional effectiveness. The reviews did not reach similar conclusions, though each

used meta-analysis techniques. Differences in the conclusions of the six reviews were attributed to differences in procedures for locating studies, inclusion criteria, selecting and coding of moderator variables, effect size calculations, and data analysis. The Abrami team suggested the need for meta-analysts to follow rigorous guidelines. Some guidelines (Rothstein & McDaniel, 1989) have appeared in the literature. However, the nuances of meta-analysis are complex and still evolving. Early expectations for a simplistic solution to the complex problem of integrating research appear to have given way to a more pragmatic exploration of appropriate applications for meta-analysis and the development of appropriate statistical procedures.

Statistical Procedures for Meta-Analysis: Concerns

As described in the earlier section, "The Basic Concept of Meta-Analysis," at least five different methods for meta-analysis have been identified. These include the classical Glassian approach, study effect meta-analysis, the combined probability method, approximate data pooling, and psychometric meta-analysis. Each method is associated with a specific research question. The Glassian method of meta-analysis is designed to examine what the literature indicates about the way research is organized and

interpreted in a given area. Study effect meta-analysis is designed to review what the literature says about a treatment's effectiveness. Neither Glassian, nor study effect meta-analysis is fundamentally designed to estimate population treatment effects. These meta-analytic methods are not, in this sense, inferential, but rather descriptive in focus.

In contrast, meta-analyses based on the combined-probability, approximate-data-pooling, or psychometric method are designed to estimate population treatment effects. These methods can be regarded as inferential.

The underlying philosophies of the five meta-analytic methods generate specific technical decisions about such controversial issues as the selection of statistical procedures, whether to include methodologically poor studies, and the appropriateness of representing individual studies by multiple effect sizes. Analysts and critics alike should attempt to understand the connection between the underlying research philosophies and the different meta-analytic methods they produce.

The two commonly used summary statistics for effect size have already been discussed. Glass et al. (1981) prefer to use the formula:

$$\Delta = (\bar{X}_E - \bar{X}_C) / s_C$$

*where Δ is the effect size,
 \bar{X}_E is the mean of the experimental group,
 \bar{X}_C is the mean of the control group,
 s_C is the standard deviation of the control group.*

Hunter et al. (1981) prefer to use the formula:

$$d = (\bar{X}_E - \bar{X}_C) / s_w$$

*where d is the effect size,
 \bar{X}_E is the mean of the experimental group,
 \bar{X}_C is the mean of the control group,
and s_w is the pooled within-group standard deviation.*

As noted earlier, the use of s_C in the denominator of the Glass formula apparently introduces more sampling error than the s_w used in the Hunter formula (Hunter & Schmidt, 1990, p. 277). Glass et al. appear to be less concerned with sampling error. This is a perspective which would be inappropriate for inferential research, but may not be so for descriptive research.

Selection of the studies to be included in a meta-analysis involves consideration of the methodological integrity of the sample. Critics (Eysenck, 1978; Gallo, 1978; Mintz, 1983) have attacked meta-analysis because methodologically weak studies included in the data pool may produce misleading results. Advocates of meta-analysis have responded in a variety of ways, reflecting their orientation to one of the five meta-analytic methods. Though most meta-analytic methods

generally advocate the inclusion of methodologically weak studies, the reason for the inclusion of these studies and the treatment of these studies differs. Glassian meta-analysis includes all studies, but studies are coded to indicate methodological rigor. Since the intent of Glassian meta-analysis is to examine the characteristics of a body of research, the proportion of methodologically weak studies is a valid product of such an analysis.

Because the purpose of study effect meta-analysis is to examine what the literature says about the effectiveness of a treatment, researchers who apply the study effect meta-analytic method tend to exclude studies that do not meet a defined set of methodological standards (Kulik et al., 1979; Kulik et al., 1980).

When using one of the inferential methods of meta-analysis, the inclusion of methodologically weak studies is left to the discretion of the researcher. However, if the researcher decides to include weak studies, separate analyses should be run to determine whether omitting the weak studies produces different results. Studies can be coded for methodological rigor, then subjected to an analysis to determine if there is significant effect size difference between studies coded as methodologically strong and those

coded as methodologically weak. If there is no difference between the groups, then all studies may be used for further analysis. This technique has been used for several meta-analyses in the area of educational research (Cook et al., 1986; Hembree, 1988).

The appropriateness of including more than one effect size from a single study is sometimes referred to as the "nonindependence problem" or the problem with "inflated Ns" (Bangert-Drowns, 1986; Chow, 1986; Glass et al., 1981) and can be usefully considered both in terms of the dependent variables and the independent variables of the meta-analysis.

In educational research, dependent variables may include achievement test scores, retention test scores, time on task, and student attitude ratings. Some studies measure results on more than one dependent variable and consequently report more than one result. In such cases, the meta-analyst is faced with the task of integrating a number of studies using diverse outcome measures. Glass and Smith (Smith & Glass, 1977; Smith, 1980), if not advocating the combined use of diverse outcome measures in meta-analysis, appear to have practiced it. However, the practice of combining or averaging effect sizes from diverse dependent variables is difficult to justify, even in the

descriptive realm of classical Glassian meta-analysis. Bangert-Drowns (1986) cautioned against such practice by explaining:

In studies of computer-based instruction, for example, achievement test scores, ratings of attitudes toward computers, and ratings of attitudes toward course content cannot be said to measure the same construct. How would one interpret their combination? "Computer-based instruction produces increases of 0.30 standard deviation in" what? (p. 392)

The nonindependence problem has even greater impact on meta-analyses used as the basis for inferential purposes. The assumption of independent samples, the cornerstone of statistical inference, is violated if the value of one sample mean is influenced by or related to the value obtained for the other sample mean (Kachigan, 1986, p. 114). This is arguably the case when one treatment in a single study produces more than one result and both are entered into a meta-analysis as data points.

For example, a study which reported effects of a treatment on attitude and achievement would enter two effect sizes into the analysis, giving this study twice as much weight in the analysis as a study which only reported an effect size for achievement. In such a

case, the meta-analysis can be biased by the results of only a few studies which report results for many dependent variables.

In order to avoid the nonindependence problem, Hunter and Schmidt (1990) have advocated running separate analyses for each dependent variable. For cases in which it would be desirable to test specific hypotheses about the effects of treatment on different dependent variables, Rosenthal and Rubin (1986) have provided procedures for combining research results for multiple effect sizes based on multiple dependent variables obtained from a single study.

The nonindependence problem not only occurs as described above when the same subjects are measured on a series of different outcome measures (dependent variables), but may also occur when different subjects in a study provide outcomes. This is typically the case in between-groups factorial design because the experimental setting and procedures are more likely to be similar within the environment of a given study than between two independent studies. As an example, consider two experiments, each with 60 subjects. In one experiment, subjects are divided into two groups. Each group is exposed to a different treatment, then given a standardized achievement test. In the other experiment, subjects are divided into four groups.

Again, each group is exposed to a different treatment, then given a standardized achievement test. The first experiment will provide one effect size, based on the difference between the mean achievement test scores for the two treatment groups. The second experiment may provide up to six effect sizes, again based on achievement test scores.

As with the nonindependence problem for dependent variables, it would appear unwise to enter into the meta-analysis six effect sizes from one study and only one effect size from the other. Abrami et al. (1988) have advocated the use of weighted statistical procedures to ameliorate the problem. Whether the weighting procedure is performed on a per-study basis or a per-subject basis may be decided once again by referring back to the research questions underlying the various methods of meta-analysis.

Both Glassian meta-analysis and study effect meta-analysis have the study as the basic unit of analysis (Bangert-Drowns, 1986). This concept is derived from the underlying philosophy that the purpose of meta-analysis is to describe a pool of research studies. An appropriate weighting strategy would be to weight each effect size such that the weights of the effect sizes for each study sum to one.

The *subject* is the basic unit of analysis for the inferential methods of meta-analysis (Bangert-Drowns, 1986). This concept is derived from the philosophy that the purpose of meta-analysis is to estimate population treatment effects from a pool of data composed of all the subjects in all the studies included in the analysis. An appropriate weighting strategy, advocated by Hunter and Schmidt (1990), would be to use the treatment group N .

To recapitulate the criticism and commentary on meta-analysis, the legitimacy of meta-analysis as a tool for research integration has been questioned and criticisms have been leveled against certain statistical and methodological procedures used by meta-analysts. The questions are complex and should be considered within the context of the underlying philosophy or purpose for a particular analysis. In meta-analysis, as in other research endeavors, the research question should be carefully considered before appropriate statistical and procedural techniques are selected.

Summary

Learner-control studies were presented in a historical context. Early researchers concerned with issues of instructional control had strong a priori assumptions that learner control of computer-based

learning environments would benefit students by reducing anxiety, increasing task engagement, increasing learning speed, improving attitudes, and improving performance. However, the results of empirical studies did not support the assumption that learner control would improve performance.

It was suggested that these early studies did not take into account the appropriate constellation of variables which might impact the effectiveness of learner control. Consequently, as the research continued, it encompassed an expanding number of variables.

Commentators, reviewers, and researchers have continually referred to the "montage of inconsistencies" in learner-control research. Specific examples of contradictory empirical results were given for between-study variables such as student grade level, treatment interval, instructional topic, and the nature of the treatment with which learner control was being compared. Additional factors which may have contributed to the inconsistency of findings from learner-control research were also discussed. These included reporting errors, lack of standardization of the courseware, lack of agreement on what constitutes learner control, and sampling error.

The lack of agreement on the fundamental terminology of what constitutes learner control was examined as an issue of particular concern because of its role in confounding the integration of learner-control research. It was suggested that it would be an oversimplification to view the research as a comparison between learner control and computer control. A more accurate perspective would be to regard the research as a comparison of different levels, types, or configurations of learner control. Several taxonomies for learner control were delineated. When examined from an operational standpoint, i.e., with the purpose of classifying learner-control studies, some of these taxonomies were found to be ineffective. A control-factor framework was proposed as an effective operational classification for courseware variables in learner-control studies. The control factors were pace, interaction, review, amount of material, number of exercises, sequence, and cognitive format.

Sampling error was discussed as a possible source for the apparently contradictory findings of learner-control research. An example drawn from Hunter and Schmidt (1990) was used to illustrate. Meta-analysis was suggested as an appropriate tool for examining the role sampling error might play in the interpretation of learner-control findings.

It was pointed out that meta-analysis has been gaining stature as a tool for quantitative integration of studies in psychology, social science, and education. The basic concept of meta-analysis was explained: The outcome measures from a collection of primary research studies are converted into standardized scores called effect sizes. These effect sizes can then be averaged or otherwise manipulated to assess outcomes across studies.

Different approaches to meta-analysis were discussed. Differences in methodology may involve the selection of descriptive or inferential techniques, choice of statistic for calculating effect size, and the criteria for accepting studies. As with primary research, the methodology should be compatible with the research question.

Criticisms of meta-analysis and concerns pertaining to the use of meta-analytic methodologies were presented. It was pointed out that techniques for meta-analysis are still evolving and that procedures should be carefully considered within the context of the research question.

CHAPTER 3
METHODOLOGY
Introduction

The purpose of this dissertation was to use meta-analysis to integrate the findings of independent research of learner control in computer-based environments in order to provide practitioners with actionable information. Three research questions were defined:

1. What are the characteristics of the body of learner-control research which has examined the effect on student achievement of learner control?
2. Is there a difference in the achievement of students provided with learner control and students who are provided with other control models?
3. Do specific moderator variables interact with learner control to produce different achievement effects?

Chapter 2 discussed meta-analysis as a tool for integrating research and described several different meta-analysis methodologies, including the classical Glassian approach and the Hunter-Schmidt psychometric approach. This meta-analysis of learner control utilized elements of the classical Glassian approach and of the psychometric approach in order to appropriately deal with both the descriptive nature of

the first research question and the inferential nature of the second and third research questions.

Here in Chapter 3, the specific methodology used for the analysis is justified and described. The first section of this chapter presents a synopsis of meta-analysis in educational research and recapitulates the rationale for applying meta-analytic methodology to the learner-control research questions. The second section of the chapter details the procedures used to collect and prepare the data for analysis. The third section of this chapter describes the statistical procedures which were used to integrate the collected learner-control studies.

**Meta-Analysis in Educational Research and
Justification of Meta-Analysis as a Tool
for Integrating Learner-Control Research**

The methodology for meta-analysis has been described by a number of sources (Glass et al., 1981; Hunter et al., 1982; Hunter & Schmidt, 1990; Wolf, 1986) and has been applied to numerous studies in educational research, including Kulik, Kulik, and Cohen (1979); Kulik, Kulik, and Cohen (1980); Kulik, Bangert, and Williams (1983); Cook, Scruggs, Mastropieri, and Castro (1985); Schmidt, Weinstein, Niemic, and Walberg (1985); and Hembree (1988).

Kulik et al. (1979) carried out a meta-analysis of 75 studies of Keller's personalized system of instruction and found that this system generally produced superior achievement, less variation in achievement, and higher student preference ratings. The overall effect size, .49, was termed 'moderate' by the researchers.

The next meta-analysis performed by the Kulik team (Kulik et al., 1980) integrated the findings from 59 independent evaluations of computer-based college teaching. The researchers found small but significant effects on achievement and on attitudes in addition to substantial reductions in instructional time. The average effect was .25 and the researchers summarized, "the effect of CBI in a typical class was to raise student achievement by about one-quarter of a standard deviation unit" (p.534).

In 1983, Kulik, Bangert, and Williams integrated the findings of 51 studies of computer-based teaching in grades 6 through 12. The meta-analysis indicated that computer-based instruction raised student scores on immediate posttests, though effects on retention tests were smaller. The average effect size was .32 which, according to the researchers, implied that "in a typical class, performance of CBI students was raised by .32 standard deviations" (p. 23). As in the

analysis at the college level, instructional time was substantially reduced and student attitudes were positive toward the instructional method.

Cook et al. (1985) conducted a meta-analysis of 19 studies on the effectiveness of handicapped students as tutors of other students. The effect size for tutors was .52 for all studies. The effect size for tutees was .58. The results were interpreted to indicate that handicapped students generally provided effective tutoring, but that tutees gained more than tutors. Both tutors and tutees showed small gains on self-concept and sociometric rating, though gains on attitude measures were larger.

The meta-analysis conducted by Schmidt et al. (1985) examined 22 studies of computer-assisted instruction for special education students and reported moderately positive effects, especially for those students at a lower level of learning. The effect size was .665.

Hembree (1988) conducted a meta-analysis of 562 studies on test anxiety. This study encompassed a number of research questions and reported an array of effect sizes related to each. Hembree's findings indicated that test anxiety caused poor performance and was related to ability, gender, and school grade level. In addition, it was found that test anxiety could be

reduced by specific behavioral and cognitive-behavioral treatments.

As explained in Chapter 2, meta-analysis is designed to quantitatively summarize or integrate a body of research. It may be viewed as a study of studies. In theory, meta-analysis is superior to heuristic narrative techniques because rigorous statistical procedures are applied to the data, reducing the possibility of reviewer bias. Though meta-analysis is an evolving methodology and not without critics, careful definition of the research question and the selection of appropriate meta-analysis methodology can produce valid and meaningful results.

The body of research on learner control of computer-based learning environments appears to be in need of integration. As indicated in Chapter 2, learner-control research results seem to be ambiguous, inconsistent, inconclusive, and contradictory. Consequently, it is difficult to apply the findings to the design and selection of courseware. Narrative reviews of learner control must deal with a complex interaction of variables and hence may offer conclusions based on an artificially simplified sample of studies. It is possible that the apparent contradictions in the learner-control research are a result of sampling error or other study artifacts. The

quantitative methodology of meta-analysis was selected for this dissertation in order to provide a comprehensive and unbiased summary of learner control and its effect on student achievement.

Collecting and Preparing the Data

The data for this meta-analysis were obtained from primary studies on learner control of computer-based learning environments. As indicated by the research questions and hypotheses in Chapter 1, the dependent variable was achievement test scores. Hence, reported achievement scores from each treatment group in the population of primary studies were collected, converted into a standard metric, and pooled for the meta-analysis.

The independent variable for the meta-analysis was the type of learner control. As indicated in earlier chapters, seven learner-control factors were examined: pace, interaction, review, number of exercises, amount of material, sequence, and cognitive format.

Moderator variables, defined as variables which may interact with findings, included class-in-school, topic of instruction, type of computer equipment, instructional media, form of publication, experimental design, study date, researcher, and length of treatment.

The data for the dependent variable, independent variables, and moderator variables were entered into a dBase III+ database, LCGRID, designed by the author of this dissertation to maintain substantive and methodological information pertaining to studies and treatment groups. Documentation for this file is included in Appendix B.

Data collection and coding were performed by the author. As a reliability check, five studies from a pilot meta-analysis of learner control performed by this author in 1990 were re-coded after a six month time interval. A reliability of .92 was achieved between the first coding and the second across all coding items.

The remainder of this section details the procedures followed for locating studies, selecting studies, and collecting data pertaining to the dependent, independent, and moderator variables.

Locating Studies

The studies used for this analysis were located by using the Educational Resources Information Center (ERIC) database on CD ROM, Comprehensive Dissertation Abstracts, the Psychological Abstracts database (PsychInfo), and the bibliographies of articles located from the databases.

Specifications were developed for the database searches. ERIC was searched using the key "learner control and computer." The Psychological Abstracts search used "learner(w)control and computer assisted instruction/de and empirical methods/de, /eng." The titles and abstracts of the articles located were examined to determine initial suitability for the analysis. Copies of suitable articles were obtained and read in full. Each article was subjected to a final screening using the admissibility criteria described in the next section.

Admissibility Criteria

To be included in the final sample, studies were required to satisfy the following criteria:

1. **Subjects in the study must interact with a computer-based instructional unit.** The instructional unit could be presented in a multi-media format, but the computer must be an integral part of the environment. The instructional medium was coded as a potential moderator variable in order to examine its impact on study effects.
2. **In one of the treatment groups, subjects must have more control over the learning environment than subjects in another treatment group.**
3. **Both published studies and dissertations may be included in the analysis.** Though there has been some

controversy in the literature regarding the use of unpublished studies in meta-analysis, Glass et al. (1981) suggested that excluding unpublished studies might introduce bias into the sample. As documentation, they cited earlier research (Smith & Glass, 1977) which found that studies in published books had an average effect size of .8, those published in journals had an average effect size of .7, dissertations averaged .6, and unpublished studies averaged .5. Hunter et al. (1982, p. 30), on the other hand, appear to believe that most of the difference between published and unpublished studies is the result of methodological quality. With due consideration of the viewpoints of both Hunter and Glass, in order to obtain a large and representative sample, this analysis included dissertations and unpublished ERIC documents as well as studies published in journals. In order to examine the effect of including unpublished studies, publication form was coded for use as a potential moderator variable.

4. Studies may be included in the meta-analysis regardless of quality. Studies were coded for internal validity so it could later be ascertained whether methodological quality was correlated with publication type.

5. Studies must report posttest scores for each learner-control treatment in a format useable for effect size calculations or these data must be available from the researchers. Phone, mail, and BITNET resources were used to contact the authors of studies who did not report data in a format useable for the calculation of effect sizes. If appropriate data did not become available, the study was not included in the meta-analysis.

Collecting Achievement Data (Dependent Variable)

As earlier defined, student scores on an immediate posttest of the computer-based lesson material were used as the measure of achievement. Researchers generally reported the mean posttest score for each treatment group, along with the standard deviation and treatment group N. These data were recorded for each treatment in a particular study and were later used in the calculation of the effect sizes for the meta-analysis.

If means and standard deviations were not available, other statistics such as F or t were used, when available, to calculate or estimate effect sizes according to procedures outlined by Glass, McGaw, and Smith (1981); Wolf (1986); and Hunter and Schmidt (1990).

Collecting Learner-Control Data for Each
Treatment Group (Independent Variable)

Learner-control factors for each treatment group in each study were identified and coded using the control-factor framework described in Chapter 2. The purpose of this coding procedure was to classify each treatment according to the type of learner control allowed in the treatment, thereby providing a standardized basis for comparison across studies. The seven control factors were pace, interaction, review, number of exercises, amount of material, sequence, and cognitive format.

A treatment received a 0 for a factor if that factor was controlled by the computer. A treatment received a 1 for the factor if it was controlled by the student. For example, Tennyson (1981) described one of his treatment groups as follows:

The learning program used the same response format as the two tests, but provided feedback on whether or not a response was correct... In the learner-control treatment condition, the subjects decided (a) whether to continue receiving examples or to go to the posttest and (b) which concept they wanted to study next. Subjects were informed in the program directions that they had complete

control over the amount and sequence of instruction. (p. 428)

In this treatment, the students controlled the pace (1), interacted with the program (1), did not control review (0), controlled the number of examples (1), controlled the amount of material (1), controlled the sequence (1), and did not control the cognitive format (0). Hence, this treatment would have been coded 1101110.

