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TWO TYPES OF TEACHER-LEARNER INTERACTION IN LEARNING BY DISCOVERY. FINAL REPORT.

BY- TWELKER, PAUL A.
OREGON STATE SYSTEM OF HIGHER EDUCATION, MONMOUTH

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EXPERIMENTAL VARIABLES INTENDED TO REPRESENT A CONTINUUM OF REINFORCEMENT FOR SEARCHING BEHAVIOR WERE--REINFORCEMENT BY PRAISE ONLY (1), AND REINFORCEMENT BY PRAISE PLUS INDIRECT GUIDANCE ON HOW TO PROCESS INFORMATION AVAILABLE TO THE LEARNER (2). A THIRD INSTRUCTIONAL CONDITION REQUIRED MINIMUM TEACHER-LEARNER INTERACTION AND DIRECT PRESENTATION OF THE SAME INFORMATION INTENDED FOR DISCOVERY IN (1) AND (2). HIGHER SCORES ON CRITERION TRANSFER TESTS WERE HYPOTHESIZED FOR (1) AND (2) OVER (3) AND (1), RESPECTIVELY. A FACTORIAL DESIGN VARIED CONDITIONS AND THREE ABILITY LEVELS FOR 121 ELEMENTARY STUDENTS RANDOMLY ASSIGNED TO TREATMENTS. BOTH THE PROGRAM PRESENTATION AND STUDENT RESPONSES WERE AUTOMATED. GROUPS OF EIGHT STUDENTS WERE TAUGHT IN 45 MINUTE SESSIONS OVER THREE WEEKS. ANY STUDENT FAILING A TEST FRAME WAS BRANCHED INTO A REMEDIAL, "DIRECT PRESENTATION-TYPE" LOOP, BUT FAILURE OCCURRED ABOUT EQUALLY FOR EACH CONDITION. DEPENDENT VARIABLES WERE MEASURES OF LEARNING, TRANSFER, RETENTION, AND SAVINGS TRANSFER (TIME SAVED IN LEARNING NEW MATERIAL). SCORES FOR (1) WERE NOT HIGHER THAN THOSE FOR (3). FOR (2) AND (1), ONLY ONE TRANSFER TEST SHOWED SIGNIFICANT TREATMENT EFFECTS, BUT FOR MIDDLE ABILITY STUDENTS, THE DIFFERENCES WERE OPPOSITE TO THOSE HYPOTHESIZED. POSSIBLE EXPLANATIONS WERE--TOO SHORT PROGRAMS, TOO LOW A CRITERION LEVEL TO OBSERVE DIFFERENTIAL TRANSFER EFFECTS, AND GROUP INSTRUCTION PROBLEMS. (LH)

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Paul A. Twelker

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CONTENTS

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Chapter I Introduction

In recent years, much attention has been directed toward "learning by discovery" and "teaching by discovery." A substantial emphasis on curriculum development alludes to the increased interest (3,5,8,10). Proponents of discovery learning claim that the learner is benefited in four ways: (1) his ability to learn related materials is increased; (2) his interest in the activity itself is developed, rather than in the rewards which may follow from the learning; (3) his ability is developed to approach problems in a way that will more likely lead to a solution, and (4) his ability is developed to more easily retrieve or reconstruct from memory material which he has learned (2,4).

One explanation for these claims is based upon the assumption that learning by discovery enables the learner to engage in exploration or "searching behavior"[1] and as a result of this behavior, good problem-solving strategies are learned and interest is fostered. As a converse to the above assumption, it is assumed that as opportunities for searching behavior decrease (e.g., as greater amounts of guidance are provided during instruction), so does the potential for attaining the benefits of discovery learning.

Research evidence is not clearly in support of the claims made for the discovery learning approach (cf, 1). For example, research evidence has indicated that withholding information in order that learners may discover for themselves may actually reduce performance, increase the time required for learning, and decrease affectivity toward the learning experience (24, 27). Even attempts to introduce information after a time of independent discovery by the learner may result in decreased motivation to learn new subject matter (23). In these and similar studies,

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[1]Searching behavior may be defined as behavior which benefits a learner in problem-solving and related tasks, such as checking a preliminary solution before accepting it, shifting strategies to a solution rather than persevering with a single strategy, checking for patterns in problems, or using a tentative solution as a model for confirming the final outcome.

the teacher does not provide assistance because the experimental method dictates that the instructor is little more than a proctor. He is required only to keep discipline and order during the experiment and to operate the training devices. Carefully sequenced materials, often presented by automation, are used in the investigation of the variables under study. In terms of this kind of instructional system, the use of automation results in a more controlled experiment than if the human instructor were used.

It is possible that findings of Wittrock (27) and Twelker (23, 24) would have more nearly reflected theoretical predictions made by advocates of discovery learning had teacher-learner interactions been allowed. It is interesting to speculate on the premise that learning and transfer outcomes advocated by discovery teaching enthusiasts depend on an instructor's active role in the classroom. J. A. Easley presents a fascinating example of the importance of an instructor:

> "Charles Van Horn, a psychologist . . . observed Max Beberman teach a ninth grade algebra class for a whole year. He remarked that 'Max works on the kids until he gets them all smiling, and then he makes them frown and starts over again'" (9).

It is doubtful whether the enthusiasm sparked by this situation could be produced with programed instruction alone.

The present investigation was based upon the premise that the ingredient in many discovery teaching experiments may indeed be the "human element". A careful analysis of the learning task would indicate that the instructor may interact with the student in one of two ways that are characterized by assisting the learner in his efforts to achieve an instructional objective without revealing information that is to be discovered. One interaction technique consists of the instructor giving praise for the learner's task-related efforts. For example, if the learner shows signs of giving up prematurely, the instructor may persuade him to continue in hopes that his efforts will soon be rewarded. This help and encouragement may be provided regardless of how successful the learner is in finding the answers he is seeking. Discovery programs are difficult for many students, and to require them to study the lessons without any encouragement might be less than an optimal situation for promoting learning and transfer. Further, if the learner shows approximations to the desired behavior, the instructor may reward him for his efforts in an attempt to reinforce that behavior. In a second type of interaction, the instructor may actually give explicit instructions which tell the learner how to begin processing the information before him. These instructions would not reveal any of the information to the learner which he is seeking, but rather would channel his thinking in a way which would increase the probability of his finding the correct solution. These instructions are sometimes termed strategies or heuristics.

The purpose of the present investigation was to examine the two types of teacher-learner interactions described above. The first involved different types of information the teacher may provide about that which is to be discovered (e. g., principles, examples). The second type of interaction involved differing ways in which an instructor interacts with the learner during instruction without providing information about that which is to be "discovered" (praise and encouragement). Evidence from the study upon which the present research was based (20) seem to indicate that these two variables were most crucial in causing differential transfer effects between the experimental treatments.

Review of Related Research

The principle concern in the investigation was the effect of different types of teacher-learner interactions on learning and transfer. A reveiw of the literature on learning by discovery reveals that few studies have dealt with this class of independent variables (cf., 1, 28). Related research evidence is provided by Kersh (19) who asked subjects to learn two rules of addition, each of which was relatively novel insofar as the learners were concerned. Subjects were taught individually with a varying amount of guidance. At one extreme, the "discovery group" was instructed to find a quicker or different way to solve the problem. At the other extreme, the "rule-given" group was told the rule and instructed to practice on the examples provided. When working with a subject in the discovery group, the experimenter asked the learner to "think aloud" or to write down his responses at every step of the way. The experimenter continually gave encouragement, his main purpose being to keep the learner going in hope that he would eventually arrive at the correct solution. Immediately after instruction, the rule-given group performed better on a learning test than did the discovery group. However, after one month, the effect reversed and the discovery group was found to be superior. The delayed, superior performance of the discovery group was attributed to post-experimental practice. It was hypothesized that individuals in the discovery group were motivated to continue their efforts to learn the rules and to practice them after the formal learning period had ceased. In another experiment, this hypothesis was sustantiated by Kersh (18).

To further investigate the motivational effects of discovery teaching, Kersh (20) developed two programed instructional sequences. Both instructional sequences were designed to teach a distributive principle of arithmetic to fifth grade children. The instructional sequences were quite long and required an average of 16 hours of classroom instruction to complete. The instruction was carefully prescribed and the instructional materials were revised and evaluated several times before being used in the experiment. In the "programed discovery" treatment, every opportunity was provided the learner to discover a principle or fact, and the instructor interacted with learners individually in an attempt to reinforce searching behavior intermittently. Although the instructor controlled the operation of equipment, what he did, when, and how he did it were prescribed rather completely. In the "programed guidance" treatment, all such opportunities for searching and reinforcement were eliminated. A third experimental treatment included in the research design was intended to represent as nearly as possible an unprogramed, conventional instructional procedure similar to that which might be used by teachers trained in the UICSM or Madison Project materials. The "free discovery" procedure encouraged the subjects to ascertain principles of mathematics from concrete examples. Searching behavior by the student was reinforced whenever it was encountered. The teacher was permitted to interact with the learners freely and to use his own judgment in guiding the learning process.

The free discovery technique resulted in the shortest learning time as well as the best performance on the "searching for patterns" criterion. Quite possibly, the teacher indirectly guided the learner to criterion performance in the free discovery procedure. For example, the learner may have been told to look at a certain feature of the problem when he experienced difficulty. If this did not prove successful, the instructor may have suggested another hint. This procedure almost guaranteed that the learner would find the answer during the course of instruction. Thus, the learners in the free discovery treatment may have been given intermittent encouragement for searching as well as hints and cues that guided the processing of information given them.

The provisions of hints and cues may have enhanced the effectiveness of the free discovery treatment in several ways. First, it may have contributed to increased efficiency in terms of time spent in instruction for that group compared with the other two. Secondly, it may have increased the amount of practice and reinforcement of searching behavior. Further, it may have enhanced divergent thinking in that different strategies of searching may have been prompted. The enhancement of divergent thinking would be expected to enhance performance on the transfer test in that the behavior classified as "searching for patterns" would more likely be exhibited by these learners.