Collecting Study Characteristics (Moderator Variables)

The studies used in the meta-analysis differed across a broad range of characteristics stemming from methodological variables, subject pool variables, and courseware variables. It was possible that one or more of these characteristics could interact with the findings, so the following study characteristics were collected for examination as moderator variables:

Year of Publication

Publication Type (journal, dissertation, unpublished)

Student Class-in-School (0 - 12, undergraduate, graduate)

Instructional Media (computer-based text only, computer-based text plus graphics, computer plus audio, computer plus photocopied)

illustrations, computer plus slides, computer plus interactive video, multi-media)

Instructional Topic (math, language arts, sociology, psychology, science, art, education, political science)

Duration of Treatment

Number of Treatment Sessions

Computer Equipment (IBM, Apple II, Macintosh, mainframe, unknown)

Type of Experimental Design (1 - 16)

Threats to Internal Validity (0 - 8)

The type of experimental design was coded for each study using the classification system presented by Campbell and Stanley (1967): One-Shot Case Study (design 1), One-Group Pretest-Posttest (Design 2), Static Group Comparison (Design 3), Pretest-Posttest Control Group (Design 4), Solomon Four-Group (Design 5), Posttest-Only Control Group (Design 6), Time Series (Design 7), Equivalent Time Samples (Design 9), Equivalent Materials Samples (Design 9), Nonequivalent Control Group (Design 10), Counter Balanced (Design 11), Separate Sample Pretest-Posttest (Design 12), Separate Sample Pretest-Posttest Control Group (Design 13), Multiple Time-Series (Design 14), Recurrent Institutional Cycle (Design 15), Regression-Discontinuity Analysis (Design 16). Factorial designs

were classified according to the basic underlying design, e.g., Design 4, 5, or 6.

Threats to internal validity were determined for each study using Campbell and Stanley's (1967) criteria: history, maturation, testing, instrumentation, statistical regression, selection of respondents, experimental mortality, multi-group interactions. For each of the eight criteria, a rating of 0 or 1 was assigned. Zero (0) indicated no threat on the criterion. An assignment of one (1) indicated a threat on that criterion.

Avoiding the "Inflated N" Problem

Several researchers (Abrami et al., 1988; Bangert-Drowns, 1986; Rosenthal & Rubin, 1986) have suggested that each study should be represented in the meta-analysis by only one effect size. Specifically, the "inflated Ns" which result when studies are represented in the meta-analysis by the number of findings they report, may pose a threat to the external validity of the analysis. Bangert-Drowns (1986) cited a study in which 413 effect sizes were gathered from 33 studies. Some studies contributed as many as 120 effect sizes to the analysis, while others only contributed one effect size. In such a case, it is apparent that a small number of studies can have

disproportionate influence on the outcome of a meta-analysis.

There are a variety of reasons why a single study may have the potential to contribute more than one effect size to a meta-analysis. For example, a study may provide data for more than one outcome measure such as a series of posttests or other assessment measures. As another example, a study may have more than two treatment groups. Whereas a study with two treatment groups has the potential to contribute only one effect size to the meta-analysis, a study with more than two treatment groups has the potential to contribute a number of effect sizes equal to Pascal's coefficient, $C(n,2)$. Hence, a study with four treatment groups could potentially contribute six effect sizes.

For this meta-analysis, three strategies were applied as appropriate to ameliorate the inflated N problem. These strategies and their application are explained in the next three sections.

Combining Dependent Data

Some researchers reported achievement scores on more than one posttest. For example, students may have taken a recall test and a multiple choice test. As another example, the achievement test scores on a multi-unit instructional sequence may have been reported by unit as three separate scores, rather than

Table 5
Example of Combining Multiple Achievement Test Scores

	LEARNER CONTROL TREATMENT			COMPUTER CONTROL TREATMENT		
	MEAN	S.D.	VAR	MEAN	S.D.	VAR
Test 1	18	3.7	13.69	15	2.8	7.84
Test 2	17	3.5	12.25	17	1.6	2.56
Test 3	12	1.5	2.25	14	3.1	9.61
Pooled	47	5.31		46	4.47	

as a combined score. In such cases, the results of the achievement tests were pooled for each treatment. The pooled mean was obtained by summing the means for each of the reported tests by treatment group. The pooled standard deviation was obtained by computing the square root of the sums of the reported variances for each test. Table 5 provides an example of this procedure.

Partitioning Factorial Designs

In some studies, particularly those using factorial designs, more than one treatment group appeared to have the same type of learner control, indicated by the seven-factor learner-control coding. Table 6 provides an example of a hypothetical study using a 2 X 2 factorial design in which there are four treatments: T1, T2, T3, and T4. The two learner-control treatments, T1 and T3 might be coded 1111000. The two computer control treatments might be coded 1000000. Such a study could potentially generate six

effect sizes for the meta-analysis. Instead, this single study would have been partitioned into two studies, with each of the resulting studies only

Table 6
Example of a 2 X 2 Factorial Design which can be Partitioned into Two Studies (Reported Findings are Posttest Scores)

	LEARNER CONTROL	COMPUTER CONTROL
HIGH APTITUDE	T1	T2
MEAN	67.9	75.2
S.D.	3.4	3.8
LOW APTITUDE	T3	T4
MEAN	59.6	63.8
S.D.	5.3	4.2

contributing one effect size to the meta-analysis.

Table 7 illustrates how the study from Table 6 would have been partitioned. One of the resulting studies provides achievement data for T1 and T2; high-aptitude students under learner control and high-aptitude students under computer control. One effect size would be produced from this study. The second study produced as a result of the partition would provide achievement data for T3 and T4; low-aptitude students under learner control and low-aptitude students under computer control. One effect size would be produced from this study.

Table 7

Example of a Single Study Partitioned into Two Separate Studies using the Experimental Variable Aptitude

PARTITIONED STUDY #1			
	LEARNER CONTROL	COMPUTER CONTROL	
HIGH APTITUDE	T1	T2	
MEAN	67.9	75.2	
S.D.	3.4	3.8	
PARTITIONED STUDY #2			
	LEARNER CONTROL	COMPUTER CONTROL	
LOW APTITUDE	T3	T4	
MEAN	59.6	63.8	
S.D.	5.3	4.2	

Combining Similar Learner-Control Treatments

In some studies, multiple treatment groups had the same learner-control characteristics but did not differ significantly on other experimental variables. In cases such as this, a weighted average of the treatment means and standard deviations was used in the effect size calculation.

Data from a hypothetical study is presented in Table 8. In this hypothetical study, treatments for Groups 1, 2, and 3 provided the same type of learner control and thus the control factor code, 0100000, is the same in each case. These three treatments ($G = 3$) would have been compressed into one treatment with a mean of 16.1, a standard deviation of 3.21, and $N =$

Table 8

A Hypothetical Example for Combining Treatment Groups
With Similar Learner Control Characteristics

TREATMENT	CONTROL FACTOR	MEAN	N	S
Group 1	0100000	15.6	12	3.30
Group 2	0100000	14.2	12	2.60
Group 3	0100000	18.5	12	3.90
Group 4	1100000	12.3	12	4.40
Group 5	1100000	14.9	12	4.90
Group 6	1100000	13.6	12	2.50

36 using the following calculations:

$$\text{Pooled mean} = \sum \bar{X}_i / G$$

where G = the number of treatment groups.

$$\text{Pooled variance} = \frac{\sum (N_i - 1) S_i^2}{(\sum N_i) - 1}$$

where S^2 is the treatment group variance,
and N_i is the treatment group size.

$$\text{Pooled } N = \sum N_i$$

In the present analysis, some studies had the potential to contribute more than one effect size. However, by applying the strategies mentioned above for avoiding the inflated N problem, a single study would not be expected to contribute a disproportionate number of effect sizes to the meta-analysis. In addition to these techniques, the bare bones meta-analysis calculations utilized an N weighted by the number of subjects in the treatment groups, ameliorating, though not eliminating, some inflation of N s for studies which

entered more than one effect size into the meta-analysis. The weighting strategy is discussed in the section on bare bones meta-analysis.

Statistical Procedures

As stated in Chapter 1, the scope of this dissertation probed three topics associated with learner control: (a) the characteristics of the body of learner-control research, (b) the effect of learner control on student achievement, and (c) the relationship between selected moderator variables, learner control, and student achievement. The statistical procedures for examining these topics are described in the next sections.

Characterizing Learner-Control Research

The first research question examined by this dissertation was:

1. What are the characteristics of the body of learner-control research which has examined the effect on student achievement of learner control?

In order to summarize the characteristics of the body of learner-control research, the following data, described in the preceding sections of this chapter, were collected for each study: study date, primary researcher, publication type, type of experimental design, number of threats to internal validity, number of treatments, duration of each treatment session,

instructional media, type of computer equipment used, and type of learner control studied. In addition, the dependent and independent variables appearing in each study were recorded. Descriptive statistics such as means, standard deviations, and ranges were used to summarize these data and answer the following questions:

1. a. How many studies have been published?
1. b. When were the studies performed?
1. c. Who were the major researchers?
1. d. What variables have been researched?
1. e. What types of research designs were used?
1. f. What was the quality of the research?

Examining the Effect of Learner Control on Achievement

The second research question set required statistical procedures for calculating effect sizes and performing the meta-analysis. Question set two was:

2. Is there a difference in the achievement of students who are provided with learner control and students who are provided with other control models?
 - 2.a. What is the effect on student achievement of providing learner control?
 - 2.b. Do specific learner-control configurations have an effect on student achievement?

2.c. What is the effect on achievement of specific learner-control factors such as pace, interaction, review, or sequencing?

The analysis of this question set required four steps. First, the subset of treatment pairs appropriate for the particular research sub-question was selected. Second, an effect size was generated for each treatment pair. Third, a bare bones meta-analysis was performed to summarize the effect sizes. Fourth, the results of the bare bones meta-analysis were interpreted in order to determine whether it was necessary to search for moderator variables. The next sections detail the procedures used for each of these steps.

Selecting Treatment Pairs for Meta-Analysis

The effect size calculation is based on a standardized difference score between two treatments within the same study, so effect sizes were calculated for pairs of treatments that offered different configurations of learner control.

The process of treatment-pair selection for research questions 2.a., 2.b., and 2.c. specified that one treatment provide a greater number of learner-control factors than the other treatment. For example, a treatment coded 1101000 would have three learner-control factors and could be paired with a treatment

coded 1000000 with only one learner-control factor or a treatment coded 1100000 with two learner-control factors. Two treatments providing the same number of learner-control factors, but in a different configuration, would not be paired for analysis.

For research question 2.a., an effect size was determined for any treatment pairs within a given study which met the criteria of a different number of control factors specified above. The effect sizes from all studies were then pooled and weighted to determine the average effect size, $ave(d)$, and to estimate the population effect size, δ .

In order to examine the diverse combinations of control factors for research question 2.b, treatment-pair selections for a number of analyses were required. These are described, along with the results of the analyses, in Chapter 4.

For research question 2.c., effect sizes were calculated as for research question 2.a. In addition, a binary mask was used to determine which learner-control factors differed between the treatment pairs. For example, if the treatments were coded 1000000 and 1100000, the treatments differed on factor 2, yielding a new coding of 0100000. This new code was used as the basis for a series of analyses of treatment pairs which differed only on the specified factor.

A dBase III+ command file, GENERATE, was used to select the appropriate treatment pairs for analysis. The source code for this command file is provided in Appendix B.

Calculating Effect Sizes

Effect Size (d) was used as the standardized measure of achievement from each of the treatment pairs. Effect size was calculated from the means and standard deviations which had been collected for treatment-group pairs using the formula suggested by Hunter et al. (1982):

$$d = (\bar{X}_E - \bar{X}_C) / s_w$$

where \bar{X}_E is the mean of the experimental group,
 \bar{X}_C is the mean of the control group,
 and s_w is the pooled within-group standard deviation.

Note that the subscript C, though used to indicate the control group may represent any experimental group with which learner control is being compared. Hence, the subscript C is used to indicate the treatment in which the learner controls fewer factors. The subscript E is used to indicate the treatment with more learner control, based on the number of factors under the learner's control (i.e., given treatments 1110000 and 1010000, treatment 1110000 in which the learners controlled three factors would be used as Treatment E).

For studies that reported means and standard deviations for achievement, these statistics were used to calculate effect size. In other cases, effect size was calculated from other statistics such as t and F using procedures outlined by Hunter and Schmidt (1990, p. 272), Wolf (1986), and Glass et al. (1981). Effect size calculations were performed by dBase III+ command files CALCES and MESH, provided in Appendix B, written by the author and verified using data sets from Glass et al. (1981, p. 102). Essentially, these command files express in dBase III+ syntax the formula for effect size calculation presented at the beginning of this section.

Bare Bones Meta-Analysis

Hunter et al. (1981) have defined a procedure for cumulating effect sizes, referred to as bare bones meta-analysis which was followed for this meta-analysis. The frequency weighted mean and variance of the study sample effect size (d) were computed for all studies, a correction was made for the variance of sampling error, and the standard deviation and confidence interval were calculated. The procedure was as follows:

1. Effect size (d) was calculated for each pair of treatments using:

$$d = (\bar{X}_E - \bar{X}_C) / s_w$$

where \bar{X}_E is the mean of the experimental group,
 \bar{X}_C is the mean of the control group,
and s_w is the pooled within-group standard deviation.

If the treatment group N was the same for both treatments in the pair, the pooled within-group standard deviation was calculated using:

$$s_w = \sqrt{(s_E^2 + s_C^2) / 2}$$

If the treatment group Ns were unequal, the pooled within-group standard deviation was calculated using:

$$s_w = \sqrt{\frac{(N_E - 1) V_E + (N_C - 1) V_C}{N_E + N_C - 2}}$$

where N_E is the experimental group sample size,
 N_C is the control group sample size,
 V_E is the variance of the experimental group,
and V_C is the variance of the control group.

2. The weighted average of d was calculated:

$$\text{Ave}(d) = \sum w_1 d_1 / \sum w_1 = D$$

where w_1 = weight (treatment group₁ sample size)²

3. The correspondingly weighted variance of d was calculated:

² Though a standard procedure for obtaining weighted estimators of effect size would use $w_1 = 1/\text{Var}(\delta_1)$, Hedges and Olkin (1985) point out that the sampling variance of δ_1 is a function of the unknown parameter δ . Hence, the optimal weights cannot be calculated exactly in most applications. However, "because the sampling variance of δ_1 is approximately proportional to $1/n_1$, the use of $w_1 = n_1$ results in a weighted estimator that is reasonably close to optimal" (p. 302).

$$\text{Var}(d) = \sum w_1 [d_1 - D]^2 / \sum w_1$$

4. The average sampling error variance was calculated:

$$\text{Var}(e) = \sum w_1 \text{Var}(e_1) / \sum w_1$$

where the sampling error variance within each study ($\text{Var}(e_1)$) was³:

$$\text{Var}(e_1) = (4/N_1) (1 + \delta_1^2 / 8)$$

Since δ_1 is unknown, Hunter and Schmidt (1990, p. 285) have suggested substituting the mean value of d (D) for δ_1 and using the average sample size N for N_1 :

$$\text{Var}(e) = (4/N) (1 + D^2/8)$$

where $N=T/K$,
T=total sample size,
K=the number of studies.

5. The standard deviation of study population effect sizes ($\text{SD}(\delta)$) was estimated:

$$\text{Ave}(\delta) = \text{Ave}(d)$$

$$\text{Var}(\delta) = \text{Var}(d) - \text{Var}(e)$$

$$\text{SD}(\delta) = \sqrt{\text{Var}(\delta)}$$

6. The confidence interval for the mean of the population effect size was calculated:

$$\text{Ave}(\delta) - 1.96(\text{SD}(\delta)) < \delta < \text{Ave}(\delta) + 1.96(\text{SD}(\delta))$$

Computations for the bare bones meta-analysis were performed by a dBase III+ command file, BBMETA. The

³ The derivation of this formula for sampling error variance was provided by Hedges (1980, p. 43).

source code for the command file is provided in Appendix B. This command file was written by the author and verified using data sets provided by Hunter and Schmidt (1990, p. 287 - 301).

Interpretation Procedures for Meta-Analysis

The results of the bare-bones meta-analysis were interpreted with regard to (a) the size of the treatment effect, (b) the size of the treatment effect in relation to the variance of treatment effect, (c) whether the confidence interval included zero, and (d) the clinical implications of the confidence interval obtained for the mean of the population effect size.

Size of treatment effect. Cohen (1977) provided a rule-of-thumb for interpretation of d . He suggested that if d is less than or equal to .20, treatment effects are too small to be observed without special measuring procedures and should be considered negligible. Values of d between .20 and .50 should be considered small. Values of d between .50 and .80 may be considered medium or moderate. Values of d which exceed .80 may be considered large. The d -value categories suggested by Cohen were used for an initial interpretation of average effect sizes obtained from the bare bones meta-analysis.

Effect size in relation to the variance. The effect size, d , is not a sufficient indicator of the impact of experimental treatments. The effect size for a group of studies, $Ave(d)$, may have a non-zero variance, $Var(d) > 0$, indicating that not all studies produced the same effect. When $Ave(d)$ is used to estimate the population effect (δ) the average population effect size ($Ave(\delta)$) will reflect the variance of the sample less the sampling error variance. The variance of $Ave(\delta)$ is then used to compute the standard deviation of δ ($SD(\delta)$) and obtain the confidence interval for δ :

$$Ave(\delta) - 1.96(SD(\delta)) < \delta < Ave(\delta) + 1.96(SD(\delta))$$

The interpretation of this confidence interval is discussed by Hunter and Schmidt (1990):

Even if there is some variation across studies, the variance may still be small enough to ignore for practical or theoretical reasons. If the variation is large, especially if it is large relative to the mean value, then there should be a search for moderator variables. (p. 287)

Though no parameters were specified by Hunter and Schmidt for the value of a "large" variation, from a clinical perspective, the concept of large variation was given meaning by examining the implication of the confidence interval. If the confidence interval spanned

more than one of Cohen's categories, a search for moderators was performed. In other words, if the confidence interval was such that δ could be classified as having effects ranging from small to large, it seemed appropriate to search for variables which would account for the discrepancy. If, on the other hand, the confidence interval did not span more than one category, the variance in δ was assumed to be unimportant from the clinical perspective and the effect size was assumed to be essentially equal to the value of $\text{Ave}(\delta)$.

Zero in the confidence interval. If the confidence interval included zero, there was a chance that the actual value of δ was zero. In interpreting the results of the bare bones meta-analysis, a confidence interval which included zero was particularly meaningful if the variance was small. In this case, it could generally be concluded that the treatment had no significant effect. However, if the confidence interval included zero and the variance was large (i.e., spanning more than one of Cohen's categories), a search for moderator variables was indicated. The hypothesis that the treatment had no effect was not accepted until a search for moderator variables exhausted the possibility of an effect that varied due to some additional moderator.

Clinical impact of the confidence interval. Let L represent the lower limit of the confidence interval for δ and let U represent the upper limit of that confidence interval. Once $SD(\delta)$, $Ave(\delta)$, L , and U were known, a standard table of z values for the normal distribution was used to obtain: (a) the percentage (P_L) of the control group which would be surpassed if the effect size was the lower limit of the confidence interval and (b) the percentage (P_U) of the control group which would be surpassed if the effect size was the upper limit of the confidence interval.

From a clinical perspective it was judged useful to search for moderator variables if (a) the spread between the upper and lower limits of the confidence interval was of such magnitude that it would be useful to identify the variables which accounted for the variation and if (b) raising the lower limit of the confidence interval would increase the usefulness of the treatment effect. Some examples follow.

When $P_L = 80$ and $P_U = 95$, it can be stated with 95% confidence ($p = .05$) that at least 80% of the control group but no more than 95% of the control group would be surpassed by the treatment group. It might not be useful to search for moderator variables in such a case since it is clear that even at the lower limit of the confidence interval, the treatment produced a

highly positive effect. On the other hand, in a case with a similar 15 point spread, but in which $P_L = 45$ and $P_U = 60$, it is within the realm of probability that the treatment actually had a negative effect. In this case, it would be useful to search for moderator variables which might produce negative effects, particularly if the 60% upper limit was considered clinically useful. Finally, in the case in which $P_L = 48$ and $P_U = 55$, it may not be useful to identify moderator variables since moving the lower limit of the confidence interval closer to the upper limit of 55 would not produce a particularly impressive difference between treatment and control groups.

Examining the Effect of Moderator Variables

The third research question set examined by this dissertation was:

3. Do specific moderator variables interact with learner control to produce different achievement effects?

3.a. Does student age or class in school interact with learner control to produce different effect sizes for student achievement?

3.b. Does the topic of instruction interact with learner control to produce different effect sizes for student achievement?

3.c. Does the type of computer equipment used for the study interact with learner control to produce different effect sizes for student achievement?

3.d. Do the instructional media interact with learner control to produce different effect sizes for student achievement?

3.e. Are the effect sizes for student achievement under learner control in published studies significantly different from those reported in unpublished studies or from those reported in dissertations?

3.f. Do effect sizes for student achievement under learner control differ by experimental design?

3.g. Do effect sizes for student achievement under learner control differ by study date.

3.h. Do effect sizes for student achievement under learner control differ by researcher?

3.i. Does the length of exposure to learner control treatments have an effect on achievement?

The effect of a potential moderator variable was examined by conducting separate meta-analyses on subsets of the data representing values of the potential moderator. For example, the potential moderator class-in-school was examined by conducting a

meta-analysis for three subsets: one for elementary school, one for high school, and one for college. This subset technique was used for the potential moderators class-in-school, topic of instruction, type of computer equipment, instructional media, publication type, experimental design, study date, researcher, and length of treatment.

Hunter and Schmidt (1990, p. 408) suggested caution in the use and interpretation of significance tests in moderator variable analysis. They said:

Hedges and Olkin have argued against this procedure on the grounds that the assumption of homogeneity of observation sampling error variances is usually not met in meta-analysis data sets...Heterogeneity of variances can affect the validity of significance tests; actual alpha levels may be larger than nominal levels (e.g., .15 versus the nominal .05).

Yet, they continued:

The problem identified by Hedges and Olkin may not be serious. The general finding has been that most statistical tests are robust with respect to violations of the assumption of homogeneity of variance.

Hunter and Schmidt offered a further caveat:

Statistical significance tests do not and should not play an important role in meta-analysis.

Overreliance on significance tests has historically been the cause of many of the problems of inappropriate data interpretation that meta-analysis seeks to solve.

Consequently, the results of the analyses of potential moderator variables were interpreted under the proviso that mean effect size and confidence interval are the most important, and perhaps the most reliable, information for clinical application of findings.

For interpretation, the effect size of a potential moderator variable was classified using Cohen's categorization of effect sizes mentioned earlier. Next, the confidence interval was examined. If the confidence interval did not span more than one of Cohen's categories, the variable was considered a moderator with an effect equal to the magnitude indicated by $Ave(\delta)$. If the confidence interval spanned more than one of Cohen's categories, the variable was considered to be a possible moderator with a range of interaction effects. If the confidence interval included zero, the variable was not considered a reliable moderator.

Summary

The methodology for this study of learner control in computer-based learning environments used a combination of two approaches to meta-analysis: the Hunter-Schmidt approach called psychometric meta-analysis and the classical Glassian approach. The data were derived from the reported findings of learner-control studies. An exhaustive literature search was conducted to locate learner-control studies. Data were collected for the dependent variable *student achievement*, the independent variable *learner control*, and selected variables identified as potential moderators. The data were then used as input to a series of statistical analyses in order to examine the three research question sets within the defined scope of the dissertation.

Descriptive statistics were used to characterize the body of learner-control research in order to answer the first set of research questions.

To answer the second set of research questions, a series of meta-analyses were used to determine if there were differences in achievement for students provided with learner control and students provided with other control models. The meta-analyses yielded weighted averages of effect sizes, weighted variances of effect sizes, and average sampling error variances. For each

analysis, the standard deviation and confidence interval for study population effect size were calculated and used to interpret the size of the treatment effect and to determine whether a search for moderator variables was in order.

The effect of possible moderator variables was examined in order to answer the third set of research questions. The sample studies were grouped into subsets based on the coded values of the variable. Meta-analyses were then conducted for these subsets.

The results of the analysis and search for moderator variables are reported in Chapter 4.

CHAPTER 4

RESULTS

The purpose of this dissertation was three-fold: (a) to characterize the body of research on learner control of computer-based learning environments, (b) to examine the effect on achievement of providing learner control, and (c) to examine the interaction between selected courseware and student variables, learner control, and student achievement. Meta-analysis, a quantitative alternative to narrative review, was used to integrate the results of a collection of primary research studies on learner control.

This chapter has three sections. The first section reports data which characterize the studies used in the meta-analysis. The second section reports the results of the meta-analysis of the effect of learner control on student achievement. The third section reports the results of the analysis of interaction between moderator variables, learner control, and student achievement.

Characteristics of the Studies used in the Meta-Analysis

The first research question examined by this dissertation was:

1. What are the characteristics of the body of learner-control research which has examined the effect on student achievement of learner control?

The following sub-questions were defined:

1. a. How many studies have been published?
1. b. When were the studies performed?
1. c. Who were the major researchers?
1. d. What variables have been researched?
1. e. What types of research designs were used?
1. f. What was the quality of the research?

The intention of this section of the chapter is to provide general descriptive statistics pertaining to the body of learner-control research which was used for the meta-analysis. These statistics are provided on a *study-by-study* basis following the meta-analytic tradition of Glass et al. (1981).

How Many Studies have been Published?

The on-line search of the PsycInfo database using the search specification LEARNER(W)CONTROL, COMPUTER ASSISTED INSTRUCTION/DE /ENG produced 51 citations. Searches of the ERIC CD-ROMS dated 1966-1981 and 1/82-6/91 using the search specification LEARNER CONTROL produced 22 and 99 citations respectively. After eliminating duplicate citations and citations for reports which were not of empirical research, the number of empirical studies of learner control was 106.

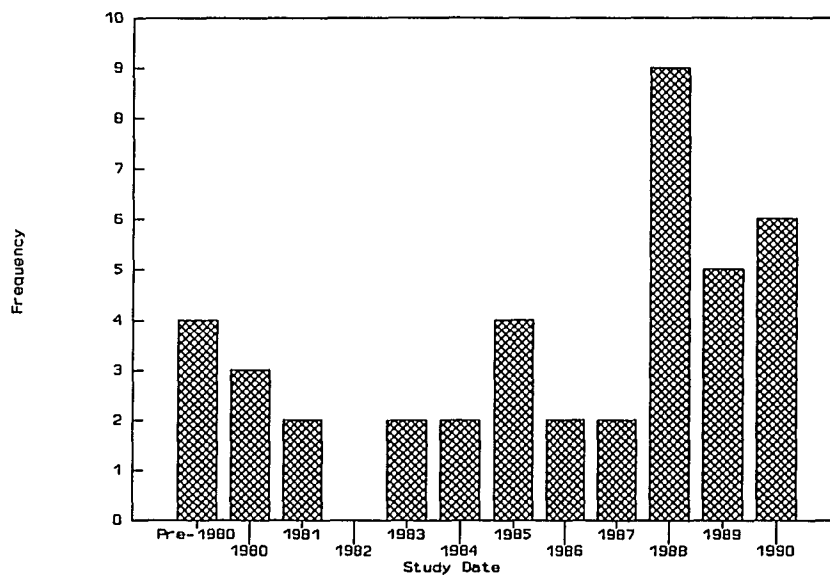
Twenty-eight of these studies were eliminated because they did not have two or more treatments under different control formats. Nine studies were eliminated because they were not computer based. Fifteen studies were eliminated because the dependent variable was not a posttest. Ten studies were eliminated because the data available was insufficient for entry into the meta-analysis despite attempts to directly contact the researchers. Three studies were unobtainable. The meta-analysis was performed using 41 studies which met the criteria for inclusion stated in Chapter 3. These studies are listed in Appendix A. The data reported in the sections which follow were obtained from the 41 studies included in the meta-analysis.

When were the Studies Published?

Publication dates for the 41 studies ranged from 1974 to 1990. Figure 1 presents a histogram of the frequency distribution of publication dates for the studies.

Of the 41 studies used for the meta-analysis, 32 (78.0%) were published in journals, 7 (17.0%) were dissertations, and 2 (5.0%) were unpublished ERIC documents.

Figure 1
Publication Date Frequencies for the 41 Studies Used
in the Meta-Analysis



Who were the Major Researchers on the
Topic of Learner Control?

The studies used for the meta-analysis represented the work of 65 researchers. Some researchers contributed to more than one study and are listed in Table 9.

What Variables have been Researched?

In order to be included in the meta-analysis, studies were required to have achievement scores from a posttest as the dependent variable. The required independent variable was type of control, meaning that subjects in one of the treatment groups were required

Table 9

Researchers Who Conducted More Than One Learner Control Study Used in the Meta-Analysis

RESEARCHER	NUMBER OF STUDIES
Tennyson, Robert D.	7
Carrier, Carol A.	3
Sullivan, Howard J.	3
Williams, Michael D.	3
Cartwright, G. Phillip	2
Davidson, Gayle	2
Ellermann, Henk H.	3
Free, Elso L.	2
Judd, William A.	2
Kinzie, Mabel B.	2
O'Neil, H. F.	2
Park, Ok-Choon	2
Schloss, Patrick J.	2
Spelt, P.F.	2

to have a different type of instructional control than subjects in another treatment group. Therefore, all studies in the meta-analysis had posttest scores as a dependent variable and type of instructional control as an independent variable. However, some studies examined additional dependent and independent variables. Table 10 lists the independent variables, the dependent variables, and their frequencies.

What Type of Research Designs were Employed?

Research designs for the studies used in the meta-analysis were categorized using the Campbell-Stanley model (Campbell & Stanley, 1963). Four (9.7%) of the studies used the Pretest-Posttest Control Group Design (Design 4). The remaining 37 (90.3%) studies used the Posttest-Only Control Group Design (Design 6).

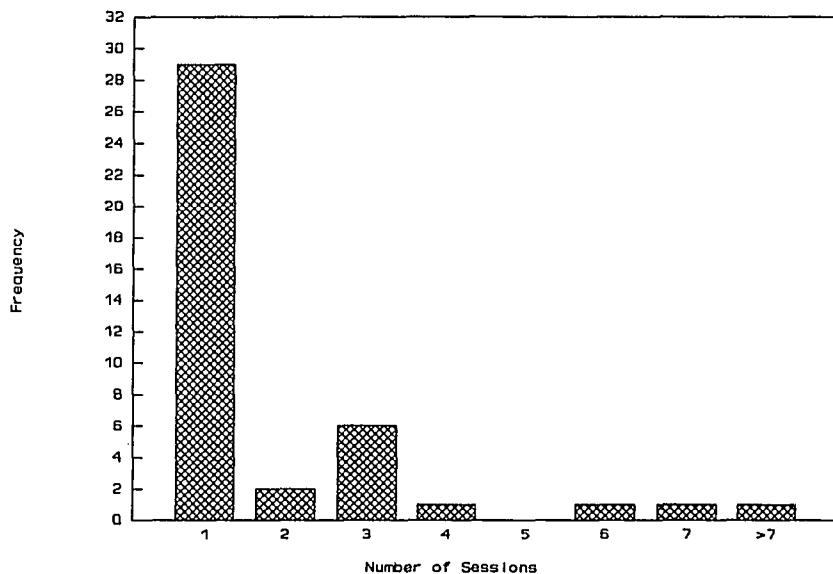
Table 10 121
Dependent and Independent Variables Examined by the
Studies Used in the Meta-Analysis

DEPENDENT VARIABLE	FREQUENCY
Anxiety	2
Attitude	15
Confidence	4
Posttest achievement	41
Response time	1
Retention test	12
Time on task	26
Use of learner control	18
INDEPENDENT VARIABLE	
Age	1
Cognitive style	7
Collaboration	1
Ethnicity	1
Gender	7
General aptitude	5
Internal locus of control	5
Memory	2
Reading ability	2
Specific aptitude	16
Type of learner control	41
Type of learning	4

Sample sizes for individual studies ranged from 19 to 700. The total number of subjects in the 41 studies was 3903. The average study sample size was approximately 95.

The number of sessions in which subjects received experimental treatments was reported for all studies and varied from 1 to 18. Experimental designs which used only one treatment session were the most numerous as can be seen from Figure 2. No studies were full replications of other studies based on the study characteristics coded for this dissertation.

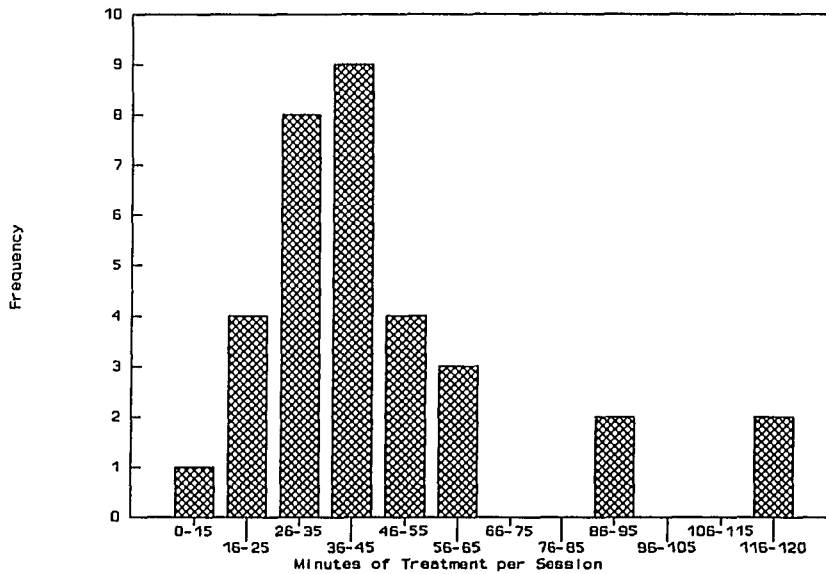
Figure 2 122
Number of Experimental Treatment Sessions per Study
(N=41)



The duration of each experimental session was reported for 33 studies and varied from 15 to 120 minutes. The frequency distribution of this duration is shown in Figure 3. Additional descriptive statistics for the duration of each treatment are contained in Table 11.

The total treatment time could be determined for 33 studies by multiplying the number of sessions by the treatment time per session. Total treatment times for each study ranged from 15 to 720 minutes as illustrated in Figure 4. The mean treatment time was approximately 112 minutes.

Figure 3
Duration of Experimental Treatment Session for 33
Learner Control Studies



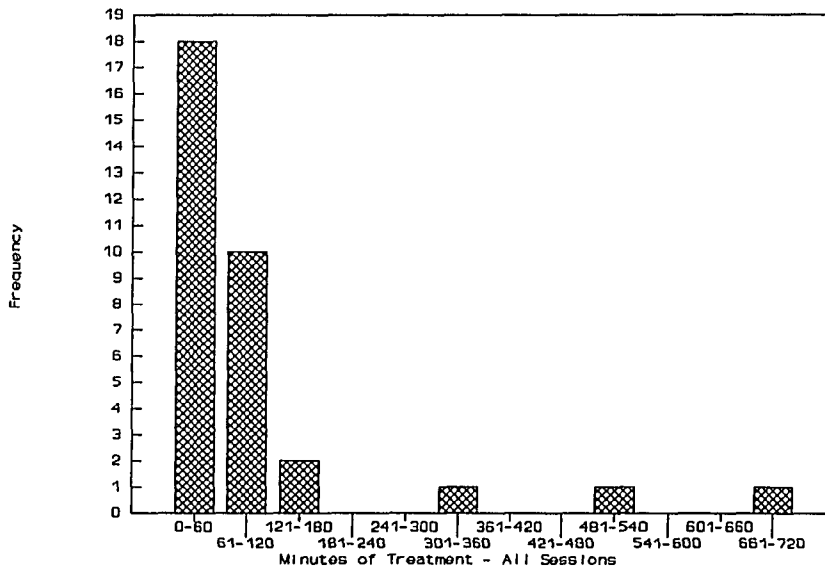
The 41 studies entered into the meta-analysis used courseware in various subject area disciplines.

Table 11
Descriptive Statistics for Duration of Treatment per
Session

Mean	46.576	Std Err	4.459	Median	40.000
Mode	40.000	Std Dev	25.613	Variance	656.002
Kurtosis	2.709	S E Kur	.798	Skewness	1.659
S E Skew	.409	Range	105.000	Minimum	15.000
Maximum	120.000	Sum	1537.000		

Valid Cases 33 Missing Cases 8

Nineteen (46.3%) used science courseware. Six (14.6%) used courseware for language arts such as English, reading, and foreign language. Five studies (12.2%)

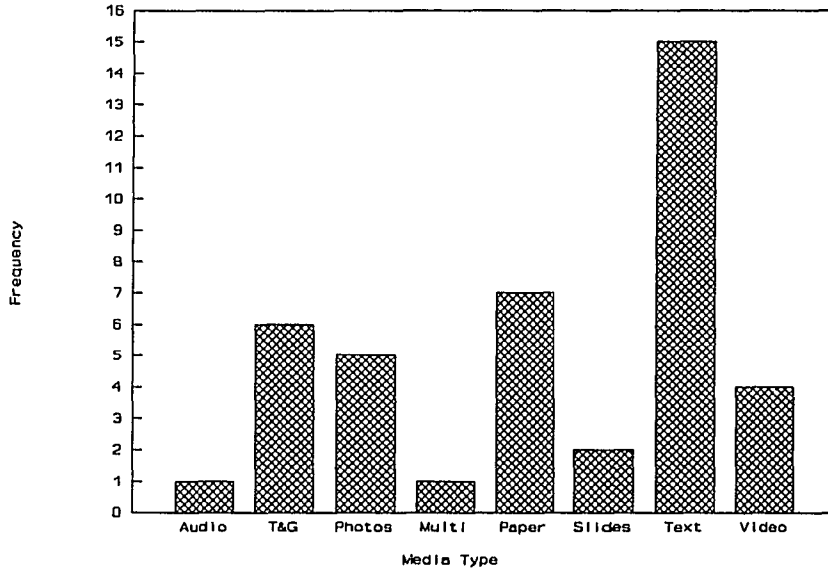
Total Treatment Time for all Sessions in 33 Studies

used sociology courseware. Four studies (9.7%) used math courseware. Three studies (7.3%) used psychology courseware. Two studies (4.9%) used courseware in the field of education. Art and political science were the subject of the courseware in one study each.

Twenty-nine of the 41 studies reported the type of computer equipment used. Computers from the Apple II family were used in 15 studies, IBM personal computers and compatibles were used in 5 studies, the Macintosh was used in one study, and mainframe terminals were used in 8 studies.

For inclusion in the meta-analysis, studies were required to have a computer as the controlling device

Figure 5
Instructional Media Used by the Learner Control
Studies in the Meta-Analysis



within the learning environment. In many cases, other instructional media were used to supplement the computer. Computer-based text-only courseware was used in 15 studies (36.6%). Computer-based text with additional screen-based still-graphics were used in 6 studies (14.6%). One study augmented the computer lesson with audio sound generated by a tape recorder. Five studies (12.2%) augmented computer-based text with photocopied pictures. Seven studies (17.1%) augmented the computer-based text with paper-based text. Two studies (4.9%) augmented computer-based text with slides. Four studies (9.7%) used computer-controlled

interactive video. One study used multiple media supplements to the computer-based text, including animation and paper-based text. These findings are summarized in Figure 5.

What was the Quality of the Studies?

For each study, potential threats to internal validity were assessed using the eight variables proposed by Campbell and Stanley (1963, p. 5); history, maturation, testing, instrumentation, statistical regression, selection, experimental mortality, and selection-maturation interaction. Each of these variables, if not controlled in the experimental design, could have produced effects which might be confounded with the effect of the experimental variable. Studies were assigned one point for each variable which did not appear to be controlled by the nature of the experimental design or by some specified procedure.

Thirteen studies (31.7%) appeared to be well-designed and lacked apparent threats. Twenty-three studies (56.1%) appeared to have one potential threat. Four studies (9.7%) appeared to have two potential threats. One study (2.5%) had three potential threats.

The SPSSx Crosstabs procedure was used to examine the null hypothesis ($H_{(0)diff}$) that the quality of journal articles, represented by the number of

Figure 6
Crosstabulation for Publication Form by Threats

		THREATS			Row Total
		Count	Row Pct	Col Pct	
PUB. FORM		0	1	2-3	
		JOURNAL	1	10 31.3 76.9	20 62.5 87.0
OTHER	2	3 33.3 23.1	3 33.3 13.0	3 33.3 60.0	9 22.0
Column Total		13 31.7	23 56.1	5 12.2	41 100.0

CHI-SQUARE	VALUE	DF	SIGNIFICANCE
Pearson	5.29983	2	.07066
Likelihood Ratio	4.56855	2	.10185
Mantel-Haenszel	1.06825	1	.30134
Minimum Expected Frequency -		1.098	
Cells with Expected Frequency < 5 -		3 OF	6 (50.0%)

potential threats to internal validity, was not significantly different from that of dissertations and unpublished ERIC documents. Figure 6 contains the results of the Crosstabs procedure. Based on the results of the Pearson chi-square, 5.29983 ($p=.07066$, $df=2$), the null hypothesis ($H_{(0)diff}$) was not rejected⁴.

⁴ Everitt (1977, p. 40) presented a discussion of the validity of the chi-square test for tables with a minimum expected frequency less than 5. He cited Lewontin and Felsenstein's (1965) conservative rule: "The 2 X c table can be tested by the conventional chi-square criterion if all expectations are 1 or greater" and noted that these authors further indicated that in the majority of cases the chi-square can be used for tables with minimum expected values of at least .05.

Hence, dissertations and unpublished ERIC documents were retained in the meta-analysis under the assumption that they would not introduce bias related to study quality.

The Effect of Providing Learner Control

The second research question examined by this dissertation was:

2. Is there a difference in the achievement of students who are provided with learner control and students who are provided with other control models?

Sub-questions were defined as:

2. a. What is the effect on student achievement of providing learner control?

2. b. Do specific learner-control configurations have an effect on student achievement?

2. c. What is the effect on achievement of providing specific learner-control factors such as pace, interaction, review, or sequencing?

The inferential nature of the second research question and its sub-questions, required further transformations of the data in order to conduct the analysis on a treatment-by-treatment or subject-by-subject basis, rather than on a study-by-study basis. As explained in Chapter 3, each treatment within each study was separately coded. To minimize the "inflated

N's" problem, factorial designs were partitioned, similar learner-control treatments were combined, and multiple posttest scores were pooled.