Objectives

From the findings and the reasonings reviewed above, the following hypotheses emerged: 1) Learners, who are given programs that maximize opportunities for searching behavior (Discovery Programs) and who are given verbal encouragement and praise by the instructor for exhibiting searching behavior during instruction, will be more likely to score higher on transfer tests than are learners who are given programs that do not incorporate opportunities for searching behavior (Exposition programs); and 2) Learners, who are given hints on how to process the information given them by the Discovery program, as well as verbal encouragement and praise for exhibiting searching behavior, will be more likely to score higher on transfer tests than are learners who are only given encouragement and praise.

The background evidence of the Kersh study suggests an expected ordering among the treatment effects. Such an ordering predicts that the treatment which does not encourage searching behavior would produce lowest performance, the treatment that encourages searching behavior with encouragement would be second, and the treatment that provides hints as well as encouragement would produce highest performance.

The investigation assumed that the teacher has an unique role in implementing instruction that is characterized by the learning by discovery approach. Specifically, the role of the teacher, in regard to providing encouragement and praise, was: 1) to identify approximations of the class of searching behaviors involved in the instructional objectives from among a great variety of other behaviors manifested by the learners during instruction; 2) to interact with the learners in an effort to elicit approximations of the desired behavior when that behavior was not evident; 3) to serve as a feedback channel for learners by informing them when they were showing the correct behavior or approximations thereof. Since instruction was group-paced, reinforcement was intermittent.

The role of the teacher in providing indirect guidance through hints and clues may be outlined as follows: 1) to inform the learner, when necessary, how to begin processing information given to him in instruction, as well as information derived by the learner during instruction; 2) to channel the learner's thinking in a way that will increase the probability of his finding the correct solution without giving him any part of the correct answer. For example, this was accomplished by instructing the learner to compare the problems given him, to notice similarities or differences between problems, and to hypothesize about the most important elements of the problem. Instructions such as these did not reveal anything to the learner concerning the answer he was seeking, but rather channeled his thinking in a way which increased his probability of his finding the answer. Detailed descriptions of the experimental procedures appear below.

Chapter II Method

A popular method of research for investigating problems involving complex intellectual skills is to present short instructional sequences to learners. Although this method makes it easy to identify what produces a given learning or transfer effect, the procedure is unrealistic in that generalization to conventional classroom-type instruction is difficult. In usual classroom situations, the teaching of one objective has a very definite effect on criterion performance related to other objectives. It is only when instruction is extended over prolonged periods of time that any implications of the research effort can be made to such problems as the arousing and maintaining of student interest in learning during instruction. Further, research has shown that limited exposure to discovery experiences during training may cause learners to adopt a strategy to search when confronted by transfer situations but leave them without the skill to successfully apply the searching strategy. "Educational development comes through continued instruction with intellectually significant subject matter and that is what we should investigate" (7, p. 90; also see 7 , p. 86-88). The present investigation employed instructional sequences that gave students ample practice in searching (when given the Discovery program) over an approximate three-week period.

Apparatus

The programs of classroom instruction were designed to take advantage of all of the unique capabilities of an automated classroom communication facility named the Teaching Research Automated Classroom (TRAC). This facility has been described in detail elsewhere (25). For purposes of the present discussion let it suffice to say that TRAC provides the experimenter and classroom teacher with the capability of a tutorial teacher-student relationship in group instruction through the use of an EDEX Teaching System. The system combines a completely automated student response system with an automated multi-media presentation system. The student response system was used in the present investigation to test students, to provide the instructor with information on how well the class understood the subject matter, and to stimulate active participation of the learners. The equipment consists of multiple-choice student responders located in front of each student station and meters in a teachers console that record the percentage of students answering to each choice, as well as each student's total score for a series of questions.

The automated multi-media presentation system was pre-programed to give directions to the class, lecture to them, show a slide with information or test questions, record the answers given by the individual members and score the answers, all by tape recording. This commercially available system was especially valuable in TRAC since it freed the teacher from responsibilities of displaying materials and information during instruction and allowed him to attend to individual students in accordance to the experimental treatment. The instructor was free to circulate among students and give individual help during problem-solving activities while the teaching system displayed different problems, scored student responses, and stored the information for future retrieval.

Panels between student response stations allowed the students to view stimulus materials in relative isolation from each other, and prevented possible confounding of results. This allowed the individual subject to be used as the experimental unit, rather than the small groups that were run under each treatment.

Materials

Instructional Programs. The instructional programs which were developed to teach a distributive principle of arithmetic (the "right" distributive principle of multiplication over addition with two factors) consisted of two programs incorporating differences in experimental treatment. These were designated Discovery and Exposition. These materials were adapted from special units developed by Kersh for use in the experiment on which the present study was based (20). As revisions were made in the programs, the materials were tested on small groups of students and modified until the programs were determined to be adequate for instruction.

The Discovery program whenever possible required subjects to search for principles and strategies to solve the problems, while the Exposition program presented this information to the subjects directly. In some cases, the subjects given the Discovery program were required to induce principles from a series of examples while subjects given the Exposition program were taught the principles first and then required to apply the principles to different examples. In other cases, subjects given the Discovery program were led to discover the need for principles or conventions, while subjects given the Exposition program were told directly. Generally, all experiences which were intended to foster searching behavior were omitted from the Exposition program. Information that might be "discovered" by the learner was given to him directly. An analysis of program differences as assessed by a technique for comparing verbal classroom interactions is presented in Appendix A.[2,3]

The programs were presented to the learners by means of taped instruction and slides. Instructional sessions averaged forty-five minutes in length for two to three weeks.

The revision of the Kersh materials essentially involved changes that maximized searching opportunities in the Discovery program and minimized searching opportunities in the Exposition program. It was originally thought that the learning tasks that were essential as tools for the learner to employ in his thinking with numbers, and that were not directly related to the ability of the learner to discover the distributive pattern, would be taught directly to the learner without incorporating searching sequences. This strategy was not followed after personal communication with teachers using the discovery method indicated that in order to achieve any gains at all using the discovery approach, extensive educational exposure and practice using the method is required. This evidence supported by others, such as Tuckman (22), led to the maximizing of discovery experiences wherever possible throughout the entire Discovery program. The revised program also ex-

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[2]Discovery experiences are indeed complex, and it is doubtful if researchers will ever agree on what constitutes discovery sequences. What may be "discovery" for some may be "guided" or "exposition" for others. To this end, Keislar and Shulman insist that the providing of type-scripts and other records is a valuable way to allow researchers to analyze the study being reported (14, p. 191). Type-scripts are available for the present study and may be obtained from the author to allow the reader to compare the Discovery and Exposition programs, and if desired, use the programs in further research.

[3]The "subfacts" taught in the program are shown in Appendix D.

cluded certain materials of the Kersh programs which was later used for transfer tests.

In studies on discovery learning, the researcher invariably encounters the problem of what to do with the student who does not "discover." Should the student be thrown out of the experiment, or allowed to continue with instruction? If the researcher is forced to discard many subjects in the "discovery" treatment and few subjects in the "exposition" treatment, he may, in turn, bias the experiment. (cf., 7, p. 83).

In the present study, it was felt that if at all possible no student should be unsuccessful in learning the distributive principle. If different levels of learning were permitted for each treatment, no legitimate test of transfer could be obtained without resorting to the use of analysis of covariance to statistically equate original learning. Since a measure of original learning (the concomitant variable) is affected by the treatments, the researcher in this situation must interpret the results extremely carefully. In effect, he is faced with interpreting a situation which did not occur in real life, and indeed, may never have occurred.

In order to avoid the statistical manipulation of original learning, all students who failed to pass a test frame at the end of each subfact were "branched" into a remedial loop. Students using both the Discovery and Exposition programs were branched into an instructional sequence that resembled a remedial Exposition program. It was reasoned that learning by trying to discover and failing was as much a part of discovery learning as complete success (cf., 7, p. 83-84).

Table 1 presents the number of groups using branches in each treatment for each subfact. Note that the branches were used about equally by each treatment group.

Table 1. Summary of the Number of Groups Using Branches During Instruction for Each Treatment.*

Subfacts	Treatment I	Treatment II	Treatment III
1	4	4	4
2	4	4	4
3	3	3	4
4	3	4	3
5	1	1	1
6	3	2	3
7a	4	4	4
7b	4	2	4
8	0	1	1
Total	27	25	28

*Four groups were run under each treatment.

Although every effort was made to bring all subjects up to criterion on each subfact, this strategy was not entirely successful. Some subjects still failed the general criterion test given immediately after training. Table 2 summarizes the number of subjects in each treatment who failed to meet the pre-established criterion level. These subjects were excluded from the analysis of the data.

Table 2. Summary of the Number of Subjects from Each Ability Level Deleted from the Experiment Due to Unacceptable Criterion Performance.

Ability Level	Treatment I	Treatment II	Treatment III
Low	2	5	9
Middle	1	1	1
High	0	0	0

Criterion Measures. The outcome variables included not only measures of learning but of retention, application, savings transfer, and interest. Unless otherwise noted, the tests were of the pencil - and - paper type.

The Prerequisite Skills Test, which measured entry behavior of each subject, consisted of sixteen problems involving simple manipulations of addition, subtraction, and division. These operations were considered prerequisite skills for the instructional program. Also, the test included four problems involving skills taught in the program such as using UICSM frame notation. Subjects were blocked into levels for purposes of analysis on the basis of this test. The test was given both before and after instruction.

The Subfact Criterion Tests were administered and scored by the EDEX Teaching System. The tests included an average of eight questions that tested the subjects performance on each objective that was taught.

The Review Quiz covered the first eight subfacts, and was given before the ninth subfact to determine whether subjects retained the information taught on the prerequisite subfacts. The test sampled about three problems from each of the eight subfact lessons. The quiz was administered and scored by the EDEX Teaching System.