Table 12
Frequency Distribution of Learner Control Factors
Provided by Study Treatments

LEARNER-CONTROL FACTOR	FREQUENCY
Pace	120
Interaction	123
Review	48
Number of exercises	49
Amount of material	36
Sequence	29
Cognitive format	6

As a result of partitioning, combining, and pooling, the original group of 41 studies was partitioned into 54 separate studies yielding 134 treatments. The minimum number of treatments provided by any single one of the newly partitioned studies was 2. The maximum number of treatments was 6.

As can be seen from Table 12, interaction was the factor most frequently provided for learner control in all the treatment groups. Note that it was possible for a study to provide control over more than one factor, so the frequencies in the table exceed 134, the number of studies. Interactive capabilities were provided by 123 of the 134 treatments.

Table 13

Frequency Distribution of the Number of Learner Control Factors Provided by Study Treatments

NUMBER OF LEARNER-CONTROL FACTORS	FREQUENCY
0	1
1	16
2	43
3	18
4	35
5	14
6	4
7	3

Table 13 indicates that the most common model of treatment was to provide two factors for the learner to control. The data resulting from study partitioning is included in Appendix A. These were the raw data used for the calculation of effect sizes.

What is the Effect on Student Achievement of Providing Learner Control?

The hypotheses associated with this research question were:

H_1 Providing learner control has an effect on student achievement.

$H_{(0)1}$ Providing learner control does not have an effect on student achievement.

To investigate these hypotheses, effect sizes were calculated for treatment pairs in all studies under the stipulation that the control configuration of one treatment within a particular study provided learners with control over a greater number of factors than the

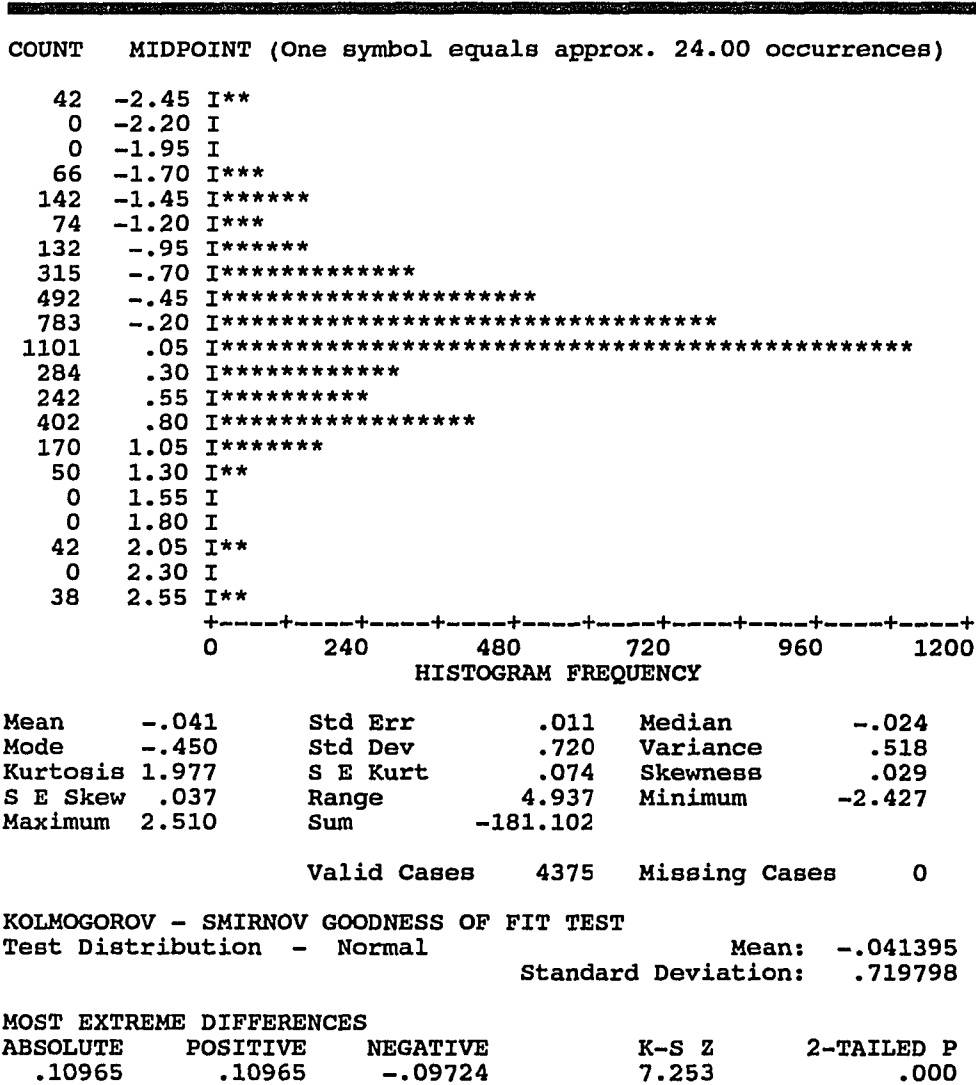
control configuration of the other treatment in the study. Hence, the generated effect size represented a comparison between treatments which provided more learner control and treatments which provided less learner control.

Table 14
Summary Statistics for the Meta-Analysis of the Effect on Achievement of Learner Control

Subject N	4375
No. Effect Sizes	94
Average(d)	-.0414
Variance(d)	.5180
Variance(e)	.0899
Ave(δ)	-.0414
Variance(δ)	.4281
SD(δ)	.6543
Confidence Interval	-1.32< δ <1.24

Using this specification, 94 treatment pairs were entered into the analysis with a total N of 4,375. The effect sizes for these treatment pairs are listed in Appendix A. SPSSx was used to carry out the Kolmogorov-Smirnov Goodness of Fit Test to ascertain whether the distribution of effect sizes approximated the normal curve. The computed Kolmogorov-Smirnov Z was 7.253 with a 2-tailed probability of .000, indicating a distribution that did not fit the normal curve. Figure 7 shows the distribution of effect sizes, and the results of the Kolmogorov-Smirnov Test. Hence, Hunter's caveats on the issue of statistical

Figure 7
Histogram of Effect Sizes and Kolmogorov-Smirnov
Goodness of Fit Results



significance tests brought out in Chapter 3, were of particular relevance to the interpretation of this data set.

The results of the analysis are reported in Table 14. The average effect size, $-.0414$ was

negligible and the confidence interval included zero. This indicated that the null hypothesis $H_{(0)1}$ should not be rejected. The analysis indicated that only 17% of the observed variance in effect sizes could be attributed to sampling error. The remaining large variance in relation to the mean effect size, $Ave(\delta)$, indicated that further analysis and search for moderator variables was in order.

Additional interpretation of these results also suggested the need to search for moderator variables: The lower limit of the confidence interval (-1.32) was the level below which 10% of the control group would fall. The upper limit of the confidence interval (1.24) was the level below which 90% of the control group would fall. Hence, at least 10%, but not more than 90% of the control group would be surpassed by the treatment group under learner control. Under these circumstances, it was thought useful to determine if moderator variables could account for some of the effects at either the lower or upper limits of the confidence interval.

Do Specific Learner-Control Configurations have an
Effect on Student Achievement?

The hypotheses associated with this research question were:

- H₂ Specific learner-control configurations have an effect on student achievement.
- H₍₀₎₂ Specific learner-control configurations do not have an effect on student achievement.

Table 15
Summary Statistics for Treatment Pair 1110000 and 1100000

Subject N	430
No. Effect Sizes	10
Average(d)	.1009
Variance(d)	.1293
Variance(e)	.0987
Ave(δ)	.1009
Variance(δ)	.0315
SD(δ)	.1774
Confidence Interval	-.25 < δ < .45

The approach to this research question was to examine treatment pairs which differed in the number of learner-control factors or in the type of learner-control configuration provided to the student. For example, one analysis examined the average effect size produced when treatments which provided 3 learner-control factors were paired with treatments which provided 1 learner-control factor.

The most common configuration of treatment pairs was coded 1110000 and 1100000, a comparison of learner

control of pace, interaction, and review paired with learner control of pace and interaction. Table 15 shows the results of the meta-analysis for this treatment pair. The average effect of providing learners with control over review produced a fairly negligible positive effect on student achievement of .1009. The confidence interval included zero, so it could not be concluded that this configuration produced a non-zero effect. The fact that the confidence interval ranged from -.25 to .45 may mean that moderator variables were operating.

A series of analyses were performed on treatment pairs selected on the basis of the number of learner-control variables provided. Four of these analyses were based on sufficient sample size to report. The

Table 16
Summary Statistics for Analyses of Treatment Pairs with a Different Number of Learner-Control Factors

	TREATMENT PAIRS			
	2 LC FACTORS: 1 LC FACTOR	3 LC FACTORS: 2 LC FACTORS	4 LC FACTORS: 2 LC FACTORS	5 LC FACTORS: 2 LC FACTORS
Subject N	546	682	797	347
No. of Effect Sizes	12	13	16	7
Average (d)	-.0012	-.0433	-.0346	-.4895
Variance(d)	.2187	.4417	.7149	.6465
Variance(e)	.0920	.0793	.0837	.0867
Ave(δ)	-.0012	-.0433	-.0346	-.4895
Variance (δ)	.1266	.3623	.6312	.5599
SD(δ)	.3558	.6020	.7945	.7482
Confidence Interval	-.70< δ <.70	-1.22< δ <1.14	-1.59< δ <1.52	-1.96< δ <.98

treatment pairs for these analyses provided the following number of learner-control factors: 2 and 1, 3 and 2, 4 and 2, 5 and 2. The results of these analyses are summarized in Table 16. Three of the analyses show results consistent with the previous analyses. Average effect sizes are negligible, confidence intervals are large and include zero. The fourth analysis has an effect size of $-.49$, but again the confidence interval is large and includes zero.

As in the analysis for H_1 , the confidence intervals produced by the H_2 analyses reflected a large variance, so the conclusion that learner control had no effect on achievement was deferred until the results of the moderator variable search were known.

What is the Effect on Achievement of Providing
Specific Learner-Control Factors?

The hypotheses associated with this research question were:

- H_3 Providing specific learner-control factors
 has an effect on achievement.
- $H_{(0)3}$ Providing specific learner-control factors
 does not have an effect on achievement.

For the third research sub-question, the contribution of each learner-control factor to achievement was individually examined. The third

research sub-question was, in fact, a series of questions: What is the impact of learner control of pace on achievement? What is the impact of learner interaction on achievement? What is the impact of learner-controlled review on achievement?, etc.

The data used for this analysis were the 94 effect sizes generated from the pool of 134 treatment groups used in previous analyses. In addition, a binary mask was generated to indicate on which control factors the treatment pairs differed. These data were then analyzed by performing meta-analyses on the subsets of studies which differed by only one of the seven learner-control factors. The results are shown in Table 17.

Table 17
Effect Size of δ and Standard Deviation of δ for Specific Learner-Control Factors

CONTROL FACTOR	δ	SD(δ)
Pace	-.1173	.7416
Interaction	.3732	.3683
Review	.2166	.4868
Number of Exercises	-.0764	.1704
Amount of Material	-.0320	.8748
Sequence	-.0140	.6312
Cognitive Format	.1180	.3921

Learner control of pace appeared to have very weak negative effects on achievement. Providing student interaction had a small positive effect on achievement as did providing learners with control over the review.

Providing learners with control over the number of exercises or the amount of instruction or the sequence of instruction had negligible effects. Very small positive effects were produced by providing learners with control over the cognitive format of instruction. Once again, the confidence intervals were large and included zero. It could not, therefore, be concluded that the effect of these control factors was non-zero and the interaction of moderator variables was indicated.

Interaction of Moderator Variables

The third research question examined by this dissertation was:

3. Do specific moderator variables interact with learner control to produce different achievement effects?

The associated sub-questions were:

3. a. Does student age or class in school interact with learner control to produce different effect sizes for student achievement?

3. b. Does the topic of instruction interact with learner control to produce different effect sizes for student achievement?

3. c. Does the type of computer equipment used interact with learner control to produce different effect sizes for student achievement?

3. d. Do the instructional media interact with learner control to produce different effect sizes for student achievement?

3. e. Are the effect sizes for student achievement under learner control in published studies significantly different from those reported in unpublished studies or from those reported in dissertations?

3. f. Do effect sizes for student achievement under learner control differ by experimental design?

3. g. Do effect sizes for student achievement under learner control differ by study date.

3. h. Do effect sizes for student achievement under learner control differ by researcher?

3. i. Does the length of exposure to learner-control treatments have an effect on achievement?

The analyses of moderator variables used the 94 effect sizes which were generated from the pool of 134 treatments under the specification that the number of control factors provided to learners in one treatment from a given study was greater than the number of

control factors provided in the other treatment from the study. These data are provided in Appendix A.

Does Student Age or Class in School Interact With
Learner Control to Produce Different
Effect Sizes for Student Achievement?

The hypotheses associated with this research question were:

H₄ Student class in school interacts with learner control to produce different effect sizes for student achievement.

H₍₀₎₄ Student class in school does not interact with learner control to produce different effect sizes for student achievement.

Table 18
Summary Statistics for δ by Class in School

VARIABLE	STUDY COUNT	SUBJECT COUNT	AVE(δ)	SD(δ)	95% CONF INT
ELEMENTARY	30	1438	.0814	.2993	-0.51 TO 0.67
HIGH SCH	14	517	-.4136	.9807	-2.33 TO 1.51
COLLEGE	50	2420	-.0348	.6899	-1.39 TO 1.32

ELEMENTARY				L-----*-----U
HIGH SCHOOL	L-----			*-----U
COLLEGE		L-----		*-----U

+-----+-----+-----+-----+-----+-----+-----+-----+-----+
-3 -2 -1 0 +1 +2

* = MEAN
L = Lower limit of 95% confidence interval
U = Upper limit of 95% confidence interval

Most researchers whose data were incorporated into the meta-analysis used intact classes as subject pools.

Consequently, class in school was reported, rather than age. Hence, data for the class-in-school variable were collected and coded for each study. The mean effect sizes for three class-in-school groups were examined by conducting meta-analyses on subsets of the sample studies. The subsets were elementary, high school, and college. Elementary was defined as grades 1 through 8. High school was defined as grades 9 through 12. College was defined as grades 13 and above. The results of the analyses are shown in Table 18.

From the clinical perspective, the mean effect size of learner control was negligible for elementary students (.0814) and at the college level (-.0348). For high school students it was slightly negative (-.4136). The negative impact of learner control on students in grades 9 through 12 may be meaningful. However, it can be seen from Table 18 that the confidence interval for learner control at the high school level encompassed a wide variation and included zero, indicating inconsistent interaction or possibly no interaction. Learner control did not appear to have significant clinical interaction with achievement at either the elementary or college level. The null hypothesis was not rejected.

Does the Topic of Instruction Interact With
Learner Control to Produce
Different Effect Sizes for Student Achievement?

The hypotheses associated with this research question were:

- H_5 The topic of instruction interacts with learner control to produce different effect sizes for student achievement.
- $H_{(0)5}$ The topic of instruction does not interact with learner control to produce different effect sizes for student achievement.

Information on the instructional topic of the courseware in each study was collected and coded for the meta-analysis. The topic categories were science, math, sociology, language arts, art, political science, education, and psychology.

As shown in Table 19, the mean effect sizes for sociology (.6285) and language arts (.6468) were positive, indicating that providing learner control in these subject areas was likely to have a positive effect on student achievement. Negligible mean effect sizes were found for science (-.1257), math (-.1141), art (-.1543), and political science (-.0219). Negative effect sizes were indicated for education (-.2420), and psychology (-.6104).

Table 19
Summary Statistics for δ by Instructional Topic

VARIABLE	STUDY COUNT	SUBJECT COUNT	AVE(δ)	SD(δ)	95% CONF INT
SCIENCE	35	1919	-.1257	.7814	-1.66 TO 1.40
MATH	15	761	-.1141	.4236	-0.94 TO 0.72
SOCIOLOGY	8	423	.6285	.2516	0.14 TO 1.12
LANG ARTS	6	159	.6468	1.0633	-1.43 TO 2.73
ART	5	250	-.1543	0.0000	-0.15 TO -0.15
POLY SCI	13	520	-.0219	0.0000	-0.02 TO -0.02
EDUCATION	4	102	-.2420	0.0000	-0.24 TO -0.24
PSYCHOLOGY	8	241	-.6104	.3919	-1.38 TO 0.16

* = MEAN
L = Lower limit of 95% confidence interval
U = Upper limit of 95% confidence interval

Clinically, the effect sizes for sociology, language arts, education, and psychology could indicate real interaction effects. However, the confidence intervals for language arts and psychology included zero, leaving sociology and education as the topics of instruction which appeared to have consistent clinical impact on student achievement. Consequently, the null hypothesis was not accepted.

Does the Type of Computer Equipment Used

Interact With Learner Control to

Produce Different Effect Sizes for Student Achievement?

The hypotheses for this research questions were:

H_6 The type of computer equipment used interacts with learner control.

$H_{(0)6}$ The type of computer equipment used does not interact with learner control.

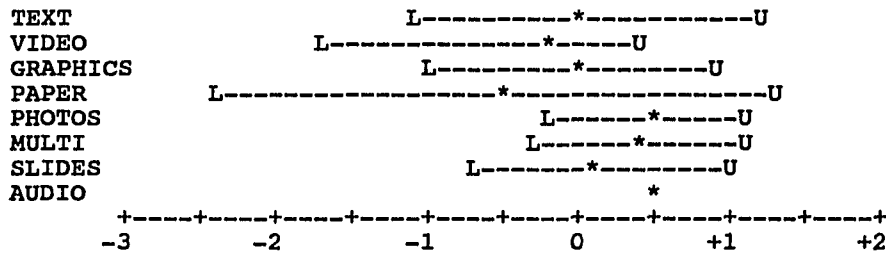
Four equipment platforms were coded from the original pool of studies: Apple II, IBM personal computer (including compatibles), Macintosh, and mainframe (including CRT, VDT, and TTY terminals). An additional category, unknown, was used for those studies which did not report the hardware used.

From the clinical perspective, the mean effect size was negligible (-.0062) for studies which used the Apple II and the confidence interval included zero, indicating no interaction effect. The mean effect size for studies which used the Macintosh was also close to zero (-.0219), indicating no clinically useful interaction. Slightly negative mean effect sizes were obtained for studies which used the IBM personal computer (-.2428) and the mainframe (-.3533). The mean effect size for the group which used unknown equipment was slightly positive (.2178). From Table 20 it can be seen that the confidence intervals for all

based text only (TEXT), (b) computer-based text plus graphics (GRAPHICS), (c) computer-based text plus audio (AUDIO), (d) computer-based text plus photocopied or other paper-based illustrations (PHOTO), (e) computer-based text plus slides (SLIDES), (f) computer-based text plus interactive video (VIDEO), (g) computer-based text plus paper-based printed materials (PAPER), and (h) computer-based text and multiple additional media (MULTI).

Table 21
Summary Statistics for δ by Instructional Media

VARIABLE	STUDY COUNT	SUBJECT COUNT	AVE(δ)	SD(δ)	95% CONF INT
TEXT	32	1556	.0377	.5789	-1.10 TO 1.19
VIDEO	11	444	-.1721	.2942	-0.75 TO 0.41
GRAPHICS	20	887	-.0180	.4803	-0.96 TO 0.92
PAPER	17	762	-.5452	.9386	-2.39 TO 1.29
PHOTOS	8	369	.4570	.3356	-0.20 TO 1.12
MULTI	2	151	.3732	.3684	-0.35 TO 1.09
SLIDES	2	166	.1308	.4179	-0.69 TO 0.95
AUDIO	2	40	.5334	.0000	0.53 TO 0.53



* = MEAN
L = Lower limit of 95% confidence interval
U = Upper limit of 95% confidence interval

From the clinical perspective, the group using paper-based printed materials in addition to the computer-based instruction had a moderately negative mean effect size (-.5452). Mean effect sizes were negligible for the groups using video (-.1721), computer graphics (-.0180), computer-based text only (.0377), or slides (.1308). Slightly positive to moderately positive mean effect sizes were obtained for the groups using multi-media (.3732), photo-copy illustrations (.4570), or audio cassette (.5334). As Table 21 shows, the confidence intervals for all the media variables except AUDIO included zero, indicating the possibility of no meaningful clinical interaction. The AUDIO data should be viewed with caution as they reflect only two treatments from a single study with a sample size of 40. The null hypothesis was not rejected.

Are the Effect Sizes for Student Achievement Under
Learner Control in Published Studies
Significantly Different from those Reported in
Unpublished Studies or from those
Reported in Dissertations?

The hypotheses for this research question were:

H₀ Effect sizes from published studies on
 learner control differ from those reported in

unpublished studies or from those reported in dissertations.

$H_{(0)\delta}$ Effect sizes from published studies on learner control do not differ from those reported in unpublished studies or from those reported in dissertations.

Table 22
Summary Statistics for δ by Publication Type

VARIABLE	STUDY COUNT	SUBJECT COUNT	AVE(δ)	SD(δ)	95% CONF INT
JOURNAL	58	2856	-.0207	.7769	-1.54 TO 1.50
DISSERTATION	34	1353	-.1063	.2817	-0.66 TO 0.44
UNPUBLISHED	2	166	.1308	.4175	-0.69 TO 0.95

* = MEAN

L = Lower limit of 95% confidence interval

U = Upper limit of 95% confidence interval

Three forms of publication were identified from the original pool of studies: (a) studies published in journals (JOURNAL), (b) studies reported in dissertations (DISSERTATION), and (c) studies which appeared as unpublished ERIC documents (UNPUBLISHED).

Published studies had a negligible mean effect size (-.0207). Dissertations had a slightly negative mean effect size for achievement under learner control

(-.1063). Unpublished studies had a slightly positive mean effect size (.1308). It should be noted that the unpublished group contained only two studies, both performed by the same researchers. As can be seen from Table 22, confidence intervals for all publication types included zero. Under the circumstances, the null hypothesis was not rejected.

Do Effect Sizes for Student Achievement Under Learner Control Differ by Experimental Design?

The hypotheses for this research question were:

- H_1 Effect sizes for student achievement under learner control differ by experimental design type.
- $H_{(0)1}$ Effect sizes for student achievement under learner control do not differ by experimental design type.

The studies from the original pool used either the Pretest-Posttest Control Group Design (Design 4) or the Posttest-Only Control Group Design (Design 6). Studies using Design 6 were more numerous and included 3,921 of the 4,375 subjects in the data pool. The results did not appear to be clinically meaningful. As indicated in Table 23, the mean effect size for Design 4 was .0765. The mean effect size for Design 6 was -.0550. Confidence intervals for both design types included zero. The null hypothesis was not rejected.

Table 23
Summary Statistics for δ by Experimental Design

VARIABLE	STUDY COUNT	SUBJECT COUNT	AVE(δ)	SD(δ)	95% CONF INT
DESIGN 4	5	454	.0765	.2204	-0.35 TO 0.51
DESIGN 6	89	3921	-.0550	.6856	-1.40 TO 1.29

* = MEAN
 L = Lower limit of 95% confidence interval
 U = Upper limit of 95% confidence interval

Do Effect Sizes for Student Achievement Under
 Learner Control Differ by Study Date?

The hypotheses for this research question were:

- H_{10} Effect sizes for student achievement under learner control differ by study date.
- $H_{(0)10}$ Effect sizes for student achievement under learner control do not differ by study date.

Studies were divided into three subsets to examine the study date variable. Studies in the first subset were published prior to 1982. Studies in the second subset were published between 1982 and 1987. Studies in the third subset were published between 1988 and 1990. The mean effect size for studies published prior to 1982 was $-.2657$, a small negative effect. The mean effect size of studies in the second subset was negligible as was the mean effect size for studies

Table 24
Summary Statistics for δ by Study Date

VARIABLE	STUDY SUBJECT		AVE(δ)	SD(δ)	95% CONF INT
	COUNT	COUNT			
PRIOR TO 1982	1275	26	-.2657	.8839	-2.00 TO 1.47
1982 - 1987	812	21	.0143	.7294	-1.42 TO 1.44
1988 - 1990	2288	47	.0638	.3919	- .70 TO .83

* = MEAN
L = Lower limit of 95% confidence interval
U = Upper limit of 95% confidence interval

published between 1988 and 1990. Table 24 shows the effect size means and confidence intervals. All confidence intervals included zero. The null hypothesis was not rejected.

Do Effect Sizes for Student Achievement Under
Learner Control Differ by Researcher?

The hypotheses for this research question were:

H_{11} Effect sizes for student achievement under learner control differ by researcher.

$H_{(0)11}$ Effect sizes for student achievement under learner control do not differ by researcher.

As indicated earlier in this chapter, certain researchers were associated with more than one learner-control study. Three of particular interest are Tennyson, Carrier, and Sullivan. Tennyson was author or co-author of 7 studies. Carrier was author or co-

author of three studies and her courseware was used in one additional study. Sullivan was the co-author of three studies.

Table 25
Summary Statistics for δ by Selected Researchers

VARIABLE	STUDY COUNT	SUBJECT COUNT	AVE(δ)	SD(δ)	95% CONF INT
CARRIER	7	343	.5354	.2142	0.12 TO 0.96
TENNYSON	17	762	-.5452	.9386	-2.39 TO 1.29
SULLIVAN	5	413	-.2600	.0000	-0.26 TO -0.26

* = MEAN
L = Lower limit of 95% confidence interval
U = Upper limit of 95% confidence interval

As seen from Table 25, the mean effect size for the Carrier studies was moderately positive (.5354). From the confidence interval it can be seen that learner control always produced positive results in studies designed by this researcher. However, the large variance of the confidence interval for the Carrier studies indicates that the effects of this moderator may be inconsistent. The mean effect size for the Tennyson studies was moderately negative (-.5452). The confidence interval for this researcher was large and included zero. Thus, Tennyson's research

appeared to produce mixed results. The mean effect size for the Sullivan studies was slightly negative (-.2600) and there was no variance in the effect size.

It is difficult to draw strong conclusions about the relationship between researchers and effect sizes because it is not known if the studies which have been published are a representative sample of that researcher's work. However, since there appears to be some interaction effect by researcher, the null hypothesis was not accepted.

Does the
Length of Exposure to Learner-Control Treatments
have an Effect on Achievement?

The hypotheses associated with this research questions were:

- H_{12} The length of treatment under learner control has an effect on student achievement.
- $H_{(0)12}$ The length of treatment under learner control does not have an effect on student achievement.

Length of treatment was calculated by multiplying the number of instructional sessions by the duration in minutes of each session. The studies were divided into three subsets based on the length of treatment. Subsets were defined as 0 to 60 minutes, 61 to 300 minutes, and 301 minutes or longer.

Table 26
Summary Statistics for δ by Length of Treatment

VARIABLE	STUDY COUNT	SUBJECT COUNT	AVE(δ)	SD(δ)	95% CONF INT
1-60 MINUTES	2331	46	-.2010	.7495	-1.67 TO 1.27
61-300 MINUTES	1299	32	.0716	.4847	-.88 TO 1.02
>300 MINUTES	217	4	.0820	.5002	-.90 TO 1.06

1-60 MINUTES	L-----*-----U				
61-300 MINUTES	L-----*-----U				
>300 MINUTES	L-----*-----U				
	+-----+-----+-----+-----+				
	-2	-1	0	+1	+2

* = MEAN
L = Lower limit of 95% confidence interval
U = Upper limit of 95% confidence interval

Table 26 shows summary statistics for the treatment length subsets. The mean effect sizes for all subsets were negligible, the confidence intervals were large and included zero. Therefore the null hypothesis was not rejected.

Summary

Descriptive statistics were used to characterize the body of learner-control research and the pool of studies used for this meta-analysis.

Meta-analyses were performed to make inferences about the effect of providing learner control. The bare bones meta-analyses of learner control and achievement produced negligible effect sizes. The confidence intervals for these analyses included zero,

but due to the large variance relative to the mean effect size, the conclusion that learner control had no effect on student achievement was deferred until the results of the moderator variable search were known.

The effects of nine potential moderator variables were examined. Two potential moderators appeared to be topic of instruction and researcher. When the topic of instruction was sociology, effect sizes were positive. When the software associated with a study was designed by Carrier, effect sizes were positive. It should be noted that Carrier's courseware is exclusively on the topic of sociology. Gray was the only other researcher to use sociology as the topic of instruction. Sullivan had very consistent interaction in a slightly negative direction. The nature of these moderators suggests that a possible interaction results from the quality of the software design. Such a moderator could not be directly measured with the available data, nor is it likely to account for a large portion of the variation in effect sizes. Other potential moderators did not produce consistent effects.

Returning, then, to hypotheses H_1 , H_2 , and H_3 which were not resolved by the moderator variable search, it appeared that providing learner control had a negligible net effect on achievement. The large variation of effect sizes could be only partially

accounted for by the variables defined as possible moderators by this dissertation.

A discussion of these results and recommendations for further research are offered in Chapter 5.

CHAPTER 5

DISCUSSION, IMPLICATIONS AND RECOMMENDATIONS

Learner control of computer-based learning environments has been proposed as a potentially effective method for individualizing the learning process, increasing learning effectiveness, and enhancing learning efficiency. However, all learner-control studies have not confirmed that learner control produces such effects. Instead, the collection of learner-control studies has been characterized by reviewers as offering findings which are inconsistent, ambiguous, contradictory, and inconclusive.

For the courseware designer or for the practitioner selecting appropriate courseware for industrial, military, or educational settings, the findings of learner-control studies do not appear to provide a clear foundation for action. Herein lies the problem addressed by this dissertation and the following related questions: What is an accurate summary of learner-control research to date? Is the research inconsistent, contradictory, ambiguous, and inconclusive, or are the apparently divergent findings, in fact, a product of sampling error or other research artifacts? Are there really no findings which can be translated into practice, or are there answers which

have been obscured by the volume of studies and methodological diversity?

The scope defined for this dissertation limited inquiry to the effect of learner control on student achievement as represented by posttest scores. Three specific research questions were delineated:

1. What are the characteristics of the body of learner-control research which has examined the effect on achievement of learner control?

2. Is there a difference in the achievement of students who are provided with learner control and students who are provided with other control models?

3. Do specific moderator variables interact with learner control to produce different achievement effects?

Descriptive statistics were used to identify the general characteristics of 41 empirical studies of learner control. Next, a quantitative technique known as meta-analysis was used to integrate the findings of the studies and make inferences about the effect of learner control on student achievement. Finally, a number of variables, identified as potential moderators, were examined for interaction effects. The results of these procedures are summarized and discussed in this chapter along with recommendations for application and further research.

The Effect of Learner Control

The average effect of providing more learner control to students using computer-based courseware was to decrease achievement scores by .04 standard deviation, an amount generally considered negligible within the educational domain. Only 17% of the observed variance in the sample of studies could be attributed to sampling error. The remaining variance was reflected in the confidence interval for δ which ranged from -1.32 to 1.24 standard deviations. Such a range of effects is not uncommon in educational research (Cook et al., 1986; Kulik et al., 1980; Kulik et al., 1983; McNeil & Nelson, 1991) and it is not unusual for the confidence interval of effects to include zero. Generally, such a situation launches a search for moderator variables.

Moderator Variables

The search for the source of variation for δ encompassed a number of courseware, student, and study variables including class in school, instructional topic, computer equipment, instructional media, publication type, experimental design, study date, researcher, and length of treatment. Potentially significant interactions were found for instructional topic and researcher. Other potential moderators did

not appear to produce significantly different achievement for learner control.

A tentative synthesis of findings might suggest that certain researchers were able to make resourceful use of available hardware features to produce courseware packages which were effective learner-control environments for a particular group of students studying a particular topic. The issues of software quality and student characteristics will be discussed in subsequent sections of this chapter.

The Meaning of Negligible Effect

It must be stressed that the negligible effect size of providing learner control did not indicate that students under learner control made no achievement gains. The nature of the meta-analytic process produced an effect size which was a comparison of treatments featuring more learner control with treatments featuring less learner control. Hence, students may have made achievement gains under all control options. The negligible effect of learner control is an indication that achievement under learner control was not different from achievement under other control models.

Earlier meta-analyses (Kulik et al., 1980; Kulik et al., 1983) have shown that computer-based instruction produced achievement effects of

approximately .3 standard deviation when compared to traditional instructional methods. If this is the case, it is likely that students provided with learner control will post achievement scores slightly higher than with traditional instruction, but at the same level as with other modes of computer-based instruction.

Learner Control Compared to Other Control Options

Why did providing learner control over more factors not have a significant impact on achievement? Several theories might be advanced. First, providing learner control may not always mean that the students exercised their ability for control. Under some software designs, students who were purportedly under a learner-control format may have been, by using the system default settings, progressing no differently than a student under computer control. Measuring differences in the effect of learner control under such circumstances may have produced unreliable results. Some researchers (Carrier & Williams, 1988; Carrier et al., 1985; Kinzie & Sullivan, 1989; Pollock & Sullivan, 1990) have attempted to quantify the amount of control a learner exercises during the lesson and have considered the impact of the amount of control exercised, rather than the amount of control provided. Further research in this direction could be important.

Second, the amount of learner control provided in the treatments may not have represented particularly diverse options. Research question 2.b. examined specific configurations of learner control. The analysis was abbreviated because a number of control configurations have not been researched. In the pool of primary studies used for the meta-analysis, there were none in which learner control of one factor was compared with learner control of six or seven factors. Similarly, there were no studies comparing learner control of two factors with learner control of six factors. In only two studies, learner control of two factors was compared with learner control of seven factors. It would seem that some of the more diverse control models were not examined by the learner-control studies.

The typical study compared the achievement of students given control of four factors with the achievement of students given control of two factors. The likely configuration of the four-factor control model would be to provide learner control of pace, interaction, review, and number of exercises. The likely configuration of the two-factor control model would be to provide learner control of only pace and interaction. Within the possible spectrum of control options, the distinction between control of two factors

and control of four factors may not be expected to yield considerable differences. Hence, many of the studies compared control models which were not as diverse as might be expected, and this was likely to have been reflected in the meta-analysis as an average effect size of zero, representing no difference in achievement between learner control and computer control. Consequently, it is recommended to exercise caution when applying the results of the analysis to computer-based learning environments which utilize control models which diverge from the typical treatments found in the pool of primary studies.

Third, there may be significant differences between control models which were not accounted for by the variables defined for this meta-analysis. The large confidence interval for δ suggests that this could be the case. For example, computer control is not necessarily a single construct. One model of computer control is characterized by linear format and fixed tasks. Under this model, all students receive the same instruction, regardless of aptitude, understanding, or performance. An alternative to the fixed model is the adaptive model in which the computer, following an instructional algorithm, adapts the instruction based on student performance. Under this model, the instruction is individualized.

Within the studies used for this meta-analysis, some of the computer control treatments were managed by adaptive instructional algorithms while others were managed under the linear model. To add complexity to the control construct, in most studies only certain instructional factors were under adaptive control while other factors were fixed. However, when the computer controlled a factor of instruction, whether by a linear or adaptive program, the factor was defined as computer controlled for the meta-analysis.

In some studies, learners were given control, but they were given advice about the decisions they were allowed to make. This advice was likely to have been the result of an adaptive algorithm. Because the learner had the option to follow or disregard the computer's advice, these factors were defined as learner controlled for the meta-analysis.

The possibility exists that the various computer-control models interacted with achievement to produce some of the observed variance in effect sizes among studies. As a hypothetical example, the studies in which learner-control provisions produced poor achievement may have employed treatments under adaptive algorithms for computer control which were then compared with treatments which employed learner control without advisement. Studies in which learner-control

provisions appeared to produce superior achievement may have employed fixed computer-control treatments which were compared with treatments which employed learner control with advisement.

Determining the effect of control models such as learner control without advisement, learner control with advisement, fixed computer control, and adaptive computer control was not within the scope of this dissertation. Instead, the emphasis was on determining the effect of learner control of specific courseware factors such as pacing, sequencing, and format. This was perceived as a first step in reconciling some of the ambiguity in the research definition of learner control. The findings of this analysis indicated that the effect of learner control did not differ from the effect of other control models taken in toto. A finer-grained analysis of the alternative control models has potential value. An initial attempt was made to disaggregate the data for this analysis using the categories learner control with advisement, learner control without advisement, fixed computer control, and adaptive computer control as possible values for each of the seven control factors. The process did not produce sufficient data per category which could be used as the basis for meaningful statistical analysis. This has been defined as an area for further research.

The Typical Learner-Control Study: A Profile

The descriptive statistics reported in the previous chapter may be used to formulate the profile of a typical or average learner-control study. This typical study might be described as a posttest-only control group design with a sample size of 95 in which college students were exposed to one treatment session lasting a little less than an hour. This typical study would compare the achievement of students provided with control over four instructional factors with the achievement of students provided with control over two instructional factors. The Apple II would be the hardware platform. The instruction would be delivered using screen-based text, unadorned with graphics or supplemental media. Science would likely be the instructional topic.

Since the findings of this meta-analysis are based on the pool of studies which produced the profile described above, the thoughtful practitioner might reflect upon these results and formulate several questions before attempting to apply the findings. In the next section these questions will be examined relative to the findings from the meta-analysis.

Student Age and Achievement Under Learner Control

Since the typical learner-control study was performed using a subject pool of college students, can

the results of these studies be generalized to other populations of learners? The students' class in school was examined as a potential moderator variable. On the average, providing learners with more control over the learning environment appeared to produce negligible effects on achievement for elementary and college students. However, the achievement of high school students appeared to be somewhat negatively affected under learner-control treatments.

These findings imply that there may not be, as Hannafin (1984) suggested, a linear relationship between age and successful achievement under the learner-control paradigm. The large spread of effect sizes within each of the class-in-school groups makes assumptions on this point tentative, though it certainly presents a topic for further study. It should be noted that empirical testing of learner control across class-in-school groups has the potential to present some troublesome experimental control problems. For example, it might be difficult to design a single computer-based lesson which could be effectively presented to learners at all grade levels.

Additional Student Variables

The students' class-in-school was the only student variable examined as a potential moderator. How do the

results of the analysis apply to special student groups?

Some of the primary studies used for the meta-analysis investigated the effects of different control models on student groups with specific characteristics. For example, Lopez and Harper (1990) studied Hispanic students, Goetzfried and Hannafin (1985) studied low achievers. Carrier et al. (1984) and Burwell (1989) studied field independent and field dependent learners. Lee and Wong (1989) reported some results by gender. The meta-analysis did not produce findings on the effect of providing learner control to these student groups. There are two reasons this was the case. First, a meta-analysis is designed to measure between-study differences. If these student variables were examined by learner-control researchers, they were most often examined within studies. For example, most of the studies used treatment subject pools of mixed gender. As this was the case, gender scores might be examined within a particular study by correlating gender with score. For the meta-analysis to detect gender differences, achievement scores and standard deviations by gender for learner control and computer control would need to be available. Researchers did not generally provide detailed data on these variables.

Second, the number of studies which focused on

specific student characteristics such as ethnic group, aptitude, or cognitive style were too infrequent to provide results of statistical significance. Other meta-analysts (McNeil & Nelson, 1991) have recognized the difficulty of producing meaningful synthesis when the research base provided data from a small number of isolated studies involving a particular learner characteristic.

Since the meta-analysis did not provide information about student variables other than class in school, it is possible that variations in the individual student's ability to operate under the learner-control model may yet provide an explanation for the variation in effect sizes.

Length of Treatment and Achievement

Under Learner Control

Most learner-control studies examined the achievement of students exposed to only one session of less than an hour. Is this a fair trial for learner control? Holmes et al. (1985) suggested that it may take learners some time to adjust to the mechanics of the learner control environment and additional time to develop the requisite meta-cognitive skills for effective learning under this unfamiliar paradigm. The question of the validity of single-session treatments follows logically.

The meta-analysis examined the length of treatment as a potential moderator variable. There did not appear to be an interaction effect between length of treatment and achievement under learner control. It might be noted, however, that only 8 of the 41 studies provided more than two sessions on the computer-based lesson. Perhaps there is a need for further study of the long term effects of learning under the learner-control mode.

The Spectrum of Learner Control

The typical experiment compared learner control of four factors with learner control of two factors. Is the difference between these control models substantial enough to provide a basis for generalization of results? Does this represent the potential spectrum of learner control?

It was possible, under the coding scheme for the meta-analysis, for learners to be provided with control of as many as seven factors in the learning environment. However, only 7 treatments from the pool of 134 provided learners with control of six or more factors. Only 3 treatments provided learners with control over all seven of the factors. It would seem that the control environments which were used in the pool of primary studies were not maximizing the control

provided to learners; on the contrary, fairly minimal control was provided.

Consider learner control over four factors. These factors might be control of pace, the ability to interact with the software, control over the review process, and control over the number of exercises attempted. Many educators would not view this as placing the bulk of instructional decision-making under the purview of the learner. For example, Mayer and McCann (1961) originally used the term learner control to refer to the opportunity for students to sequence the objectives within a particular course. In the pool of 134 treatments used for the meta-analysis, only 29 treatments provided the students with control over sequencing. In most of these studies, sequence control was allowed within a single lesson, rather than throughout an entire course of instruction.

The implementation of learner control in the pool of treatments examined by the meta-analysis was further limited in that most of the experimental sessions occurred at a time and place not under the control of the learner. Students also did not, in most situations, have control over the topic of instruction. The time, place, and topic parameters were generally pre-determined by the researchers. The models of learner control which were implemented by the pool of

primary studies, and subsequently subjected to meta-analysis may be considered a somewhat limited representation of the potential spectrum of learner control, but may be a reflection of the current teacher-centered educational paradigm (Mukherjee, 1991).

The opportunities for learner control are likely to increase as new hardware capabilities and new software standards impact educational computing. In a 1988 textbook on the design, development, and evaluation of instructional software, Hannafin and Peck remarked that the overwhelming majority of CAI lessons can be classified as tutorials, drill and practice, simulations, or instructional games (p. 139). However, they pointed out that other models exist and it is likely that most CAI models will evolve as computer architecture and software engineering increase in sophistication (pp. 367-378). It is possible to speculate about the role of the learner in controlling the events of instruction under these evolving models and imagine that access to large computer databases, telecommunications, hypermedia, artificial intelligence, and collaborative software will require increased participation and control on the part of the user.