From each of the nine subfact criterion tests, four questions were randomly drawn and used to develop two forms of the General Criterion Test. Each form used two items each from the nine subfact tests. One form of General Criterion Test was given immediately after training, and the second was given three months later as a retention test. Both tests were administered through the EDEX system.

Transfer Test I consisted of two parts. The Word Problems section involved ten problems representing the distributive principle taught, but stated in verbal form. The Distributive Examples section involved ten problems representing the distributive principle. There were five examples of the left distributive principle over two numbers, two examples of the left distributive principle over three numbers, three examples of the right distributive principle over three numbers, and one example of the right distributive principle over four numbers. The time limit for the test was about forty minutes.

Transfer Test II (Number Puzzle) consisted of sequential patterns wich were designed to test searching strategies of each subject. Three lines of a number pattern were given and the student was asked to complete the next two lines of the pattern. A time limit of approximately ten minutes was given for subjects to complete each of the five patterns. An example of a test item appears below:

> When you have found the pattern on both sides of the equal sign you are to write the next two lines which follow.

$$1 \times 8 + 1 = 9$$
$$12 \times 8 + 2 = 98$$
$$123 \times 8 + 3 = 987$$

____

____

The two Savings Transfer Tests reflect the point of view discussed by Cronbach (6, p. 123-124) that the crucial test of transfer is "how well the student can master a new mathematical topic" and "whether we have equipped him to work his way through a mass of material to assimulate it." The savings measure of transfer used in the present study was the savings in time to learn new material. The actual procedure to test savings transfer does not seem to be entirely clear. In studies of retention, the subject learns a lesson to some criterion and after an interval of time, relearns the same lesson. The savings is the difference between the original learning time and the relearning time. In measuring transfer using savings criterion, this is not feasible. A transfer measure requires a different lesson than the one initially taught. Further, measuring performance when learning involves nonsense syllable pairing is quite different from measuring performance when learning involves the distributive principle of arithmetic. Performance on the former may be measured during the presentation of material, sometimes on a memory drum, while performance on the latter must be subsequent to learning.

The crucial question in the methodology of savings transfer tests is when to measure learning. If it is measured too soon, no differential effects between treatments may be revealed. If it is measured too late in learning all groups may reach criterion with some subject having overlearned.

The present investigation adopted the following procedure. The new lesson consisted of an explanation and examples. The lesson was short, and did not go into any detail that might insure learner success on the first trial. Then subjects were tested immediately on their ability to answer test questions. The lesson was repeated, and again subjects were tested. The first savings transfer test repeated the lesson a total of four trials while the second test repeated the lesson for five trials. In each case, the measure of transfer was the total number of correct responses to the test following each exposure to the new material.

The Savings Transfer Test I (Meanings of Operations) attempted to teach subjects to discover "make-believe" mathematical operations. The lesson explained two examples of hypothetical operations. Learners were asked to practice using the operations on twelve different examples. Then five new problems were presented and subjects were required to find their meaning and apply the operations. An example of a test problem follows:

> The symbol * is to be a sign of operation. 3 * 4 tells you to operate on 3 and 4 in a certain way. It is read, "Three star four." Here are some results of the operation, star, on two numbers. Try to find the meaning of star.
>
> (a) 3 * 4 = 8 (c) 2 * 6 = 9 (e) 1 * 1 = 3
> (b) 5 * 6 = N___ (d) 2 * 7 = N___ (f) 5 * 4 = N___

The lesson was presented four times, and subjects were given five minutes to solve the five problems between lessons. The total time limit was 20 - 25 minutes.

The Savings Transfer Test II (Distributive Principle of Division over Subtraction) attempted to teach a new distributive principle to the learners and test their ability to apply the principle to new examples. The lesson was presented five times, and subjects were given five minutes between lessons to answer eleven test items. An example of a test item follows:

> As I show you a slide on the screen you are to tell me if both expressions name the same number or not. Push the button matching your choice.
>
> $(10 \div 2) - (4 \div 2)$ | $(10 - 4) \div 2$
>
> A. YES B. NO

Measures of Classroom Persistance were obtained from each subject to assess motivational effects of instruction. After formal instruction terminated, the teacher announced to the class that instruction was over and then offered learners the choice of engaging in alternative activities. For example, learners were given the choice of returning to their regular classroom or seeing some colored travel slides. On other occasions, they were asked to decide if they wanted to continue with the instruction or see some vacation slides. In this way, it was reasoned that a measure of motivation would be gained that was more generally tied to reality than attitude scales and the like.

The data from these measures are not presented in Chapter III because it was found that they were tied to reality in such a way as to be worthless. Had all groups and all treatments been run simultaneously, the measure would have been quite useful. However, since groups were run successively, the measure assessed the subject's persistance under quite different conditions. For example, when through with instruction one group returned to recess while another returned to the classroom and in fact skipped recess. Hence, the measure was related more to what followed than to what preceded the question. In order for a measure of classroom persistance to be meaningful, it must be given under similar conditions to all subjects in each treatment.

Estimates of reliability of the tests were determined wherever possible. The estimates of reliability, obtained by the split-half method and corrected by the Spearman-Brown modified formula, appear in Table 3.

Table 3. Estimates of Reliability of Selected Tests.

Test	Reliability Coefficient
Prerequisite Skills Test (Given Before Instruction)	.68
Prerequisite Skills Test (Given After Instruction)	.94
General Criterion Test (Given Three Months After Instruction)	.81
Transfer Test I, Part I	.83
Transfer Test I, Part I (Given Three Months After Instruction)	.79
Transfer Test I, Part 2	.86
Transfer Test I, Part 2 (Given Three Months After Instruction)	.87

Experimental Design

A 3 x 3 factorial design was employed in the experiment with three instructional procedures along one dimension and three levels of learner ability along the other. Learner ability was assessed by the Prerequisite Skills Test as described above. Two of the instructional modes may be thought of as representing a continium of reinforcement for searching behavior: reinforcement by encouragement only, and reinforcement by presentation of indirect guidance in addition to encouragement. The third instructional mode had no provision for searching behavior to be reinforced and directly presented the information which was to be "discovered" by learners in the other treatments.

It should be noted that three distinct variables may be identified in the instructional procedures:

(1) Type of Program: Discovery and Exposition
(2) Praise: given and not given
(3) Indirect Guidance: given and not given.

Because of the exploritory nature of the project, every combination of the three variables was not studied and only three groups were included in the design.

Treatments

The instructional phase of training for each treatment is outlined below:

I. <u>Exposition program; praise not given; indirect guidance not given.</u>

Instruction for this control group was similar to that used by Kersh (20) for the "programed guidance" group. Experiences which were intended to foster searching behavior and the reinforcing events contingent upon such behavior were omitted. Information that might be "discovered" by the learner was given to him directly. In this instructional system, the main function of the instructor was to maintain order and discipline in the class when necessary and to operate the TRAC facility. The procedure provided for a minimum of teacher-learner interaction.

II. <u>Discovery program; praise given; indirect guidance not given.</u>

This procedure attempted to prescribe the conditions under which "searching behaviors" would be emitted by the learner in his pursuit of the instructional objectives. This procedure was similar to the "programed discovery" group used by Kersh (20). The experimenter gave verbal approval to individual students for their efforts, regardless of how successful they were. The experimenter was provided with specific instructions as to the appropriateness or inappropriateness of various comments. The following statements are examples of the encouragement and praise given:

"Excellent, very good, you're starting off very well."

"Fine, keep up the good work. You show that you understand the material."

These and other statements were the same as those used by Frase (11) in a study of social reinforcers in a programed learning task. A complete list of these statements appears in Appendix B.

III. <u>Discovery program; praise given; indirect guidance given.</u>

The instructional procedure was identical to that outlined for the previous treatment with the exception that when deemed appropriate, the teacher would give explicit instructions which told the learner how to process information before him. This information did not reveal any of the information he was seeking, but rather served to channel his thinking in a way which would increase the probability of his finding the correct solution. The following statements are examples of the indirect guidance given:

"What difference do you see between these sentences?"

"How are these sentences the same?"

"Have we used the frame correctly?"

"Did you work from left to right?"

A complete list of the indirect guidance statements given appears in Appendix C.

In addition to giving information of this type, the instructor reinforced all appropriate behavior by methods outlined under Treatment II.

Subjects

The experimental subjects were selected from the fourth, fifth, and sixth grades of an elementary school in Monmouth, Oregon. Only subjects within the normal range of scholastic aptitude or higher who were performing at grade level were used in the experiment. The instructional program was developed specifically for this student population.

Assignment of students to treatments was random, with two exceptions. The first exception involved five students who were trained during the first term the experiment was conducted. Because of administrative problems, these five students were not able to participate in the treatment assigned them. After examination of pre-test scores, which indicated that all of these students were performing at essentially the same level, they were arbitrarily assigned to groups with whom they could meet. A second exception involved the summer school term. Because of administrative limitations, the experimenter could not randomly assign subjects to treatments. For this experimental period intact groups were assigned randomly to each treatment. This procedure was followed only after the school principal described the random nature of the initial assignment of the students to the groups. It is unlikely that any bias entered into the original assignment.

Several subjects who were not involved with the experiment were used solely for the purpose of evaluating and revising the instructional programs. These students did not take part in the experiment proper.

A total of 121 subjects completed training with the experimental materials. In addition to the nineteen subjects who failed to attain the minimum score on the final criterion test, eighteen subjects were dropped from the experiment because of excessive absenteeism. The ns for the experiment are shown in Table 4.

Table 4. Summary of the Number of Subjects in Each Treatment.

	Treatment		
	I	II	III
Immediate Transfer Tests	27	35	22
Retention Tests	18	31	16

Procedures

Instruction took place in the TRAC facility with small groups of about eight students each. Each instructional period lasted an average of about forty-five minutes. Six groups (late 5th graders) were run during the spring term, two groups (incoming 6th graders) during the summer term, and four groups (early 5th graders) during the fall term.