Additional control issues arise when learner control is considered outside of the realm of the traditional institutional setting. Distance education, for example, may require learners to take control over additional aspects of the learning environment such as motivation and scheduling. On this basis, one might conclude that learner-control studies to date have examined only a very limited aspect of the learner-control question and hope that in the future researchers will make an effort to probe the effects of expanded learner-control options.

Computer Equipment

Most of the studies were carried out on Apple II computers. This is old technology. Do the results of learner-control studies using these computers apply to more sophisticated hardware platforms? The meta-analysis examined the effect of computer equipment on achievement under different control models. On the average, it appeared that using Apple II computers did not increase or decrease achievement under learner control. Results for using Macintosh computers were similar.

Average achievement effect sizes for groups using the IBM personal computers and mainframe terminals were negative, indicating that providing students with more learner control was associated with poorer achievement.

These findings are not conclusive since there was a large spread of effect sizes within each equipment group and the confidence intervals included zero.

It would be tempting to attribute lower levels of achievement under learner control on these hardware platforms to the folklore decrying the lack of user-friendliness resulting from the command-line format of DOS and mainframe computer interfaces. One might formulate the hypothesis that command-line interfaces would be difficult to operate, particularly for the learner-control students, who presumably had to interact with the system more frequently to make choices about number of exercises, sequence, amount of material, and so forth. However, the Apple II also operates under a command-driven environment and the average effect size for this hardware platform showed that essentially no difference in achievement resulted from providing more learner control. Consequently, it would not seem correct to attribute the poorer achievement of learner-control students using IBM personal computers and mainframe platforms to generalized deficiencies in the user interface.

Past differences which resulted from dissimilar hardware platforms appear to be diminishing as software philosophy becomes more homogeneous. Further data related to this issue will be discussed in the next

section on the quality of learner-control courseware and its impact on achievement.

An additional facet of the question concerning old technology is that the results of the meta-analysis indicated that study date was not a moderator variable which interacted with learner control. It appeared that the availability of more powerful technology for more recent studies did not impact student achievement under the different control models. Perhaps, the increased processing, storage, and graphics capabilities were not incorporated in the courseware used by recent learner-control researchers. Perhaps, the CAI models which were used for the older studies were not significantly changed for the newer studies.

Computer Software and Achievement

Can the design and quality of the courseware account for achievement differences? Roblyer (1985) suggested that learning effects vary depending on the quality of the courseware. The reported information from the pool of primary studies used for this meta-analysis was not detailed about many aspects of the software design which might contribute to its quality. Though some general characteristics of the courseware were coded to indicate the use of text, graphics, and supplementary media; in most cases, researchers did not provide the detail necessary to code qualitative

aspects relating to the user interface, dialogue structure, navigational structure, or response time.

Software quality would seem to be an important aspect of the research, particularly in experiments with only a single treatment session. The amount of time it takes a learner to master the mechanics of the software could have an impact on the results of the learning experience (Holmes et al., 1985).

In the learner control studies used for the meta-analysis, two versions of the courseware were necessary. One version provided computer control. The other version provided some degree of learner control. Lacking evidence to the contrary, may we assume that the learner-control version was simply a modification of the original computer-control version of the courseware? Can we then assume that this authoring process resulted in software that was equivalently useable under the specified control paradigm? We are asking, then, if the learner-control courseware was as easy to use as the computer-control courseware despite the fact that the learner-control courseware was likely to have more user options and require user navigation from one section of the program to another. Navigation presents concerns in a variety of interface models including menu-driven and graphics systems (Rubin, 1988, pp. 70-73).

Did the students under learner control understand the options they were given and understand how to use those options? Research indicates that this may be a concern. Schloss et al. (1988) and Carrier and Williams (1988) examined how students used learner-control options. Their findings showed that some students did not use the control options provided. However, it was not clear whether this was due to operational difficulties or motivational deficiencies.

Additional findings from the meta-analysis point to some possibility of intervention based on software quality. First, sociology was found to be an *instructional topic* that produced consistently positive achievement effects. Gray (1987) suggested that instructional topics which de-emphasize drill and are not based on linear progressions might be more suited to learner control. She specifically mentioned sociology as an instructional topic which might be effectively adapted to learner-control courseware. Other instructional topics examined in the search for moderator variables would not appear to differ in key pedagogical elements from sociology. So, perhaps it was not sociology per se which was associated with positive effects. Perhaps there was some specific element in the design of the sociology software which made it particularly effective.

Second, an examination of experiments performed by specific researchers revealed significant differences between the mean indicators for achievement. For example, in studies using software designed by Carrier, learner control was always associated with positive achievement effects whereas in studies performed by Sullivan, learner control was always associated with negative achievement effects. The possibility that these effects may be due to the specific design of the software cannot be discounted, though it was not possible to directly test this assumption within the scope of this dissertation. Perhaps, in the future, researchers will make a greater effort to describe the qualitative aspects of the courseware used in their experiments.

Application of Results

Providing students with control over more factors of the learning environment did not appear to have an effect on achievement. Is it appropriate, then, to abandon the learner-control model in favor of a computer-controlled model which might appear simpler and more cost effective to implement?

The computer-controlled models in the research pool encompassed a variety of designs. All were not based on simple linear-control models. Some of the computer-controlled models were adaptive or had a

subset of program features under adaptive control. Adaptive models require branching nodes and the use of instructional algorithms to model or evaluate student progress and are consequently rather complex (Park & Tennyson, 1983). Hence, it is not correct to assume that learner control would require more programming than a computer-controlled model.

Instead of abandoning the learner-control model, it is suggested that the results of this analysis may empower educators to explore learner-controlled environments as a pedagogically sound format for computer-based learning environments. Though the findings of the meta-analysis do not directly dispute claims that students under learner control terminate instruction prematurely (Carrier et al., 1985; Ross & Rakow, 1982; Tennyson, 1980) and do not make effective learning decisions (Hannafin, 1984; Steinberg, 1977), the net result of providing more learner control was a negligible difference in achievement.

The results of this analysis provide some justification for the design and use of learner-controlled exploratory learning experiences such as those using HyperCard stacks, scholar workstations with telecommunications access to content databases, interactive texts, and collaborative multimedia courseware. Dede (1987) described three types of

evolving computer-based learning environments: empowering environments, hypermedia, and microworlds. Each of these environments was described as emphasizing student control, interaction, and active learning. Simonson and Thompson (1990) have suggested that these themes -- learner control, student involvement, and active learning -- will be important to computer applications of the future. Educators who are seeking to use computers not just to automate current pedagogical practices, but who are devising ways to use computers to restructure the teaching-learning process, may find the results of the meta-analysis liberating.

As with any software development project, the design of computer-based learning environments which provide learner-control should follow systematic procedures which include an initial needs assessment, formative, and summative evaluation. The reader is referred to Jonassen's (1988) compilation of material on microcomputer courseware design.

As new models of courseware are devised, it will be important to evaluate those models. Current modes of content testing may prove to be inadequate. It could become increasingly difficult to compare students who have received instruction under a fixed-content model with students who have used a more exploratory model. Students under the fixed-content model can be

easily tested on their understanding of the content covered by the instruction. Students under the exploratory model may be covering a unique configuration of material and it is likely that the computer which provided the material for the exploratory environment will be called into service to track the student's path through the material, then generate customized tests. Teach-back methods, protocol analysis, and the use of learner-generated test items may also play a role in evaluation efforts for learner-control models.

Conclusion

The problem which inspired this dissertation was the difficulty of summarizing and applying the findings of learner-control research -- a body of research which has been described as presenting "a montage of inconsistencies, contradictions and caveats" (Carrier and Williams, 1988, p. 286). The underlying objective was to determine whether the research on learner control could be correctly characterized as inconsistent, contradictory, ambiguous, or inconclusive.

For the learner-control research to be accurately described as *inconsistent*, it is implied that some studies would have been replicated, but results from the replications were different from those of the

original studies. Based on the study characteristics coded for this meta-analysis, none of the studies in the sample could be considered replications of others. From this perspective, inconsistent is not a characteristic of the learner-control research.

Learner control had an average effect size which was essentially zero, but the effect sizes calculated for treatment pairs exhibited some variation. Clearly, some studies reported positive achievement results under learner control whereas other studies reported negative results. These results could appear to be *contradictory* within the setting of casual literature review.

It is probably more accurate to refer to this phenomenon as the variability in learner-control effects. The meta-analysis was not able to account for all the variability in the sample of primary studies. As discussed earlier, this variability is not unusual in the educational domain and may be the result of interactions between courseware and student variables which are too complex to be statistically discernable.

The *ambiguity* of learner-control research has less to do with the findings of the research than with the concept of learner control itself. As pointed out in Chapter 2, what one researcher defined as learner control may have been defined as computer control by

another researcher. The seven-factor learner-control model used by this dissertation was an attempt to apply some standardization to the spectrum of strategies encompassed by the term learner control.

Can the learner-control research be characterized as *inconclusive*? Researchers have been struggling to demonstrate the superiority of either learner control or computer control. The meta-analysis indicated that these two control models produce essentially the same achievement results. The conclusion which can be drawn is that there is not a single control model which provides the best learning environment for all students, in all subjects. Instead, both computer control and learner control may provide acceptable choices.

Based on the results of the meta-analysis, learner control may be considered to be one of several viable models for courseware design for the general student population under the current class-based, teacher-controlled educational paradigm. Further, it may be worthwhile to pursue developmental efforts on individualized, learner-centered, and exploratory learning projects which might incorporate learner control, though it is recommended that such development efforts follow standard practices of design and evaluation.

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APPENDIX A

Study Data

The Original Pool of Studies

(Note: The Ellerman* and the Tennyson** citations each reported the results of two independent studies, make the total study sample size = 41.)

AVNER ET AL 1980

Avner, R. A., Moore, C., & Smith, S. (1980). Active external control: A basis for superiority of CBI. Journal of Computer-Based Instruction, 6(4), 115-118.

AXTEL 1978

Axtel, R. (1978). Learner-controlled instructional strategy options: Effects for generality help and instance help on a procedure-using task. Brigham Young University, Provo, UT.

BELLAND ET AL 1985

Belland, J. C., Taylor, W. D., Canelos, J., Dwyer, F., & Baker, P. (1985). Is the self-paced instructional program, via microcomputer-based instruction, the most effective method of addressing individual learning differences? Educational Communication and Training Journal, 33(3), 185-198.

BURWELL 1989

Burwell, L. B. (1989). The interaction of learning styles with learner control treatments in an interactive videodisc lesson on astronomy. Dissertation Abstracts International, 50(11), 3458A. (University Microfilms No. 9005810)

CAMPANIZZI 1978

Campanizzi, J. A. (1978). Events of locus of control and provision of overviews in a computer-assisted instruction sequence. AEDS Journal, 12(1), 21-30.

CARRIER & WILLIAMS 1988

Carrier, C. A., & Williams, M. D. (1988). A test of one learner-controlled strategy with students of differing levels of task persistence. American Educational Research Journal, 25(2), 285-306.

CARRIER ET AL 1984

Carrier, C., Davidson, G., Higson, V., & Williams, M. (1984). Selection of options by field independent and dependent children in a computer-based concept lesson. Journal of Computer-Based Instruction, 11, 49-54.

CARRIER ET AL 1985

Carrier, C. A., Davidson, G. V., & Williams, M. D. (1985). The selection of instructional options in a computer-based coordinate concept lesson. Educational Communication and Technology Journal, 33(3), 199-212.

CHANG 1987

Chang, Y. (1987). The effects on student learning of program control versus learner control in computer-based vocabulary drills. Dissertation Abstracts International, 49(1), 34A. (University Microfilms No. 8802995)

*ELLERMANN & FREE 1990

Ellermann, H.H., & Free, E. L. (1990). A subject-controlled environment for paired associate learning. Journal of Computer-Based Instruction, 17(3), 97-102.

GAY 1986

Gay, G. (1986). Interaction of learner control and prior understanding in computer-assisted instruction. Journal of Educational Psychology, 78, 225-227.

GILLINGHAM 1989

Gillingham, M., Garner, R., Guthrie, J. T., & Sawyer, R. (1989). Children's control of computer-based reading assistance in answering synthesis questions. Computers in Human Behavior, 5, 61-75.

GOETZFRIED & HANNAFIN 1985

Goetzfried, L., & Hannafin, M. J. (1985). The effect of the locus of CAI control strategies on the learning of mathematics rules. American Educational Research Journal, 22(2), 273-278.

GRAY 1987

Gray, S. (1987). The effect of sequence control on computer assisted instruction. Journal of Computer-Based Instruction, 14(2), 54-56.

HO 1986

Ho, C. P., Savenye, W., & Haas, N. (1986). The effects of orienting objectives and review learning from interactive video. Journal of Computer-Based Instruction, 13(4), 126-127.

JOHANSEN & TENNYSON 1983

Johansen, K. J., & Tennyson, R. D. (1983). Effect of adaptive advisement on perception in learner-controlled, computer-based instruction using a rule-learning task. Educational Communication and Technology Journal, 31(4), 226-236.

JUDD 1974A

Judd, W. A., O'Neil, H. F., & Spelt, P.F. (1974). Individual differences and learner control: II. Investigation of control over pictorial mediators in computer-assisted instruction. US AFHRL Technical Report No. 74-30. (ERIC No. ED 094 733)

JUDD 1974B

Judd, W. A., O'Neil, H. F., & Spelt, P. F. (1974). Individual differences and learner control: I. Program development and investigation of control over mnemonics in CAI. US AFHRL Technical Report No. 74-3. (ERIC No. ED 094 732)

KINZIE & SULLIVAN 1989

Kinzie, M. B., & Sullivan, H. J. (1989). Continuing motivation, learner control and CAI. Educational Technology Research and Development, 37(2), 5-14.

KINZIE ET AL 1988

Kinzie, M. B., Sullivan, H. J., & Berdel, R. L. (1988). Learner control and achievement in science computer-assisted instruction. Journal of Educational Psychology, 80(3), 299-303.

KLEIN & KELLER 1990

Klein, J. D., & Keller, J. M. (1990). Influence of student ability, locus of control, and type of instructional control on performance and confidence. Journal of Educational Research, 83(3), 140-146.

LEE & WONG 1989

Lee, S., & Wong, S. (1989). Adaptive program vs. learner control strategy on computer aided learning of gravimetric stoichiometry problems. Journal of Research on Computing in Education, Summer, 367-379.

LOPEZ & HARPER 1990

Lopez, C. L., & Harper, M. (1990). The relationship between learner control of CAI and locus of control among Hispanic students. Educational Technology Research and Development, 37(4), 19-28.

MACGREGOR 1988

MacGregor, S. K. (1988). Instructional design for computer-mediated text systems: Effects of motivation, learner control, and collaboration on reading performance. Experimental Education, 56(3), 142-147.

MILHEIM 1988

Milheim, W. D. (1988). The effects of two learner control variables -- pacing and sequence -- on learning from an interactive video lesson. Kent State University, Kent, OH.

MULLEN 1983

Mullen, F. M. (1983). The relationship between learner control, field orientation, and achievement in computer-aided instruction. Dissertation Abstracts International, 44(11), 3307A. (University Microfilms No. 8403326)

PETERS 1988

Peters, C. L. (1988). The effects of advisement, content mapping, and interactive video on learner control and achievement in computer-based instruction. (Doctoral Dissertation, University of Georgia, 1988).

POLLOCK & SULLIVAN 1990

Pollock, J. C., & Sullivan, H. J. (1990). Practice mode and learner control in computer-based instruction. Contemporary Educational Psychology, 15, 251-260.

ROSS ET AL 1990

Ross, S. M., Morrison, G. R., & O'Dell, J. K. (1990). Uses and effects of learner control of context and instructional support in computer-based instruction. Educational Technology Research and Development, 37(4), 29 - 39.

ROWLAND & STUESSY 1988

Rowland, P., & Stuessy, C. L. (1988). Matching mode of CAI to cognitive style: An exploratory study. Journal of Computers in Mathematics and Science Teaching, Summer, 36-40.

SCHLOSS ET AL 1988

Schloss, P. J., Wisniewski, L. A., & Cartwright, G. P. (1988). The differential effect of learner control and feedback in college students' performance on CAI modules. Journal of Educational Computing Research, 4(2), 141-150.

SCHLOSS ET AL 1988B

Schloss, P.J., Sindelar, P. T., Cartwright, G. P., & Smith, M. A. (1988). Learner control over feedback as a variable in computer assisted instruction. Journal of Research on Computing in Education, 20(4), 310-320.

SILVERSTEIN 1989

Silverstein, N. E. (1989). Computer-based training: The effects of graphics and learner control on retention. Dissertation Abstracts International, 50(7-A). (University Microfilms No. 8923414)

TENNYSON & PARK 1984

Tennyson, R. D., & Park, S. I. (1984). Process learning time as an adaptive design variable in concept learning using computer-based instruction. Journal of Educational Psychology, 76(3), 452-465.

TENNYSON 1980

Tennyson, R. D. (1980). Instructional control strategies and content structure as design variables in concept acquisition using computer-based instruction. Journal of Educational Psychology, 72(4), 525-532.

**TENNYSON 1981

Tennyson, R. D. (1981). Use of adaptive information for advisement in learning concepts and rules using computer-assisted instruction. American Educational Research Journal, 18(4), 425-438.

TENNYSON ET AL 1980

Tennyson, C. L., Tennyson, R. D., & Rothen, W. (1980). Content structure and instructional control strategies as design variables in concept acquisition. Journal of Educational Psychology, 72(4), 499-505.

TENNYSON ET AL 1985

Tennyson, R. D., Park, O., & Christensen, D. L. (1985). Adaptive control of learning time and content sequence in concept learning using computer-based instruction. Journal of Educational Psychology, 77(4), 481-491.

WENZEL & GOTFREDSEN 1988

Wenzel, A. & Gotfredsen, E. (1988). Learner versus teacher controlled structure in computer-assisted instruction for intraoral radiology. Dentomaxillofac. Radiol., 17, 49-55.

Data Resulting from Study Partitioning

	PCE	INT	REV	#EX	AMT	SEQ	TRT	MEAN	S.D.
** AVNER ET AL 1980-1									
PASSIVE CBI	1	0	0	0	0	0	0	0.00	0.00
INTERACTIVE CBI	1	1	0	0	0	0	0	0.00	0.00
** AVNER ET AL 1980-2									
PASSIVE CBI	1	0	0	0	0	0	0	0.00	0.00
INTERACTIVE CBI	1	1	0	0	0	0	0	0.00	0.00
** AXTELL 1978									
GH, IH, LC	1	1	1	1	0	0	0	78.30	4.50
NO HELP	1	1	1	1	0	0	0	82.50	3.10
GH, IH	1	1	1	1	0	0	0	84.10	4.40
GH	1	1	1	1	0	0	0	87.60	4.30
IH	1	1	1	1	0	0	0	83.10	4.40
GH, LC	1	1	1	1	1	0	0	81.30	4.50
IH, LC	1	1	1	1	1	0	0	82.90	4.30
** BELLAND ET AL 1985									
EXTERNALLY PACED + CP	0	1	1	0	0	0	0	12.22	1.10
EXTERNALLY PACED NO CP	0	1	1	0	0	0	0	9.02	1.10
SELF PACED	1	1	1	0	0	0	0	10.57	1.10
** BURWELL 1989-1									
PROGRAM CONTROL FIELD DEPENDENT	1	1	0	0	0	0	0	78.00	15.73
STUDENT CONTROL FIELD DEPENDENT	1	1	1	0	1	1	0	88.00	8.80
** BURWELL 1989-2									
PROGRAM CONTROL FIELD INDEPENDENT	1	1	0	0	0	0	0	86.00	6.87
STUDENT CONTROL FIELD INDEPENDENT	1	1	1	0	1	1	0	82.00	13.39
** CAMPANIZZI 1978									
PROGRAM CONTROL	1	1	0	0	0	0	0	0.00	0.00
LEARNER CONTROL	1	1	1	0	0	0	0	0.00	0.00
** CARRIER & WILLIAMS 1988									
FULL	1	1	0	0	0	0	0	6.20	3.00
LEAN	1	1	0	0	0	0	0	4.70	2.90
OPTIONS TREATMENT	1	1	0	1	1	0	0	7.40	3.20
** CARRIER ET AL 1984-1									
NO-OPTIONS/FI	1	1	0	0	0	0	0	11.55	3.46
OPTIONS/FI	1	1	0	1	1	0	0	11.45	3.53
** CARRIER ET AL 1984-2									
NO OPTIONS/FD	1	1	0	0	0	0	0	6.67	4.58
OPTIONS/FD	1	1	0	1	1	0	0	9.80	3.61
** CARRIER ET AL 1985									
FULL	1	1	0	0	0	0	0	10.14	2.91
LEAN	1	1	0	0	0	0	0	9.91	2.34
OPTIONS	1	1	0	1	1	0	0	11.71	1.56

Data Resulting from Study Partitioning (con't)