All subjects were required to complete all programs. Students who missed an instructional session were given make-up sessions at the end of the week. Subjects who missed more than three consecutive sessions were dropped from the experiment.

Chapter III Results

Methods of Analysis

The data were analyzed with standard parametric procedures such as analysis of variance and individual comparison tests. Since administrative problems made it impossible to assure that each treatment group had an equal number of subjects, the general linear hypothesis model (15, p. 234-251) was used to avoid arbitrarily eliminating subjects to equalize cell ns and to gain an accurate estimate of the main and interactional effects of the treatment variables. To gain accurate estimates of simple effects and differences between individual groups in cases of statistically significant interactions, the Newman-Keuls procedure was used (cf., 26, p. 210-211; 238-239; 80-85). In cases where significant differences were found on the individual comparison tests, the results are shown below the graph of the interaction. With one exception, all tables and graphs are presented in Appendix E.

Prerequisite Skills Test (Administered before Training)

Examination of Tables E1 and E2 shows that the treatment effects did not differ significantly from each other on this measure. On the average, subjects responded correctly to fourteen of the twenty problems. An item analysis of the test revealed that the four problems involving the skills taught in the program were among those most frequently missed. From this evidence, it was concluded that it was appropriate to present instruction on the distributive principle which assumed prerequisite knowledge and skills in handling basic mathematical operations such as multiplication and addition.

Since the subjects were grouped into three ability level groups on the basis of this test, the statistically significant Level factor shown in Table E2 is not surprising. It should be noted, however, that the three ability level groups represent a wide spread in performance on the test. The low ability level subjects correctly solved about eleven of the twenty items on the average while the high ability level subjects correctly solved about eighteen of the twenty items on the average. It could be assumed that the performance of the high ability group reflects a "ceiling effect" of the test.

Prerequisite Skills Test (Administered after Training)

The same prerequisite skills test was given after training to determine what influence the treatment had on this particular measure. Examination of Tables E3 and E4 reveals that both main effects and the interaction effect was statistically significant. Examination of Figures E1 and E2 reveals that subjects given the Discovery program (Treatment II and III) performed better than subjects given the Exposition program (Treatment I) at the low ability level ($p < .05$). There were no significant differences at the medium and high ability levels.

Prerequisite Skills Test Change Scores.

Since the identical test was used to measure the prerequisite skills both before and after training, an analysis of change scores is appropriate. A summary of the results is presented in Tables E5 and E6. These data indicate that the main effects due to Treatment and Level, and the interaction effect, were statistically significant. Figures E3 and E4 aid in the interpretation of the Treatment x Level interaction. It should be noted that the hypothesized ordering of treatments appears for the low ability level students. Subjects receiving the Exposition program (Treatment I) scored lower than subjects receiving the Discovery program with teacher-presented praise (Treatment II), while subjects who received the Discovery program with teacher supplemented praise and indirect guidance (Treatment III) scored higher than either Treatments I and II ($p < .05$).

Instructional Program

Time to Learn. The average amount of time required to complete the instructional programs for each treatment is shown in Table 5. It may be seen that groups receiving the Exposition program took about twenty-five per cent less time to learn than those groups receiving the Discovery programs. In terms of the number of instructional sessions, this difference would amount to two or three extra days spent in training using the Discovery program.

Table 5

Treatment Group Means for the Time to Learn

Treatment	Time in Minutes
I	377
II	481
III	503

Subfact Criterion Tests. Summaries of the results of each of these tests are presented in Tables E7 through E28 and in Figures E5 through E8 in Appendix E. Cursory examination of these data reveal that during the early stages of training, differential results were achieved on the tests by the treatment groups. No single treatment seemed to produce consistently high scores on the first four subtests.

On the first subfact test, a significant Treatment x Level interaction ($p < .05$) revealed that all groups performed similarly with the exception of the low ability level group of Treatment II (see Tables E7 and E8). This group performed poorer than the other groups (see Figures E5 and E6). On the second subfact test, a statistically significant main effect for the Treatment factor ($p < .05$) revealed that Treatment I produced better performance than Treatment III (see Tables E9 and E10). Treatment II produced a performance in between that of the others. On the third subfact test, Treatments I and II were found to be superior to that of Treatment III, while on the fourth subfact test, Treatments II and III were superior to Treatment I ($p < .05$) (see Tables E11 through E14).

After the fourth subfact, no statistically significant effects were noted with the exception of the last subfact. On Part 3 of the ninth subfact test, a significant Treatment x Level interaction ($p < .01$) revealed that subjects in Treatment I out-performed subjects in Treatment II at the medium ability level. (See Tables E27 and E28; Figures E7 and E8). In turn, the Treatment I and II subjects out-performed subjects in Treatment III at the medium ability level. On the other hand, subjects in Treatments I and II were out-performed by subjects in Treatment III at the low ability level.

General Criterion Test, and Review Quiz. As shown by Tables E29 through E32, no statistically significant Treatment differences were found on the review quiz that covered the first eight subfacts or on the general criterion test that covered the entire instructional program. On the General Criterion Test, the data revealed a main effect for the level factor ($p < .01$). Students at the high ability level performed better than students at either the low or medium ability levels. This effect was not found on the Review Quiz, although the tests were given within a few days of each other.

Transfer Tests

Transfer Test I, Part 1 (Word Problems). Examination of Tables E33 and E34 reveals that the main effect for Level was statistically significant ($p < .05$). Individual comparison tests showed that subjects of high ability performed significantly better than subjects of low ability. The differences between Level 2 and Levels 1 and 3 were not significant. The main effect for Treatment and the T x L interaction did not prove to be significant.

Transfer Test I, Part 2, (Distributive Examples). Data presented in Tables E35 and E36 reveal that the main effect due to Level and the T x L interaction were statistically significant. Figures E9 and E10 reveal that for subjects of high mathematical ability, it did not matter what type of program was presented. Transfer performance was high regardless of whether the subjects received the Discovery program or the Exposition program. For subjects of low mathematical ability, transfer performance was poor, regardless of what type of program was used. For subjects of average mathematical ability, the Exposition program and the Discovery program which was supplemented by teacher-presented encouragement and praise produced good transfer performance. However, when teacher-presented indirect guidance supplemented the Discovery program, performance was significantly reduced.

Transfer Test II (Number Puzzles). No statistically significant differences were recorded for this test (see Tables E37 and E38).

Savings Transfer Test I (Meanings of Operations). Examinations of Tables E39 through E48 reveal that the only statistically significant effect on each of the four trials was attibutable to the Level factor. Generally, the low ability level students performed at a poorer level than the medium and high ability level students. The Level factor was not significant on the Trial 4 - Trial 1 change score analysis.

Savings Transfer Test II (Distributive Principle of Division over Subtraction). With the exception of Trial 3, there were no statistically significant differences found (see Tables E49 through E60). On Trial 3, the Treatment x Level interaction was significant ($p < .05$). On the basis of the graphical interpretation of the interaction (see Figure E11), the results would seem to indicate that low and high ability level students performed best with the Discovery program while the medium ability level students performed best when given the Exposition program. The Trial 5 - Trial 1 change score analysis did not show significant differences at the .05 level.

Retention Tests

The General Criterion Test and Transfer Test I were given approximately three months after instruction. Of the 84 subjects used in the original analysis of the data, 65 remained in the area and were tested. Tables 61 through 72 summarize the results of these tests.

General Criterion Test. No statistically significant treatment differences or level differences were found on the test ($p < .05$), as shown by Tables E61 and E62. Also, no statistically significant differences were detected on the change score analysis (see Tables E63 and E64). It is interesting to note that subjects performed at only a slightly poorer level on the retention test than on the first test.

Transfer Test I, Part 1 (Word Problems). Examination of Tables E65 and E66 reveals that the main effect for level was statistically significant ($p < .05$). Students at the high and medium levels performed better than the students at the low ability level ($p < .05$). No significant differences were found on the change score analysis (see Tables E67 and E68).

Transfer Test I, Part 2 (Distributive Examples). Tables E69 and E70 show that the main effect for level was statistically significant ($p < .01$). Students at the high ability lever performed better than students at the medium and low ability levels (p $.05$). Further, students at the medium ability level performed better than the lower ability students. On the change score analysis (see Tables E71 and E72), a significant Treatment x Level interaction was detected ($p < .05$). On the basis of the graphical interpretation of the interaction (see Figure E12), the results seem to indicate that for subjects of low ability level, the Exposition program produced poorer performance than the Discovery program. The differences between treatment groups at the medium and high ability levels are not different enough to consider.

Chapter IV Discussion

The purpose of the present investigation was to examine two types of teacher-learner interaction during instruction that is characterized by the discovery approach: (1) teacher-presented praise and encouragement for exhibiting searching behavior, and (2) teacher-presented indirect guidance. The two hypotheses tested were: (1) learners who were given programs that maximize opportunities for searching behavior (Discovery programs) and are given verbal encouragement by the instructor for exhibiting searching behavior during instruction, will be more likely to score higher on transfer tests, in contrast to learners who are given programs that do not incorporate opportunities for searching behavior (Exposition programs); and (2) learners who are given hints on how to process the information given them, as well as verbal encouragement and praise for exhibiting searching behavior, will be more likely to score higher on transfer tests in contrast to learners who are given only encouragement and praise.

Little support was found for the first hypothesis. Where data did show Treatment II performing better that Treatment I (on the third trial of the Saving Transfer Test II, and on Transfer Test I, Part 2, given three months after instruction), the increment was obtained only with the lower ability level subjects. Further, the differences were not significant as measured by individual comparison tests. In one case, on the third trial of the Saving Transfer Test II, the evidence suggested an ordering opposite of that predicted for the middle-ability level group, but here again, the differences as measured by individual comparison tests were not significant.

In summary, little evidence was gathered to support the first hypothesis. No clear-cut evidence was found that indicated that a "discovery" program can produce better transfer of learned principles to novel problems than an "exposition" program under the conditions of the present study. Recall that Transfer Test I required subjects to apply a previously learned principle to novel problems. One might expect that the Discovery program, which involves a search-and-selection process, would have the effect of enhancing transfer for two reasons: (1) the process of dealing with concepts is better recalled from the learner's repertorie, and (2) the subject has practice in discriminating adequate from less-than adequate principles or heuristics (cf., Gagne, 12). This advantage of the discovery program might not show itself in a "near" transfer task such as the one just described. To be able to apply principles such as the one taught might be as easily accomplished by one who previously is given the principle and is required to apply it to various instances as compared with one who is required to discover the principle and apply it. However, an advantage might be evidenced if the transfer test were a "far" transfer task, or a savings test that involved novel principles, or at least novel variations of a principle. In the case of the "far" transfer test, subjects must deal with a process of handling data, and not simply applying principles. Search and selection would be most important in this case.

The expected trade-off on a saving transfer test, where subjects who had the Discovery program were expected to learn faster on new but related tasks than the subjects who had the Exposition program, did not materialize. Differences were in the right direction for the low-ability group, but in a direction opposite of that predicted for the middle-ability group.

In regard to the second hypothesis, on only one transfer test (Transfer Test I, Part 2) did the data indicate significant differential treatment effects. However, for the middle-ability level learners the direction of the difference was in opposition to the hypothesis. Several reasons could be given for these results. First, the Discovery program clearly involved the subjects in problem-solving situations where they were required to search for answers to specific problems as well as principles to solve similar problems. On the other hand, subjects who took the Exposition program were not required to search for principles or strategies inherent in using the distributive principle, but for the most part, were given problems to solve which demanded application of given principles. Examination of criterion test performance on each subfact presented during the instructional program showed essentially little difference after the fourth subfact. Up to then, performance generally was high for those using the Exposition program. Searching on the part of students using the Discovery program was something new, and it took approximately one-third of the program for them to become adjusted to this type of instruction. Recognizing that criterion test performance was not different between the experimental groups toward the end of the program, we may ask why teacher-presented indirect guidance caused the decrement in transfer test performance for the middle-level ability students. That this indirect guidance caused subject-matter confusion during learning is not substantiated upon examination of the criterion test performance at the end of the program.

This writer favors an alternative hypothesis, namely, that the guidance caused "process" confusion during learning and this in turn affected transfer. Subjects in the Discovery program who received only praise developed their own heuristics to solve the problems in the program and to discover the various principles involved. However, when teacher-presented indirect guidance (which essentially was aimed at helping the student develop their own heuristics) was introduced, it had a detrimental effect for the subjects in the low and middle level groups. It should be noted that all subjects in the low ability group performed less well than those in the high ability groupings, and teacher-presented indirect guidance as well as encouragement did not enhance performance for these students. For the middle level ability students, it clearly had a detrimental effect. Possibly, these average students were not able to cope with this supplemental information and at the same time develop their own heuristics for effective problem-solving. For the high ability students, they either were able to use the information effectively, or were able to "tune out" the teacher and disregard the information.

While the hypotheses only included the transfer tests, it should be noted that another finding involving the Prerequisite Skills Test <u>(administered after training)</u> deserves comment. A statistically significant interaction effect revealed that low ability level subjects given the Discovery program performed better than low-ability level subjects given the Exposition program. There were no significant differences at the medium and high ability levels.

Recall that the test included two types of items. Sixteen problems involved skills such as adding, subtracting, and multiplying, -- skills that were deemed prerequisite to the learning of the distributive principle taught in the program. Four problems involved the distributive principle as taught. An item analysis of each Prerequisite Skills Test item (administered after training) revealed no startling differences between error rates for either of the two groups of items. Thus, any hypothesis that attempts to explain the findings by stating that the differences resulted in differential performance on these two types of problems, is untenable.

In attempting to explain the findings, it is difficult to refrain from speculating that the subjects who performed poorly really knew how to answer the test items, but were suffering from "learning fatigue" brought about by the Exposition program. This speculation is in line with the hypothesized advantages of "discovery" learning, and disadvantages of "expositon" learning. The learner is benefited by discovery learning, so the advocates say, in that his ability to learn related materials is increased, and his interest in that activity itself is developed. A natural extention of this statement is that when students are given non-discovery type experience, motivation is thwarted and performance on related materials is reduced. If the Exposition program indeed did have a negative influence upon performance on the Prerequisite Skills Test, it is conceivable that this effect might not exhibit itself a few days ater training. In the present investigation, the test was given immediately after instruction when any negative effects could be assumed to be most powerful. Of course, it is possible that subjects who performed poorly really did not know how to perform the basic mathematical operations at the same level as the other subjects. However, this seems unlikely in view of consistent subfact criterion test performances throughout the instructional program.

Why were the differences obtained in the investigation largely statistically insignificant? Three possible explanations are: (1) the instructional programs were too short to produce consistent or significant findings, (2) since all subjects learned the distributive principle to a predetermined level, differential transfer effects were obviated from the treatments, and (3) group-paced instruction did not realize the full benefits of the Discovery program (or the Exposition program for that matter).

Just how long an instructional program that involves the discovery approach must continue before positive effects are noted is a difficult question to answer (cf., 7, p. 86). Most of the studies related to discovery learning have been conducted in a laboratory setting where the instructional programs have taken a little longer than an hour (e.g., 23, 24, 27). In other studies, instruction has lasted for six weeks or longer under classroom situations (e.g., 29). Keislar and Shulman (14; cf., 13) have suggested that experiments lasting from two to twenty weeks might be an acceptable compromise. This recommendation seems to leave much to be desired in this author's view. To date, we have little knowledge that would help us determine the optimal time and length of discovery experiences. We may not even safely conclude that "the more abundant the discovery experiences, the better the results." There is no way to determine in the present investigation how long the instructional program should have been to produce significant and consistent results.

In terms of the second factor mentioned above, it is interesting to note that in many studies, not all subjects reached the same level of performance on the learning test. What effect the treatments have on transfer when all subjects do not reach the same level of mastery of the subject matter is difficult to determine.

Some studies (e.g., 29) report only an analysis of covariance where original learning is equated statistically. Most of the earlier studies do not attempt to equate original learning at all. In one study, Twelker (23) conducted separate analyses in order to assess the effect of initial learning on transfer. It was concluded that the findings of the analysis of covariance, which equated learning statisitically, suggested that the obtained results from the analysis of variance, which did not equate original learning, were largely independent of the subject's performance on the immediate task of learning, as long as the level of learning was high. This implies that in research where initial learning is not high among all subjects, transfer effects may be due to an artifact. However, these findings may not be taken as conclusive evidence that might be generalizable to all experimental situations. Further, it certainly cannot explain the findings of the present investigation where initial learning was high and little was found in way of differential transfer effects.

Finally, the training characterized in the present investigation was designed to achieve specific objectives. The use of standardized instructional programs tended to limit each student in a group to the same instructional experience. This procedure did not capitalize on the individual strengths and weaknesses of each student. It would seem that appropriately constructed instructional programs could contain variations whose effect would be to take into account individual differences of students.

An ideal situation in education is to have the instructional program match well the characteristics of the learners -- their interests, their personality, their cognitive style, and their abilities. Most instruction in the classroom takes little account of individual differences. In the present programs, branching made it possible to "wash-back" learners who failed to pass a criterion test item at the end of the "main-stream" program, while fast learners proceded immediately to the next subfact. If the program were administered individually. However, since subjects were run in small groups, most or all of the learners were required to pass the criterion item, thus making the program, in effect, a group-paced instructional experience. Since the groups were randomly selected and heterogeneous in terms of ability, a few slow learners in a particular group could, and often were observed to, necessitate the entire group being taken through the remedial branch. Further, the Discovery program utilized large segments of sequences where subjects were required to induce principles from a large number of examples. A few fast learners caught on quite rapidly, but were forced to remain in the room throughout the lesson while the slow learners were still quite absorbed in the instructional program. In Kersh's experiment (20), subjects who discovered the solution to the problem or principle rapidly, were returned to their room. In the present investigation administrative reasons demanded that the group be kept intact. Further, since the experiment was conducted in a carefully controlled pseudo-classroom situation, every effort was made to typify instruction as it might be found in group-paced instruction. It is quite likely that these conditions contributed to the large error variances obtained in the study.

In summary, little data were gathered to support the hypotheses. It is clear, however, that any hypothesis involving discovery learning <u>must</u> consider the characteristics of the learner as well as the instructional program. This is not to say that there may not be treatments that are strong enough to show up as main effects in an experiment. Yet, all the data of the present investigation, as well as a body of data from previous studies point to just one overwhelming conclusion: the benefits of discovery learning must be viewed in light of the ability level of learners who use the program.

This implies that education may not be able to generate large numbers of generalizable principles covering a wide range of student characteristics and subject matter areas. It also implies that researchers should begin taking more time to adequately describe their experimental population so that research findings from a number of different studies may be more readily compared and synthesized.

Chapter V Conclusions and Implications

Evidence revealed that the two experimental hypotheses were inadequate to explain the results. Although many of the differences obtained in the investigation were statistically insignificant, there was evidence that indicated the following:

First, the variables of teacher-presented encouragement and indirect guidance given during pre-designed instruction that is characterized by the discovery approach do produce certain differential treatment effects (see page 26).

Second, since these differential effects are found only with certain measures, it is important that future studies consider multiple measurements. Various types of transfer should most certainly be included.

Third, since these differential effects are found only with certain classes of students, it is crucial that learner characteristics be considered in the experimental design.

Fourth, pre-designed materials that have been duly evaluated and pretested may have to be "teacher-proofed" if the expected behaviors are to be realized. This should not be construed as meaning that teachers should be excluded from the instructional system, however. Several examples will be presented below that illustrate the necessary involvement of a teacher in discovery learning and other instructional situations where complex objectives are involved.