	PCE	INT	REV	#EX	AMT	SEQ	TRT	MEAN	S.D.
** CHANG 1987									
PROGRAM CONTROL	1	1	0	0	0	0	0	44.04	4.40
LEARNER-CONTROL	1	1	1	1	0	1	0	43.60	6.10
** ELLERMANN & FREE 1990									
DET	0	1	0	0	0	0	0	42.40	10.94
SET	1	1	0	0	0	1	0	48.50	7.27
FREE	1	1	1	0	0	1	0	64.80	5.88
** ELLERMANN & FREE 1990-2									
DET	0	1	0	0	0	0	0	10.20	7.02
FREE	0	1	0	1	0	1	0	16.70	5.76
** GAY 1986-1									
PROGRAM-CONTROLLED TREATMENT/HP-1	1	1	0	0	0	0	0	18.00	1.12
LEARNER-CONTROLLED TREATMENT/HP-1	1	1	1	1	1	1	1	17.85	1.46
** GAY 1986-2									
PROGRAM-CONTROLLED/LP-2	1	1	0	0	0	0	0	17.25	1.74
LEARNER-CONTROLLED/LP-2	1	1	1	1	1	1	1	14.35	2.81
** GILLINGHAM 1989									
NO RESOURCES (1)	1	1	1	0	0	0	0	1.20	0.75
ASSISTANCE	1	1	1	0	1	0	0	1.40	0.92
PRESCRIBED ASSISTANCE	1	1	1	0	1	0	0	1.90	0.93
** GOETZFRIED & HANNAFIN 1985									
ADAPTIVE CONTROL	0	1	0	0	0	0	0	5.40	1.50
LINEAR CONTROL	1	0	0	0	0	0	0	5.30	1.80
LEARNER CONTROL WITH ADVISEMENT	1	1	1	1	1	0	0	5.10	1.50
** GRAY 1987									
CONDITION 1 (LINEAR SEQUENCE)	1	1	1	1	1	0	0	48.75	16.04
CONDITION 2 (FLIP SEQUENCE)	1	1	1	1	1	1	0	66.00	17.51
** HO 1986A									
COMPUTER CONTROLLED REVIEW (OBJECTIVES)	1	1	0	0	0	0	0	28.30	2.82
LEARNER CONTROLLED REVIEW (OBJECTIVES)	1	1	1	0	0	0	0	28.32	2.97
** HO 1986B									
COMPUTER CONTROLLED (NO OBJECTIVES)	1	1	0	0	0	0	0	26.66	3.36
LEARNER CONTROLLED (NO OBJECTIVES)	1	1	1	0	0	0	0	26.34	4.31
** JOHANSEN & TENNYSON 1983									
ADVISEMENT LEARNER CONTROL	1	1	0	1	0	1	0	61.20	4.37
PARTIAL LEARNER CONTROL	1	1	0	1	0	1	0	49.90	6.98
LEARNER CONTROL	1	1	0	1	0	1	0	40.40	7.64

Data Resulting from Study Partitioning (con't)

	PCE	INT	REV	#EX	AMT	SEQ	TRT	MEAN	S.D.
** JUDD 1974A									
TREATMENT PRESENT	1	1	0	0	0	0	0	-30.90	11.20
LEARNER CONTROL	1	1	1	0	0	0	1	-33.60	10.34
** JUDD 1974B									
TREATMENT PRESENT	1	1	0	0	0	0	0	-16.30	11.40
LEARNER CONTROL	1	1	1	0	0	0	1	-10.50	4.73
** KINZIE & SULLIVAN 1989									
PROGRAM CONTROL	1	1	0	0	0	0	0	11.59	2.33
LEARNER CONTROL	1	1	1	0	0	0	0	10.83	2.83
** KINZIE ET AL 1988									
PROGRAM CONTROL	1	1	0	0	0	0	0	8.86	2.68
LEARNER CONTROL	1	1	1	0	0	0	0	10.21	2.56
** KLEIN & KELLER 1990									
PROGRAM CONTROL	1	1	0	0	0	0	0	10.12	3.31
LEARNER CONTROL	1	1	0	1	1	0	0	10.24	3.18
** LEE & WONG 1989									
PROGRAM CONTROL	1	1	0	0	0	0	0	15.08	4.92
LEARNER CONTROL	1	1	0	1	0	1	0	12.24	4.92
** LOPEZ & HARPER 1990									
NO	1	1	0	0	0	0	0	14.91	4.00
MODERATE	1	1	1	0	0	0	0	15.54	3.69
HIGH	1	1	1	1	0	0	0	14.25	4.20
** MACGREGOR 1988									
CC-EXT	1	1	1	1	0	0	0	6.50	1.40
SC-EXT	1	1	1	1	1	0	0	5.60	1.60
** MILHEIM 1988									
PROGRAM	0	0	0	0	0	0	0	16.57	2.74
LC PACE	1	0	0	0	0	0	0	16.80	3.89
LC SEQ	0	0	0	0	0	1	0	15.64	3.95
LC PACE AND SEQUENCE	1	0	0	0	0	1	0	15.67	3.70
** MULLEN 1983									
ADAPTIVE	1	1	0	1	1	0	0	11.00	4.48
ADVISED	1	1	1	1	0	1	0	8.04	3.97
** PETERS 1988									
5	1	1	1	0	0	0	0	192.40	34.99
3	1	1	1	1	0	0	0	200.90	32.80
4	1	1	1	1	0	0	0	190.30	31.02
1	1	1	1	1	1	1	0	201.40	27.68
2	1	1	1	1	1	1	0	192.60	32.03
6	1	1	1	1	1	1	1	189.40	29.85
** POLLOCK & SULLIVAN 1990									
PROGRAM CONTROL	1	1	0	0	0	0	0	0.00	0.00
LEARNER CONTROL	1	1	0	1	0	0	0	0.00	0.00

Data Resulting from Study Partitioning (con't)

	PCE	INT	REV	#EX	AMT	SEQ	TRT	MEAN	S.D.
** ROSS ET AL 1990									
MAXIMUM	1	1	1	0	0	0	0	26.38	8.19
MINIMUM	1	1	1	0	0	0	0	26.44	8.61
LEARNER CONTROL	1	1	1	1	0	0	0	27.44	8.17
** ROWLAND & STUESSY 1988									
SIMULATION	1	1	0	0	1	1	0	8.30	2.83
TUTORIAL	1	1	0	0	1	1	1	10.40	2.68
** SCHLOSS ET AL 1988-1									
FORCED LOOP NO FEEDBACK (FL/NF)	1	1	0	0	0	0	0	24.67	2.54
CHOICE LOOP WITHOUT FEEDBACK (CL/NF)	1	1	1	0	0	0	0	24.72	2.37
** SCHLOSS ET AL 1988-2									
FORCED LOOP WITH FEEDBACK (FL/F)	1	1	0	0	0	0	0	28.00	1.73
CHOICE LOOP WITH FEEDBACK (CL/F)	1	1	1	0	0	0	0	26.29	3.35
** SCHLOSS ET AL 1988B-1									
HAFI	1	1	0	0	0	0	0	26.50	1.09
HACL	1	1	1	0	0	0	0	25.46	2.96
** SCHLOSS ET AL 1988B-2									
LAFI	1	1	0	0	0	0	0	23.85	2.73
LACL	1	1	1	0	0	0	0	23.42	3.15
** SILVERSTEIN 1989-1									
GROUP 3 PROGRAM CONTROL, GRAPHICS	1	0	0	0	0	0	0	63.40	8.10
GROUP 1 LEARNER CONTROL, GRAPHICS	1	0	0	0	0	1	0	64.20	12.00
** SILVERSTEIN 1989-2									
GROUP 4 PROGRAM CONTROL, NO GRAPHICS	1	0	0	0	0	0	0	65.10	13.00
GROUP 2 LEARNER CONTROL, NO GRAPHICS	1	0	0	0	0	1	0	66.00	15.31
** TENNYSON & PARK 1984									
PLT	0	1	0	0	0	0	0	16.40	2.10
IPLT	0	1	0	0	0	0	0	13.70	3.10
MPLT	0	1	0	0	0	0	0	14.90	2.10
NPLT	1	1	0	0	0	0	0	15.10	2.20
** TENNYSON 1980 SIM									
ADAPTIVE CONTROL-SIMULTANEOUS	1	1	0	0	0	0	0	23.00	3.10
LEARNER CONTROL SIMULTANEOUS	1	1	0	1	1	1	0	15.80	4.90
LEARNER-ADAPTIVE CONTROL SIMULTANEOUS	1	1	0	1	1	1	0	23.80	3.70

DATA RESULTING FROM STUDY PARTITIONING (CON'T)

	PCE	INT	REV	#EX	AMT	SEQ	TRT	MEAN	S.D.
** TENNYSON 1980 SUC									
ADAPTIVE CONTROL SUCCESSIVE	1	1	0	0	0	0	0	17.90	3.90
LEARNER CONTROL SUCCESSIVE	1	1	0	1	1	1	0	13.60	4.40
LEARNER-ADAPTIVE CONTROL SUCCESSIVE	1	1	0	1	1	0	0	17.80	4.30
** TENNYSON 1981A									
ADAPTIVE CONTROL	1	1	0	0	0	0	0	20.60	2.90
LEARNER CONTROL	1	1	0	1	0	1	0	11.70	4.30
LEARNER ADAPTIVE CONTROL	1	1	0	1	1	1	0	19.50	3.00
** TENNYSON 1981B									
LEARNER-ADAPTIVE CONTROL	1	1	0	1	1	0	0	55.00	5.03
LEARNER-PARTIAL-CONTROL	1	1	0	1	1	0	0	42.00	5.78
LEARNER-CONTROL	1	1	0	1	1	0	0	30.80	5.82
** TENNYSON ET AL 1980-1									
GROUP 1	1	1	0	0	0	0	0	19.70	2.90
GROUP 2	1	1	0	1	1	0	0	14.50	4.70
** TENNYSON ET AL 1980-2									
GROUP 3	1	1	0	0	0	0	0	16.30	3.00
GROUP 4	1	1	0	1	1	0	0	10.50	5.00
** TENNYSON ET AL 1980-3									
GROUP 5	1	1	0	0	0	0	0	15.30	3.70
GROUP 6	1	1	0	1	1	0	0	12.40	4.20
** TENNYSON ET AL 1985A-1									
ADAPTIVE CONTROLLED GENERALIZATION (GROUP 1)	0	1	0	0	0	0	0	15.60	3.30
LEARNER CONTROLLED GENERALIZATION (GROUP 4)	1	1	0	0	0	0	0	13.60	2.50
** TENNYSON ET AL 1985A-2									
ADAPTIVE CONTROLLED DISCRIMINATION (GROUP 2)	0	1	0	0	0	0	0	14.20	2.60
LEARNER CONTROLLED DISCRIMINATION (GROUP 5)	1	1	0	0	0	0	0	12.30	4.40
** TENNYSON ET AL 1985A-3									
ADAPTIVE CONTROLLED GENERALIZATION/DISCRIMINATION (GROUP 3)	0	1	0	0	0	0	0	18.50	3.90
LEARNER CONTROL GENERALIZATION/DISCRIMINATION (GROUP 6)	1	1	0	0	0	0	0	14.90	4.90
** WENZEL & GOTFREDSEN 1988									
TEACHER CONTROL	1	1	0	1	1	0	0	31.00	2.04
LEARNER CONTROL	1	1	1	1	1	1	0	30.00	1.27

Effect Sizes for the 94 Treatment Pairs Used
in the Meta-Analysis

STUDY	N	ES	G		TOPIC	S	DUR	E	A	P	N	S	RESEAR
			E	E									
SCHLOSS ET AL 1988-1 0 0 1 0 0 0 0	36	0.0204	88	13	EDUCATION	1	90	J	T	APP	6	1	SCHLOS
SCHLOSS ET AL 1988-2 0 0 1 0 0 0 0	14	-0.6414	88	13	EDUCATION	1	90	J	T	APP	6	1	SCHLOS
SCHLOSS ET AL 1988B- 0 0 1 0 0 0 0	27	-0.4735	88	13	EDUCATION	1	90	J	T	APP	6	0	SCHLOS
SCHLOSS ET AL 1988B- 0 0 1 0 0 0 0	25	-0.1463	88	13	EDUCATION	1	90	J	T	APP	6	0	SCHLOS
SILVERSTEIN 1989-1 0 0 0 0 0 1 0	40	0.0774	89	13	PSYCHOLOGY	0	0	D	G	IBM	6	2	SILVER
SILVERSTEIN 1989-2 0 0 0 0 0 1 0	39	0.0632	89	13	PSYCHOLOGY	0	0	D	G	IBM	6	2	SILVER
TENNYSON & PARK 1984 1 0 0 0 0 0 0	48	-0.6045	84	9	SCIENCE	1	25	J	P	APP	6	1	TENNY
TENNYSON & PARK 1984 1 0 0 0 0 0 0	48	0.5208	84	9	SCIENCE	1	25	J	P	APP	6	1	TENNY
TENNYSON & PARK 1984 1 0 0 0 0 0 0	48	0.0930	84	9	SCIENCE	1	25	J	P	APP	6	1	TENNY
TENNYSON 1980 SIM 0 0 0 1 1 1 0	66	-1.7561	80	13	SCIENCE	1	20	J	P	CRT	6	1	TENNY
TENNYSON 1980 SIM 0 0 0 1 1 1 0	66	0.2344	80	13	SCIENCE	1	20	J	P	CRT	6	1	TENNY
TENNYSON 1980 SUC 0 0 0 1 1 1 0	66	-1.0343	80	13	SCIENCE	1	20	J	P	CRT	6	1	TENNY
TENNYSON 1980 SUC 0 0 0 0 0 1 0	66	-0.9655	80	13	SCIENCE	1	20	J	P	CRT	6	1	TENNY
TENNYSON 1980 SUC 0 0 0 1 1 0 0	66	-0.0244	80	13	SCIENCE	1	20	J	P	CRT	6	1	TENNY
TENNYSON 1981A 0 0 0 0 1 0 0	42	2.1039	81	12	SCIENCE	1	30	J	P	APP	6	2	TENNY
TENNYSON 1981A 0 0 0 1 1 1 0	42	-0.3728	81	12	SCIENCE	1	30	J	P	APP	6	2	TENNY
TENNYSON 1981A 0 0 0 1 0 1 0	42	-2.4268	81	12	SCIENCE	1	30	J	P	APP	6	2	TENNY
TENNYSON ET AL 1980- 0 0 0 1 1 0 0	30	-1.3316	80	10	PSYCHOLOGY	1	15	J	P	TTY	6	2	TENNY
TENNYSON ET AL 1980- 0 0 0 1 1 0 0	30	-1.4067	80	10	PSYCHOLOGY	1	15	J	P	TTY	6	1	TENNY
TENNYSON ET AL 1980- 0 0 0 1 1 0 0	30	-0.7327	80	10	PSYCHOLOGY	1	15	J	P	TTY	6	1	TENNY
TENNYSON ET AL 1985A 1 0 0 0 0 0 0	24	-0.6832	85	11	PSYCHOLOGY	1	50	J	P	APP	6	0	TENNY
TENNYSON ET AL 1985A 1 0 0 0 0 0 0	24	-0.5258	85	11	PSYCHOLOGY	1	50	J	P	APP	6	0	TENNY
TENNYSON ET AL 1985A 1 0 0 0 0 0 0	24	-0.8129	85	11	PSYCHOLOGY	1	50	J	P	APP	6	0	TENNY
WENZEL & GOTFREDSEN 0 0 1 0 0 1 0	40	-0.5885	88	13	SCIENCE	18	27	J	T	IBM	6	0	WENZEL

APPENDIX B

dBASE Documentation

OVERVIEW OF dBASE FILES AND PROCEDURES

List of dBase Files, Report Formats, and Command Files

The files and command files are designed to store information on the primary studies used for the meta-analysis, then analyze them to create a file of effect sizes suitable for use with SPSS.

LCGRID.DBF	LCGRID.FRM	GENERATE.PRG
LCFACT.NDX	LCVAR.FRM	CLACES.PRG
LCGRID.NDX	LCSUMM.FRM	MAKEFILE.PRG
LCTEMPL.DBF	LCSPSS.FRM	BBMETA.PRG
STARTUP.DBF		OPEN.PRG

dBase Procedures

For this meta-analysis, data were stored in a dBase III+ file on a Kaypro 286 personal computer. dBase III+ command files were used to calculate effect sizes, perform the bare-bones meta-analysis, and report results. Some ancillary procedures were run on SPSSx release 4.0 for IBM OS/MVS running on the mainframe computer system at Northern Michigan University and accessed through the MUSIC user facility.

1. To enter study data, open the necessary files and indexes by typing DO OPEN. Use the APPEND command to add records. One record is completed for each treatment in a study. Refer to the File Structure section for details on entry conventions, codes, abbreviations, etc.
2. After data for all studies have been entered and an analysis is required, use the command DO GENERATE to launch the program which selects studies, calls the module to calculate effect sizes, performs the bare-bones meta-analysis, and creates a data file containing the results of the analysis.
3. The "Bare-Bones Meta-analysis" calculations are not stored. They can be redisplayed by SELECTing LCTEMPL as AREA 2, then requesting DO BBMETA. Request SET PRINT ON for a hard copy of this report.
4. After the analysis is complete, to create a data file that SPSS will use, SELECT 2 then request: REPORT FORM LCSPSS PLAIN TO FILE SPSS.DOS. This creates an ASCII file which can be uploaded to and read by SPSS. This procedure is only necessary for analyses which require the use of SPSSx.

5. ASCII files may be uploaded to the mainframe MUSIC system using the MUSIC command XTMUS <filename>. This file may then be merged into the SPSS command file as a DATA LIST or referenced using the FILE HANDLE command.

dBase File Structures

FILE: LCGRID.DBF

Description: The main file for storing study data. One record is used for each treatment in a study.

INDEXES: LCGRID - ON SHORTCIT
LCFACT - ON PCE+INT+NUM+AMT+SEQ+INS

REPORTS: LCGRID - CIT/PCE/INT... GROUPED BY CITATION
(USE LCGRID.NDX)

LCVAR - SHORTCIT, GRADE, MEAN, SD... BY STUDY
(USE INDEX LCGRID)

LCSUMM - SHORTCIT, TREAT, PCE, INST...
GROUPED BY LC FACTORS (USE INDEX LCFACT)

FILE STRUCTURE:

NAME	TYPE	WIDT	DEC	MEMVAR	NOTES
SHORTCIT	C	40		MSHORTCIT	
TREAT_GRP	C	80			
N	N	3		MN(1,2)	NUMBER
MEAN	N	6	2	MMEAN(1,2)	TRT. MEAN
SD	N	6	2	MSD(1,2)	TRT STD.D.
COMMENT	M				
PCE	C	1		MPCE	PACE
INT	C	1		MINT	INTERACTION
REV	C	1		MREV	REVIEW
NUM	C	1		MNUM	NUM. OF EXERC.
AMT	C	1		MAMT	AMT OF MATERIAL
SEQ	C	1		MSEQ	SEQUENCE
INS	C	1		MINS	INSTRUCTION
DATE	C	2		MDATE	DATE OF STUDY
GRADE	N	2		MGRADE	YEAR IN SCHOOL
SUBJECT	C	10		MSUBJECT	TOPIC OF LESSON
SESSIONS	N	2		MSESSIONS	# OF SESSIONS
DURATION	N	4		MDURATION	# OF SESSIONS
PUBFRM	C	1		MPUBFRM	PUBL. FORM
MEDIA	C	1		MMEDIA	TYPE OF MEDIA
EQUIPMENT	C	3		MEQUIP	IBM, APP, MAC, OTH
EXPDESIGN	C	2		MEXPDES	1 - 16
THREATS	N	1		MTHREATS	TO INTERNAL VALIDITY
RESEARCHER	C	15		MRESEARCH	PRIMARY RESEARCHER
UPDATE	D			MDATE	DATE OF LAST UPDATE
IPCE	C	1		MIPCE	LOCUS OF PACE

IINT	C	1		MIINT	LOCUS OF INTERACTION
IREV	C	1		MIREV	LOCUS OF REVIEW
INUM	C	1		MINUM	LOCUS OF # OF EX.
IAMT	C	1		MIAMT	LOCUS OF AMOUNT
ISEQ	C	1		MISEQ	LOCUS OF SEQUENCE
IINS	C	1		MIINS	LOCUS OF COG FORMAT
FLAG	N	1		MFLAG	FLAG=1 IF MANUAL ES, 2 IF NOT AVAILABLE
DUP	C	15		MDUP	LEAVE BLANK
POSSIBLE	N	3		MPOSS	MAXIMUM POSSIBLE ON POSTTEST
ATTMEAN	N	6	2	MATTMEAN	ATTITUDE MEAN
ATTSD	N	6	2	MATTSD	ATTITUDE S.D.
ATTFLAG	N	1		MATTFLAG	2 IF NOT AVAILABLE
TIMEMEAN	N	6	2	MTIMEMEAN	TIME-ON-TASK MEAN
TIMESD	N	6	2	MTIMESD	TIME-ON-TASK S.D.
TIMEFLAG	N	1		MTIMEFLAG	2 IF NOT AVAIALBLE

Coding Entries into LCGRID

MSHORTCIT

The last name of the primary researcher followed by the date: PETERS 1988. If two authors: PETERS & JONES 1988. If more than two authors: PETERS ET AL 1988.

TREAT_GRP

The name of the treatment group. Use author's terminology, if possible.

N

The number of subjects in the treatment group.

MEAN

The mean of the treatment group.

SD

The standard deviation of the treatment group.

NOTE: if MEAN and SD are not available, set FLAG = 1.