Fifth, excessive amounts of information in the form of teacher-presented guidance that was supplementary to the pre-designed instructional program may retard transfer performance for medium-ability level students.

Sixth, no general statement may be made supporting the use of a "discovery" program as compared with an "exposition" program. Performance on either program seemed related to student ability level (see the third point above).

Three explanations were presented to account for the statistically insignificant findings found for many of the transfer measures. It should be noted that these same factors could explain <u>significant</u> findings of other studies that may be artifacts of the experimental method:

First, the instructional programs may have been too short to produce consistent or significant findings.

Second, since all subjects learned the distributive principle to a predetermined level, differential transfer effects could have been obviated from the treatments.

Third, group paced instruction did not realize the full benefits of the Discovery program (or the Exposition program for that matter).

One implication may be drawn from this investigation that overshadows all others: if supplementary information designed to help the student actually may inhibit transfer, then what is happening in classrooms every day when teachers make "on the spot" decisions with learners having difficulty or with learners simply engrossed in study? Is it possible that many teachers are guilty of overloading their students? Research on learner-controlled instruction by Mager (21) suggests that the role of information supplier to students is a need felt more by the teacher than by the student. It is a well-established fact that when too much information is presented to the learner -- that is, in human engineering terms, when signal input rate exceeds operator information-processing capacity, signals are not only unidentified but they function as a distraction.

What should be the role of the teacher in instruction, particularily that related to discovery teaching? Recall that the evidence of the present investigation indicated that instructional designers must think seriously about "teacher-proofing" pre-designed materials that they develop. The data indicated that an instructional program that is properly developed, tried out and tested, revised, and retested, and then shown to produce predictable results, could in the hands of a teacher, fail. Is this support for excluding the teacher from all discovery teaching experiences?

Kersh (16, 17) makes a case for the inclusion of an instructor in discovery teaching. He argues that there are some instructional objectives that are amenable to automated or self-instruction and there are other instructional objectives which are most readily developed through human instruction. It is suggested that involvement from an instructor may be required in the attainment of instructional objectives for one or more of the following reasons:

"(1) The required behavior cannot be identified by machine processes presently available or by the learner himself without previous instruction.

(2) The required behavior cannot be readily elicited through direct or indirect intercommunication with another person who is capable of identifying the required behavior once it has been elicited.

(3) The learner cannot determine that he is making progress toward the instructional objective independently comparing his own behavior against a behavioral standard or model."

Kersh suggests that instrucitonal objectives that involve the attainment of factual knowledge are amenable to automated instruction while objectives which involve patterns of behavior occurring at unpredictable intervals and reflecting "mediational" processes will be more readily attained through human instruction. It follows that the processes involved in learning by discovery probably are most readily attained through the use of a human instructor. For example, in Kersh's project, and in the present investigation, only an instructor could identify approximations of the class of complex behaviors called "searching behaviors" among a variety of other behaviors shown by the students during instruction. When this behavior did not appear, the instructor was expected to interact with the student in an attempt to elicit approximations of the desired behavior. Finally, only an instructor was able to present feedback to the students, either in the form of knowledge of results or approval of the students efforts.

In addition, Kersh also suggests that the provision of feedback may be a crucial factor in our choosing between automated or human instruction. When factual material is being learned, knowledge of results must be provided to the learner as he practices. Studies have shown that it is usually desirable to give an explanation for the correct answer, if practical, rather than simply telling the student that he is correct or incorrect. In the attainment of complex objectives, e.g., those that involve problem solving, hypothesis formation, searching for patterns, and so forth, feedback may be delayed for some time until the student arrives at an answer to the problem or finally formulates a hypothesis. Kersh suggests that the instructor might interact with the learner during his problem-solving activity and offer encouragement such as "Keep up the good work" or "You are doing very well" without interrupting the learner (cf., 17). The instructor would probably offer such encouragement only while the learner was exhibiting approximations to the behavior that was desired. Of course, if the learner was not behaving in the appropriate manner, the instructor could prompt the learner with suggestions that would lead him toward the use of a correct strategy without giving the answer to him directly.

Easley phrases the argument for the inclusion of the teacher in the discovery teaching situation in a somewhat different manner (9). He argues that computers are able to present experiences that approximate discovery-teaching experiences normally presented by a live instructor. However, the computer fails in the manner in which it handles the feedback function, for it cannot be programed completely to anticipate all responses from the student during instruction. Since communication must be a two-way process, a current project at the University of Illinois is attempting to couple human teachers and authorities with a computer. The coupling in fact makes the instructional system highly adaptive to non-standard student behavior. Easley further argues that a teacher is in a unique position to not only choose among several alternative learning branches of a previously developed lesson plan on the basis of an on-the-spot evaluation of learning, but he may choose to create an entirely different branch at a moments notice. This characteristic of discovery-teaching is what Easley calls "provocative feedback." "The feedback has the quality of provoking the teacher into abandoning his current teaching tactic (and oftem his strategy as well) and striking out in search of some more attractive possibilities" (9, p. 11).

The arguments presented by Kersh and Easley are complimentary. Kersh argues that no technical device yet devised can identify approximations of the class of behaviors involved in complex objectives represented by discovery learning. Once these behaviors are recognized, the actions required of the instructor in both Kersh's and Easley's point of view are the same. The instructor must decide how to respond, and then respond. The capability of the human instructor to choose new alternatives in favor of preprogramed branches makes him uniquely suitable for providing what Easley calls provocative feedback.

Other investigators have reported interesting examples of the importance of the human instructor in an instructional system. Silberman reports that in an experiment where children were isolated from each other and from the teacher in a dimly lit room and instruction was presented by a computerized system, the children would reach out to touch the experimenter as he walked about the room.[1] Apparently physical contact played an important role for the children in this man-machine instructional system where communications with the computer was impersonal and non-rewarding, at least in terms of interpersonal relations.

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[1]Personal Communication

If "teacher-proofing" of pre-designed materials is necessary, the above examples would indicate that teacher-proofing cannot mean exclusion of the teacher from the instructional system. Clearly, the benefits of an instructional strategy that utilized the teachers' unique capabilities would be all but lost. Yet, if the pre-designed instructional materials, for example, withheld certain information from the learners so that they could discover on their own certain strategies and solutions, teacher-proofing possibly could involve a training experience for the teacher in preparation to using the materials. It is obvious that such an orientation experience could never guarantee undesired teacher interference, but it would help.

It is not the purpose of this discussion to discuss the merits of completely automated instructional systems as compared with augmented human instructional systems. It is common knowledge that men are being replace by machines at a rapid pace in our society today. The computer definitely has a place in instructional systems, although the full impact of computer-assisted instruction will not be felt for quite a few years (cf., 30). Rather, the examples above are presented merely to illustrate the premise upon which the present investigation was based. That is, the learning and transfer outcomes that have been advocated by certain discovery teaching enthusiasts may depend upon supplementary information or utterances that the human instructor presents during a pre-designed course of instruction, and future research must take these variables into account.

Chapter VI Summary

Introduction

The purpose of the present investigation was to examine two types of teacher-learner interaction in discovery teaching. Both were characterized by assisting the learner in his efforts to achieve an instructional objective without revealing information that was to be discovered. The first involved the instructor giving praise for the learners task-related efforts. For example, if the learner showed signs of giving up prematurely, the instructor persuaded him to continue in hopes that his efforts would soon be rewarded. The second type of interaction involved the presenting of indirect guidance which told the learner how to process the information before him. These instructions did not reveal answers that the learner was seeking, but rather channeled his thinking in a way that would increase the probability of his finding the correct solution. The present investigation was based upon the premise that the missing ingredient in many discovery teaching experiences may be the "human element". That is, the investigation assumed that the teacher has an unique role in implementing instruction that is characterized by the learning by discovery approach. The study investigated the two variables mentioned above since they seemed to be most important in causing differential transfer effects in a previous study upon which the present one was based (20).

The two instructional modes mentioned above may be thought of as representing a continuum of reinforcement for searching behavior: reinforcement by encouragement only, and reinforcement by presentation of indirect guidance in addition to encouragement. A third instructional mode was included in the study, and made no provision for searching behavior or its reinforcement. The direct presentation of information which was to be "discovered" by learners in other treatments allowed for comparison of "discovery" and "exposition" experiences.

The following hypotheses were tested: (1) Learners, who are given programs that maximize opportunities for searching behavior (Discovery programs) and who are given verbal encouragement and praise by the instructor for exhibiting searching behavior during instruction, will be more likely to score higher on transfer tests than are learners who are given programs that do not incorporate opportunities for searching behavior (Exposition programs); and (2) Learners, who are given hints on how to process the information given them by the Discovery program, as well as verbal encouragement and praise for exhibiting searching behavior, will be more likely to score higher on transfer tests than are learners who are given only encouragement and praise.

Method

A 3 x 3 factorial design was employed in the experiment with the three instructional procedures along one dimension and three levels of learner ability along the other. Learner ability was assessed by a Prerequisite Skills Test, and subjects were divided into low, medium, and high levels. Instruction for Treatment I (Exposition program; praise not given; indirect guidance not given) was similar to that used by Kersh (20) for the "programed guidance" group. Experiences which were intended to foster searching behaviors and the reinforcing events contingent upon such behavior were omitted. The procedure provided for a minimum of teacher-learner interaction. Instruction for Treatment II (Discovery program; praise given; indirect guidance not given) attempted to describe the conditions under which "searching behaviors" would be emitted by the learner in his pursuit of the instructional objectives. This procedure was similar to the "programed discovery" group used by Kersh. The experimenter gave verbal approval to individual students for their efforts regardless of how successful they were. Instruction for Treatment III (Discovery program; praise given; indirect guidance given) was identical to that outlined for Treatment II with the exception that when deemed appropriate, the teacher would give explicit instructions which told the learner how to process information before him.

The present investigation employed instructional sequences that gave students ample practice in searching (when given the Discovery program) over an approximate three-week period. Instructional sessions averaged forty-five minutes in length. Programs taught a distributive principle of arithmetic, and were presented to the learners by means of taped instruction and slides. All students who failed to pass a test frame at the end of each subfact in either of the two programs were "branched" into a remedial loop. The outcome variables included not only measures of learning but of retention, application, savings transfer, and interest.

The subjects were selected from the fourth, fifth, and sixth grades, and assigned at random to the treatments. Instruction took place with small groups of about eight students each.

Results

The data were analyzed with the standard parametric procedures of analysis of variance and individual comparison tests. Little support was found for the first hypothesis. Where data did show Treatment II performing better than Treatment I (on the third trial of the Savings Transfer Test II, and on Transfer Test I, Part 2, given three months after instruction), the increment was obtained only with the lower ability-level students. Further, the differences were not significant as measured by the individual comparison tests. In one case, on the third trial of the Savings Transfer Test II, the evidence suggested an ordering opposite of that predicted for the middle ability-level group, but here again, the differences as measured by individual comparisons tests were not significant. In summary, little evidence was gathered to support the first hypothesis. No clear-cut evidence was found that indicated that a "discovery" program which involved praise but no guidance could produce better transfer of learned principles to novel problems than an "exposition" program under the conditions of the present study.

In regards to the second hypothesis, on only one transfer test (Transfer Test I, Part 2) did the data reveal significant differential treatment effects. However, for the middle ability-level learners, the differences were in a direction opposite to that stated in the hypothesis. When teacher-presented indirect guidance (which essentially was aimed at helping students develop their own strategies) was introduced, it had a detrimental effect on the middle ability-level students. Teacher-presented praise had no such detrimental effect.

Three possible explanations were given for the largely insignificant differences obtained in the investigation: (1) the instructional programs were too short to produce significant findings, (2) since all subjects learned the distributive principle to a predetermined level, differential transfer effects were obviated from the treatments, and (3) group-paced instruction did not permit the realization of the full benefits of the Discovery program (or the Exposition program for that matter).

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Appendix A[1]

Analysis of Program Differences

Introduction

An analysis of the interacts between presenter and learner via the Discovery and Exposition programs was made to identify and describe the function of each message given in the two instructional programs.* In writing this report, it was felt that an analysis of the kinds of interaction taking place between "presenter" and learner, e.g., discovery-type messages vs. exposition-type messages, would be useful in comparing differences between the Discovery and Exposition programs. The Teaching Research Classroom Interaction Analysis System (TR System) was used for the analysis. A brief description of the TR System, how it was employed, and the results obtained from the analysis will be presented here.

Specifically, in the TR System, an effort has been made: (1) to tie the system conceptually to that which is known about the cognitive development-teaching-learning process, (2) to make it inclusive of both the instructional and the management parameters of teaching, (3) to provide in it for the detailed description of both teacher and learner interaction behavior, (4) to use as a data base both the verbal and non-verbal aspects of teacher-learner interaction, and (5) to conceptualize teaching behavior so as to make the system applicable across a wide range of ages and settings, e.g., the home or nursery school, the play ground or classroom, the elementary or the secondary school. In addition, the TR System provides a detailed record of the setting variables which influence teacher and/or child behavior, e.g., a running record of the activity in which a class is involved, the characteristics of the children in the class, the physical characteristics of the classroom, and the occurrance of unusual events which vary the ordinary routine of a classroom. In short, the observation system represents an attempt to develop a means of looking at teaching behavior wherever and whenever it occurs and to describe it as occurring in relation to the full range of factors

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[1]Prepared and written by Sidney S. Micek

*An interact is generally defined as a message sent by a person to influence another.

which influence it.*

Description of the Programs

In the present study, two trained observers using the TR System coded the Discovery and Exposition programs. Although the TR System is designed to be used in face-to-face observations, the transition by the observers to recording the interaction between presenter and learner in script-form was made without any problem. Since the topic and activities in which the class was involved were primarily constant throughout the eighteen subfacts, no record of setting variables was kept.

Classes of Behavior. In the two instructional programs, the presenter always focused (i.e., directed attention) on the cognitive development of the learner. Two classes of behavior were elicited by the presenter: (1) direct influence behavior, and (2) indirect influence behavior. Broadly defined, influence behavior is that which one person directs to another or a group of others which has as its intent the shaping or modification of the behavior of another. In this sense, direct influence behavior is that behavior which promotes the actual learning of knowledge and the intellectual development of the learner, which is the objective in the lesson taught by the presenter. This is opposed to indirect influence behavior, which facilitates the actual learning activity by organizing the activity in some way so that direct influence behavior can take place.

Table 1 is provided to allow the reader to see for each subfact the number of times the presenter was fixed on either a class of direct influence behavior or an indirect class of influence behavior. The frequency counts in columns 1 and 2 identify how often in each subfact the presenter was involved in direct and/or indirect influence behavior. In each subfact in the two instructional programs, the presenter always sent more messages having a direct influence nature. This varied from 63 direct influence behaviors and 9 indirect influence behaviors in Subfact 9 of the Discovery program to 9 direct influence behaviors and 8 indirect influence behaviors in Exposition Subfact 2. The cumulated total of direct influence and indirect influence behavior messages for the Discovery program outweigh the total for the Exposition program. However, proportionally the two instructional programs compare almost identically in terms of direct and indirect classes of influence behavior (see Figure 1).

*A detailed description of the TR System can be obtained by writing Teaching Research, Monmouth, Oregon.

Table 1. Individual Subfact Frequency Counts of Teacher Messages Sent By Class of Influence Behavior, Instructional Component, and Instructional Tactic In the Exposition and Discovery Programs.

Exposition

SUBFACT	Direct Influence Behavior	Indirect Influence Behavior	Exposure to Information: Exposition (Direct Influence) E	Exposure to Information: Exposition (Indirect Influence) E'	Inquiry (Direct Influence) W	Precipitation of Performance: Inquiry (Indirect Influence) W'	Precipitation of Performance: Direction (Direct Influence) D	Direction (Indirect Influence) D'
	1	2	3	4	5	6	7	8
1	10	10	8	2	1	0	1	8
2	9	8	9	1	0	0	0	7
3	21	12	13	1	8	0	0	11
4	13	6	9	1	4	0	0	5
5	12	5	10	0	2	0	0	5
6	14	7	12	0	2	0	0	7
7	37	10	23	1	13	0	1	9
8	19	11	17	1	1	0	1	10
9	20	9	15	3	5	0	0	6
Total	155	78	116	10	45	0	3	68

Table 1. Continued.

SUBJECT	Direct Influence Behavior	Indirect Influence Behavior	Discovery: Exposure to Information: Exposition (Direct Influence) E	Exposition (Indirect Influence) E'	Inquiry (Direct Influence) W	Precipitation of Performance: Inquiry (Indirect Influence) W'	Direction (Direct Influence) D	Direction (Indirect Influence) D'
	1	2	3	4	5	6	7	8
1	12	19	7	8	2	0	3	11
2	12	16	1	9	11	0	0	7
3	18	8	8	5	10	0	0	3
4	38	21	17	8	14	0	7	13
5	13	6	10	1	3	0	0	5
6	22	15	13	0	9	0	0	15
7	67	25	41	4	21	0	5	21
8	27	7	17	0	9	0	1	7
9	63	9	49	1	12	0	2	8
Total	324	140	181	39	109	0	34	101

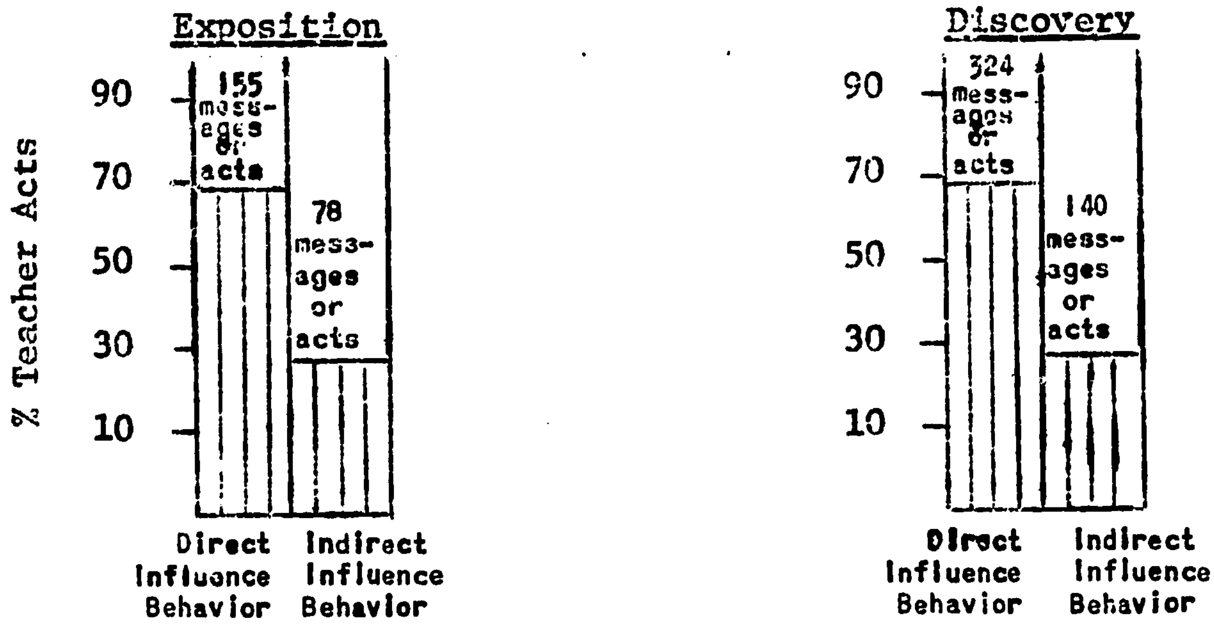


Figure 1. Proportion of Teacher Acts Within Teacher's Cognitive Development Forms.