COMMENT

Miscellaneous comments about the study or treatment group.

PCE (Pace = display time)

Set PACE = 1 if the learner controls the time a screen is displayed. Set PACE = 0 if the screen is displayed for some fixed or calculated period of time.

INT (Interaction/feedback)

Set INT = 1 if the learner is able to enter information such as responses and answers which are evaluated and responded to by the program. Set INT = 0 if the learner only presses the Enter key to move through the program or if learner input is not responded to.

REV (Branch back and review previous instruction)

Set REV = 1 if the learner is able to decide if he/she wants to try missed problems again, perhaps after viewing previous instructional screens. Set REV = 0 if the student is forced to review or if there is no review.

NUM (Number of exercises)

Set NUM = 1 if the student can select the number of exercises to try. This can either be by indicating, for example, that 5 exercises are desired or by responding to a prompt such as "Do you want to try another problem?" Set NUM = 0 if the student does not select the number of exercises (or if there are no exercises in the program).

AMT (Amount of instructional information)

Set AMT = 1 if the student can select the number of screens or otherwise determine the amount of concept instruction to be received. Set AMT = 0 if the program determines the amount of instruction.

SEQ (Sequence of topics)

Set SEQ = 1 if the learner can select the order in which to learn concepts. These might be a sequence of rules, topics, modules, etc. Set SEQ = 0 if the program determines the order of concept presentation.

INS (Cognitive presentation of instruction)

Set INS = 1 if the student can select the cognitive format of instruction (i.e., inductive, deductive, graphic, audio, text, etc.)

DATE (Study date)

Enter the last two digits of the year.

GRADE

Enter the grade of the students in the study. 0 = Kindergarten, 1 = first grade, etc. 13 = undergraduate. 17 = graduate. If students are from more than one grade, use the highest grade.

SUBJECT

Enter the subject of instruction using one of the following categories: MATH, SCIENCE, ENGLISH, POLY SCI (government), SOCIOLOGY, EDUCATION.

SESSIONS

Enter the number of sessions the students received computer-based instruction. Do not include sessions in which the students only took paper and pencil tests or received orientation not related to the subject of instruction. If number of sessions are unknown, enter 0.

DURATION

Enter the duration in minutes of the instructional sessions. The same restrictions as for SESSIONS apply. For example, if students spent 3 sessions with the CBI of 50 minutes each, the DURATION = 150. Use the maximum time allotted for the sessions, if given. If this time is not indicated, use the mean time on task + one standard deviation. If time is unknown, enter 0.

PUBFRM (Publication form)

Enter J for journals, D for dissertation, P for proceedings, O for other.

MEDIA

Enter C if all instruction is on a computer, P if the computer is augmented by print materials, V if computer is augmented by video or film, A if computer is augmented by audio, M if computer is augmented by multiple media.

EQUIPMENT (Type of computer)

APP = Apple II/+/e, MAC = Macintosh, IBM = IBM PC or clone, TTY = teletype terminal for a mainframe, VDT = VDT terminal for a mainframe, UNK = unknown.

EXPDESIGN (Experimental design)

Use Campbell and Stanley designs 1 - 17.

THREATS (Threats to internal validity)

Use Campbell and Stanley. Since most designs are of type 6, typical threats might involve:

- Non-randomized assignment to treatment groups
- Not all students tested at once (history)
- Students treated in a "special" setting (instrument)
- No explanation of student drop-out

RESEARCHER

Some researchers have done several learner control studies. Their experimental and CAI designs are likely to be similar. Enter the name of the most prolific researcher on the team.

UPDATE

Enter the date when the record is created or modified.

IPCE (Locus of control of display time)

IPCE = L if the learner can decide when to move to the next instructional display. IPCE = F if displays may be viewed for a set time determined by the program. The time will be the same for all students. IPCE = A if displays are shown for an interval determined for a particular learner by use of an adaptive algorithm.

IINT (Type of interaction)

IINT = L if the learner can select the type of interaction, i.e., asking for performance statistics, requesting elaborate or minimal feedback on responses. IINT = F if a non-evaluative response follows learner input, i.e., "Here's another one." IINT = A if students are given context sensitive feedback, i.e., "That's correct!" IINT = N if the student is not given feedback, or if the student is not given the opportunity to input.

IREV (Locus of control over review)

IREV = L if the learner can control review of previous material or retry of missed exercises/problems. IREV = F if the program requires all students to review. IREV = A if

review is adaptive - it is required for some students, but not for others, depending on performance or other student variables. IREV = W if students are given advice on review, but following the advice is optional. IREV = N if there is no facility for review.

INUM (Locus of control over the number of exercises)
 INUM = L if the learner can control the number of problems worked. INUM = F if a fixed number of problems are given to all students. INUM = A if the number of problems vary from student to student, based on an adaptive algorithm. INUM = W if the student is advised about the number of problems to work, but following the advice is optional. INUM = N if there are no problems.

IAMT (Locus of control of the amount of instructional information)
 IAMT = L if the learner can control the number of instructional displays or otherwise control the amount of instructional material. IAMT = F if the amount of instructional information is constant for all students. IAMT = A if the amount of instructional information varies for each student based on student performance or other student variables. IAMT = W if learners are advised about the amount of instruction which should be viewed, but the decision is left to the student.

ISEQ (Locus of control over the sequence of instructional presentation)
 ISEQ = L if the learner can select the order of study topics, or instructional events. ISEQ = A if the order of instruction is adapted to the individual student based on performance or other student variables. ISEQ = F if the instructional sequence is constant for all students. ISEQ = W if students are advised on the optimal instructional sequence, but may determine whether or not to follow the advice.

IINS (Locus of control over instructional format)
 IINS = L if the learner may request alternate cognitive instructional formats such as inductive, deductive, graphical, video, text. IINS = fixed if the cognitive presentation is constant for all students. IINS = A if the cognitive presentation is adapted to a particular student's needs based on performance or other student variables. IINS = W if students are advised on the optimal cognitive instructional format, but are given the option whether or not to follow the advice.

FLAG

Set FLAG = 1 if means and/or standard deviations are not available and effect size must be entered manually.

DUP

This field is used for processing. Leave it blank.

POSSIBLE

Enter the maximum possible posttest score. If the score is not available, enter 0.

ATTMEAN

Enter the mean score on the attitude questionnaire, if available. If not available, enter 0 and set ATTFLAG=2.

ATTMEAN

Enter the standard deviation from the attitude questionnaire, if available. If not available, enter 0 and set ATTFLAG=2.

ATTFLAG

This is set to 2 if data on attitude are not available.

TIMEMEAN

Enter the mean time on task for the treatment group, if available. If not available, enter 0 and set TIMEFLAG=2.

TIMESD

Enter the standard deviation for time on task, if available. If not available, set TIMEFLAG=2.

TIMEFLAG

This is set to 2 if data for time on task are not available.

FILE: LCTEMP.DBF

Description: A temporary file used in processing effect sizes.

NO INDEXES, NO REPORTS. THIS FILE IS A TEMPORARY FILE USED AS AN ARRAY FOR PROCESSING.

SAME FILE STRUCTURE AS LCGRID.

FILE: LCTEMPL.DBF

Description: This file is created by the command file MAKEFILE. It contains the subset of treatments used for a particular bare-bones meta-analysis.

INDEXES:

REPORTS: LCSPSS.FRM - SHORTCIT, EFFECT SIZE, GRADE, DURATION...

FILE STRUCTURE:

NAME	TYPE	WIDT	DEC	MEMVAR	NOTES
SHORTCIT	C	40		MSHORTCIT	
N	N	3		NM	NUMBER IN BOTH TRTS
ESH	N	8	4	MESH	HUNTER EFFECT SIZE
DATE	C	2		MDATE	STUDY DATE
GRADE	N	2		MGRADE	YEAR IN SCHOOL
SUBJECT	C	10		MSUBJECT	TOPIC OF CAI
DURATION	N	1		MDURATION	MINUTES OF INSTRUCT
SESSIONS	N	2		MSESSIONS	# OF SESSIONS
PUBFRM	C	1		MPUBFRM	PUBL. FORM
MEDIA	C	1		MMEDIA	MEDIA TYPE
EQUIPMENT	C	3		MEQUIP	IBM, APP, MAC, OTH
EXPDESIGN	C	2		MEXPDES	1 - 16
THREATS	N	1		MTHREATS	TO INTERNAL VALIDITY
RESEARCHER	C	15		MRESEARCH	
FLAG	N	1		MFLAG	

FILE: STARTUP.DBF

Description: this file holds data on the last logical search request. It also stores the name of the file last used for output.

NO INDEXES OR REPORTS.

FILE STRUCTURE:

NAME	TYPE	WIDT	DEC	MEMVAR	NOTES
ST1	C	80		S1	STRING 1
ST2	C	80		S2	STRING 2
OPFILE	C	8		MOPFILE	OUTPUT FILE
NAME					

FILE: META.DBF

Description: This file contains the complete bibliographic citation for each study used in the analyses.

NAME	TYPE	WIDT	DEC	MEMVAR	NOTES
SHORTCIT	C	40			
CITATION	C	256			

INDEXES:

META - ALPHABETIZED ON CITATION (MAIN AUTHOR'S LAST NAME)

Command Files

COMMAND FILE: GENERATE

=====

```

*GENERATE: CONTROL PROGRAM FOR CREATING EFFECT SIZE DATA
FILE
SET TALK OFF
SET DELETED ON
CLOSE ALL DATABASES
CLEAR
USE LCTEMPL
DELE ALL
PACK
USE STARTUP
STORE 00.0000 TO MESH
STORE 00.0000 TO MESHG
STORE ST1 TO S1
STORE ST2 TO S2
STORE OPFILE TO MOPFILE
CLEAR
@ 1,1 SAY "THIS PROGRAM WILL GENERATE AN EFFECT SIZE DATA
FILE"
@ 3,1 SAY "VARIABLES = PCE INT NUM AMT SEQ INS"
@ 5,1 SAY "ENTER LOGICAL STRING #1: "
@ 6,1 GET S1
@ 8,1 SAY "ENTER LOGICAL STRING #2: "
@ 9,1 GET S2
@ 11,1 SAY "ENTER OUTPUT FILE NAME: " GET MOPFILE
READ
REPLACE ST1 WITH S1, ST2 WITH S2
IF MOPFILE <> 'LCTEMPL'
    USE LCTEMPL
    COPY STRU TO &MOPFILE
    USE
ENDIF
SELECT 2
*USE LCTEMPL
USE &MOPFILE
select 1
use lcgrid INDEX LCGRID
SORT ON SHORTCIT /A, TOTAL /D TO LC
SELECT 1
USE LC
*START MAIN LOOP
DO WHILE .NOT. EOF()
SELECT 1
STORE SHORTCIT TO MSHORTCIT1
STORE RECNO() TO REC
? "THIS IS "+SHORTCIT

*MOVE DATA FOR ONE STUDY INTO A TEMPORARY FILE

```



```

Copy all TO LCTEMP FOR SHORTCIT = MSHORTCIT1
SELECT 3
USE LCTEMP
GO TOP
STORE DATE TO MDATE
STORE GRADE TO MGRADE
STORE SUBJECT TO MSUBJECT
STORE DURATION TO MDURATION
STORE PUBFRM TO MPUBFRM
STORE MEDIA TO MMEDIA
STORE EQUIPMENT TO MEQUIP
STORE EXPDESIGN TO MEXPDES
STORE THREATS TO MTHREATS
STORE RESEARCHER TO MRESEARCH
STORE SESSIONS TO MSESSIONS
STORE FLAG TO MFLAG
*LOOK AT EACH TREATMENT
DO WHILE .NOT. EOF()
  STORE RECNO() TO REK
  IF &S1
    STORE SHORTCIT TO MSHORTCIT1
    STORE TREAT GRP TO MTREAT
    STORE PCE TO MPCE
    STORE INT TO MINT
    STORE REV TO MREV
    STORE NUM TO MNUM
    STORE AMT TO MAMT
    STORE SEQ TO MSEQ
    STORE INS TO MINS
    STORE N TO MN1
    STORE MEAN TO MMEAN1
    STORE SD TO MSD1
  STORE DUP TO MDUP
  STORE IPCE TO MIPCE
  STORE IINT TO MIINT
  STORE IREV TO MIREV
  STORE INUM TO MINUM
  STORE IAMT TO MIAMT
  STORE ISEQ TO MISEQ
  STORE IINS TO MIINS
  GO TOP

  *CHECK FOR A POSSIBLE MATCH
  DO WHILE .NOT. EOF()
  IF &S2 .AND. RECNO() <> REK .AND.
    AT(LTRIM(STR(RECNO())) , MDUP) = 0

*
IF MFLAG = 1, THE EFFECT SIZE MUST BE MANUALLY
INPUT
IF MFLAG = 1
  STORE 00.00 TO MESH
  STORE N TO MN2

```

```

? SHORTCIT
?   MTREAT, MPCE, MINT, MREV, MNUM, MAMT,
    MSEQ, MINS, REK
?   TREAT_GRP, PCE, INT, REV, NUM, AMT,
    SEQ, INS, RECNO()
INPUT "PLEASE ENTER THE EFFECT SIZE: " TO
    MESH
REPLACE DUP WITH TRIM(DUP)+LTRIM(STR(REK))
STORE VAL(MPCE)-VAL(PCE) TO MX1
STORE VAL(MINT)-VAL(INT) TO MX2
STORE VAL(MREV)-VAL(REV) TO MX3
STORE VAL(MNUM)-VAL(NUM) TO MX4
STORE VAL(MAMT)-VAL(AMT) TO MX5
STORE VAL(MSEQ)-VAL(SEQ) TO MX6
STORE VAL(MINS)-VAL(INS) TO MX7
ELSE
STORE N TO MN2
STORE MEAN TO MMEAN2
STORE SD TO MSD2
?   MTREAT, MPCE, MINT, MREV, MNUM, MAMT,
    MSEQ, MINS, REK
?   TREAT_GRP, PCE, INT, REV, NUM, AMT, SEQ, I
    NS, RECNO()
STORE VAL(MPCE)-VAL(PCE) TO MX1
STORE VAL(MINT)-VAL(INT) TO MX2
STORE VAL(MREV)-VAL(REV) TO MX3
STORE VAL(MNUM)-VAL(NUM) TO MX4
STORE VAL(MAMT)-VAL(AMT) TO MX5
STORE VAL(MSEQ)-VAL(SEQ) TO MX6
STORE VAL(MINS)-VAL(INS) TO MX7
? MX1, MX2, MX3, MX4, MX5, MX6, MX7
? "THESE TWO FIT THE CRITERIA"
REPLACE DUP WITH TRIM(DUP)+LTRIM(STR(REK))

*           ACCEPT "PRESS ANY KEY" TO JUNK
           DO CALCES
ENDIF
           DO MAKEFILE
*           STORE TRIM(MDUP)+LTRIM(STR(RECNO())) TO
           MDUP
           ENDIF
           SKIP
           *END POSSIBLE MATCH CHECK
ENDDO
ENDIF
GOTO REK
SKIP
ENDDO
*END PROCESSING FOR ONE STUDY
USE
DELETE FILE LCTEMP.DBF

```

```

*END MAIN LOOP
SELECT 1
GOTO REC
DO WHILE SHORTCIT = MSHORTCIT1 .AND. .NOT. EOF()
SKIP
ENDDO
ENDDO
CLEAR
use startup
*store opfile to mopfile
? S1
? S2
do bbmeta
? "ALL DONE! - YOUR DATA IS IN A FILE CALLED " + MOPFILE

```

COMMAND FILE: CALCES

=====

```

*CALCES.PRG
* CALCULATE HUNTER EFFECT SIZE (MESH)
STORE MSD1*MSD1 TO MSSD1
STORE MSD2*MSD2 TO MSSD2

IF MN1 = MN2
  S = SQRT((MSSD1+MSSD2)/2)
ENDIF

IF MN1 <> MN2
  S = SQRT(((MN1-1)*MSSD1)+((MN2-1)*MSSD2))/(MN1+MN2-2))
ENDIF

STORE (MMEAN1 - MMEAN2)/S TO MESH
? MESH
RETURN

```

COMMAND FILE: BBMETA

=====

```

*BBMETA: PERFORMS CALCULATIONS FOR HUMTER'S "BARE BONES
*META-ANALYSIS" USING DATA FROM LCTEMPL
SELECT 2
*USING LCTEMPL

*CALCULATE THE TOTAL NUMBER OF SUBJECTS (T)
SUM ALL N TO T
? "T", T

*CALCULATE AVERAGE NUMBER OF SUBJECTS IN EACH TREATMENT
(NUM)
GO BOTTOM
STORE RECNO() TO K
STORE T/K TO NUM
? "K", K

```

```

*CALCULATE WEIGHTED AVERAGE OF THE EFFECT SIZES (AVED)
SUM ALL (N*ESH) TO TAVED
STORE ROUND(TAVED/T,4) TO AVED
? "AVED", AVED

```

```

*CALCULATE THE WEIGHTED VARIANCE OF EFFECT SIZES (VARD)
SUM ALL (N*((ESH- AVED)^2)) TO TVARD
STORE TVARD/T TO VARD
? "VARD", VARD

```

```

*ESTIMATE THE SMALL SAMPLE ESTIMATE OF THE SAMPLINE ERROR
*VARIANCE (VARE)
STORE ((NUM-1)/(NUM-3))*(4/NUM)*(1+AVED^2/8) TO VARE
? "VARE", VARE

```

```

*ESTIMATE THE STUDY POPULTION EFFECT SIZE VARIANCE (POPVAR)
STORE VARD - VARE TO POPVAR
? "POPVAR", POPVAR

```

```

*ESTIMATE THE POPULATION STANDARD DEVIATION (POPSD)
IF POPVAR >0
  STORE SQRT(POPVAR) TO POPSD
ENDIF
IF POPVAR <=0
  STORE 0 TO POPSD
ENDIF
? "POPSD", POPSD

```

```

*CALCULATE EFFECT SIZE VALUES EXPECTED IN 95% OF CASES
STORE AVED-1.96*POPSD TO LL
STORE AVED+1.96 *POPSD TO UL
? STR(LL,5,2)+"< Population Mean "<"+STR(UL,5,2)
? "CLINICAL SIGNIFICANCE = " , aved/popsd
RETURN

```

COMMAND FILE: MAKEFILE

=====

```

*MAKEFILE:  PRODUCES 1 RECORD IN A FILE CALLED LCTEMPL WHICH
WILL
*BE USED BY SPSS AS 1 EFFECT SIZE.

```

```

SELECT 2
APPEND BLANK
REPLACE SHORTCIT WITH MSHORTCIT1
REPLACE N WITH MN1+MN2
REPLACE ESH WITH MESH
REPLACE DATE WITH MDATE
REPLACE GRADE WITH MGRADE
REPLACE SUBJECT WITH MSUBJECT
REPLACE DURATION WITH MDURATION
REPLACE SESSIONS WITH MSESSIONS

```

```
REPLACE PUBFRM WITH MPUBFRM
REPLACE MEDIA WITH MMEDIA
REPLACE EQUIPMENT WITH MEQUIP
REPLACE EXPDESIGN WITH MEXPDES
REPLACE THREATS WITH MTHREATS
REPLACE RESEARCHER WITH MRESEARCH
REPLACE X1 WITH MX1
REPLACE X2 WITH MX2
REPLACE X3 WITH MX3
REPLACE X4 WITH MX4
REPLACE X5 WITH MX5
REPLACE X6 WITH MX6
REPLACE X7 WITH MX7
SELECT 3
RETURN
```

BIOGRAPHICAL DATA

June Parsons has been a faculty member at Northern Michigan University since 1976. Her current position entails a faculty appointment in the Department of Marketing, Management, and Computer Information Systems in the Walker L. Cisler School of Business and additional responsibilities as the campus-wide Academic Computing Coordinator.

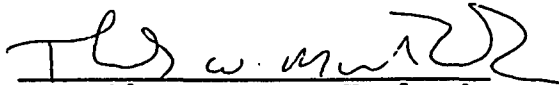
Ms. Parsons studied psychology as an undergraduate student at the University of Michigan from 1969 - 1971. She received the B. A. in Education from Northern Michigan University in 1974 Summa Cum Laude, then completed the M. A. in Education from Northern Michigan University in 1977.

A variety of computer-related positions have kept Ms. Parsons busy through the years including owner of a computer retail store, design consultant for the Traveler's Guild line of foreign language courseware, and textbook reviewer for computer texts from publishers such as Wiley, Benjamin Cummings, MacMillan, and Irwin.

I certify that I have read and am willing to sponsor this dissertation submitted by June A. Parsons. In my opinion, it conforms to acceptable standards and is fully adequate in scope and quality as a dissertation for the degree of Doctor of Education at Nova University.

1-6-92

(date)



Dr. Thomas W. MacFarland
Major Advisor

I certify that I have read this dissertation and in my opinion it conforms to acceptable standards for a dissertation for the degree of Doctor of Education at Nova University.

Dec 9, 1991.

(date)




Dr. Ramachandran Bharath
Committee Member

I certify that I have read this dissertation and in my opinion it conforms to acceptable standards for a dissertation for the degree of Doctor of Education at Nova University.

1-6-92

(date)



Dr. John Flynn
Committee Member