Instructional Components and Tactics. After determining that the focus or domain of each instance of the presenter's behavior was in the cognitive area, and after identifying the class of teaching behavior exhibited, two other kinds of information were obtained about the value of each instance of behavior: (1) the instructional component that the behavior act represents (that is, does it expose information, does it precipitate a response, or does it serve as an evaluation of a response) and (2) the teaching "method" or teaching "tactic" which it represents (for example, is it a lecture or a demonstration, is it a request for a note or an applied response, is it in the form of positive or negative feedback?) Table 2 discusses each component and tactic as they are used in the TR System.

Table 2. Instructional Operations: Components and Tactics Within Components.

Instructional Operations

Component I: Exposure to Existing Knowledge

Tactic R: Confrontation with an Instance of Reality. Defined as any exposure to objects, events, or processes in their natural or "real" state. This includes exposure through observation, manipulation, exploration, experimentation, etc. Some examples are everyday household objects for an infant, displays of real objects for school children, e.g., fruit, automobiles, chickens hatching, animals mating, a display of leaves, visitations by persons of differing nationalities, races or religion, etc. Exposure to numbers, letters and/or words as realities in themselves is included within this category.

Tactic M: Confrontation with a Model. Defined as any exposure to replicas of the real world, e.g., plastic models of the human body, film strips showing a forest fire, line drawings of an historic building or event, etc.

Tactic E: Exposition. Defined as an exposure to existing knowledge via symbolic means, either verbal, written, or numerical. Thus lecture, discussion, non-illustrated written material, radio, etc. represent instances of exposition. Use of a blackboard by a teacher may or may not fall into this category: if she is simply using it as a substitute for the printed page, then it is an instance of exposition. If she is sketching on it, however, or if she is illustrating how to make a letter or a number, then her behavior is classified as an instance of modeling and an instance of reality confrontation respectively.

Component II: Precipitation of Performance

Tactic W: Through a Question. Defined as an invitation to a child to respond. (Note: this is a "genuine" question in the sense that is "asks" for information or the pursuit of a line of reasoning. Often a teacher will employ a question form grammatically when she is in fact directing a child to do something, e.g., "Would you read now Billy?" When this is the case the interact is recorded as representing a direction rather than a question.

Tactic D: Through a Direction. Defined as a demand without censorship, for a child to respond. The demand may be either straightforward, e.g., "Read the next paragraph Billy.", cushioned, e.g., "You may not know all of the words, but would you try the next paragraph Billy?", with an explanation, e.g., "It's your turn next; would you read Billy?", or quite indirectly, e.g., "Next."

Component III: Evaluation

Tactic A: Through Positive Reinforcement. Defined as any instance of "reward", e.g., the granting of a priviledge, the awarding of an "award", e.g., a gold star or a kiss, or verbal or symbolic recognition, e.g., "Fine", "Good", "Well done", or a mark of 100, etc. Encouragement is considered as a generative operation.

Tactic N: Through Negative Reinforcement. Defined as any instance of censorship or punishment. Censorship is defined operationally as any instance in which the academic performance of a child is indicated as being changeworthy, e.g., "No. That's not right.", "Are you sure that is correct?", You need to do that over."

In the analysis of the subfacts, the teacher's behavior in terms of instructional component, enhanced the learning process by either exposing the learner to information or by precipitating performance of the pupil. The method or tactic used in exposing to information was always by exposition or telling, i.e., lecture. There was never any exposure to knowledge by showing or demonstration. Precipitating a response from the learner was either attempted through inquiry or direction. The evaluative instructional component was obviously not present in the scripts.

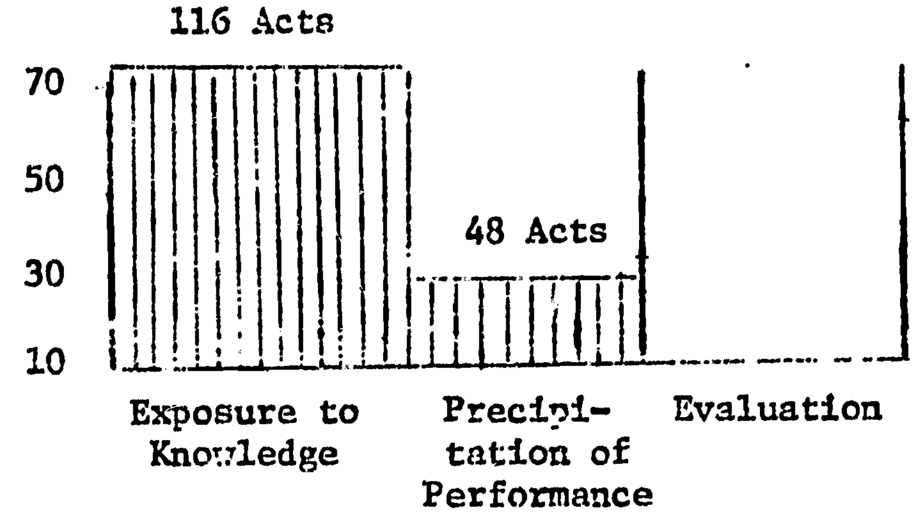
In columns 3 to 8 of Table 1 are found the frequency counts of the tactics used in each component of instruction. The numbers in columns 3, 5, and 7 represent the number of times that tactic was used by the presenter when eliciting direct influence behavior, and the numbers in columns 4, 6, and 8 indicates how often the tactic was used in indirect influence behavior. For example, in

Subfact 1 of the Exposition program, 10 messages were sent as direct influence behavior with the presenter's focus on the connitive domain. Of the tactics used, exposition occurred a total of 8 times and inquiry occurred once as did the tactic of direction. Indirect influence behavior messages occurred 10 times in Subfact 1. Two of the 10 indirect influence behavior messages were exposition messages and the other 8 were direction messages.

In the two instructional programs, messages being sent to the learner by the presenter most often involved exposure to information by the lecture method. This is indicated in the frequency counts identifying the tactic used in each program. When inquiry was utilized by the presenter to precipitate performance, in the subfacts of both programs it always dealt with the direct class of influence behavior. Therefore, when a question was asked of the learner it always was intended for pure intellectual development and in no way was ever intended to facilitate the learning process by organizing or preparing for the learning activity. When a direction to do something was given to precipitate performance, e.g., "give me the answer" or "take out your books", it generally occurred for the purpose of facilitating the learning activity.

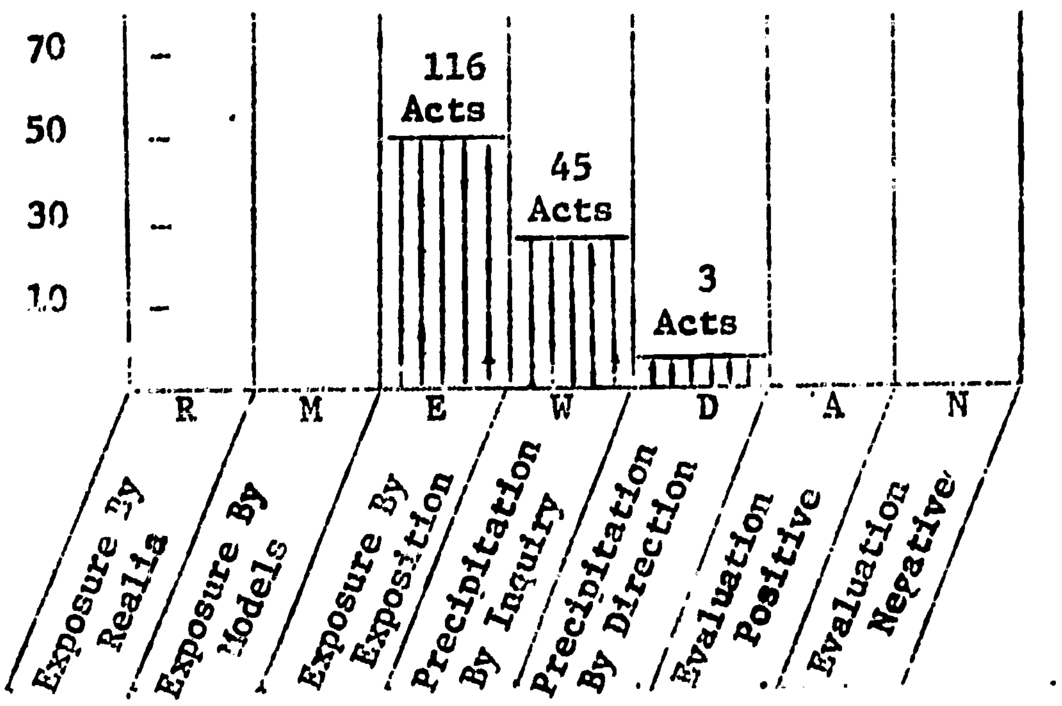
In looking at the proportion of total presenter acts by instructional components and instructional tactic used for the Exposition and Discovery programs, precipitation of the learner to respond was greater in the Discovery program as compared to the Exposition program. The information from Figures 2 and 3 enables the reader to compare the two programs in relation to instructional components and tactics. Figure 2 illustrates the proportion of components and tactics used in instruction (direct influence behavior) for the Exposition instructional program and Figure 3 gives the same information for the Discovery program. In looking at the proportion of teacher acts by instructional components Figure 2 shows that the presenter instructed the learner in the Exposition progtam primarily by exposure to information. The bar graph identifies that 70% of the teacher acts (messages) were of this instructional component and only 30% of the teacher acts were for precipitating learner performance. The second bar graph in Figure 2 gives a breakdown of the proportion of teacher acts by the instructional tactics used in each instructional components.

Proportion Of Teacher Acts By Instructional Component



Instructional Components

Proportion Of Teacher Acts By Instructional Tactics



Instructional Tactics

Figure 2. Components And Tactics Used In Instruction In The Exposition Program

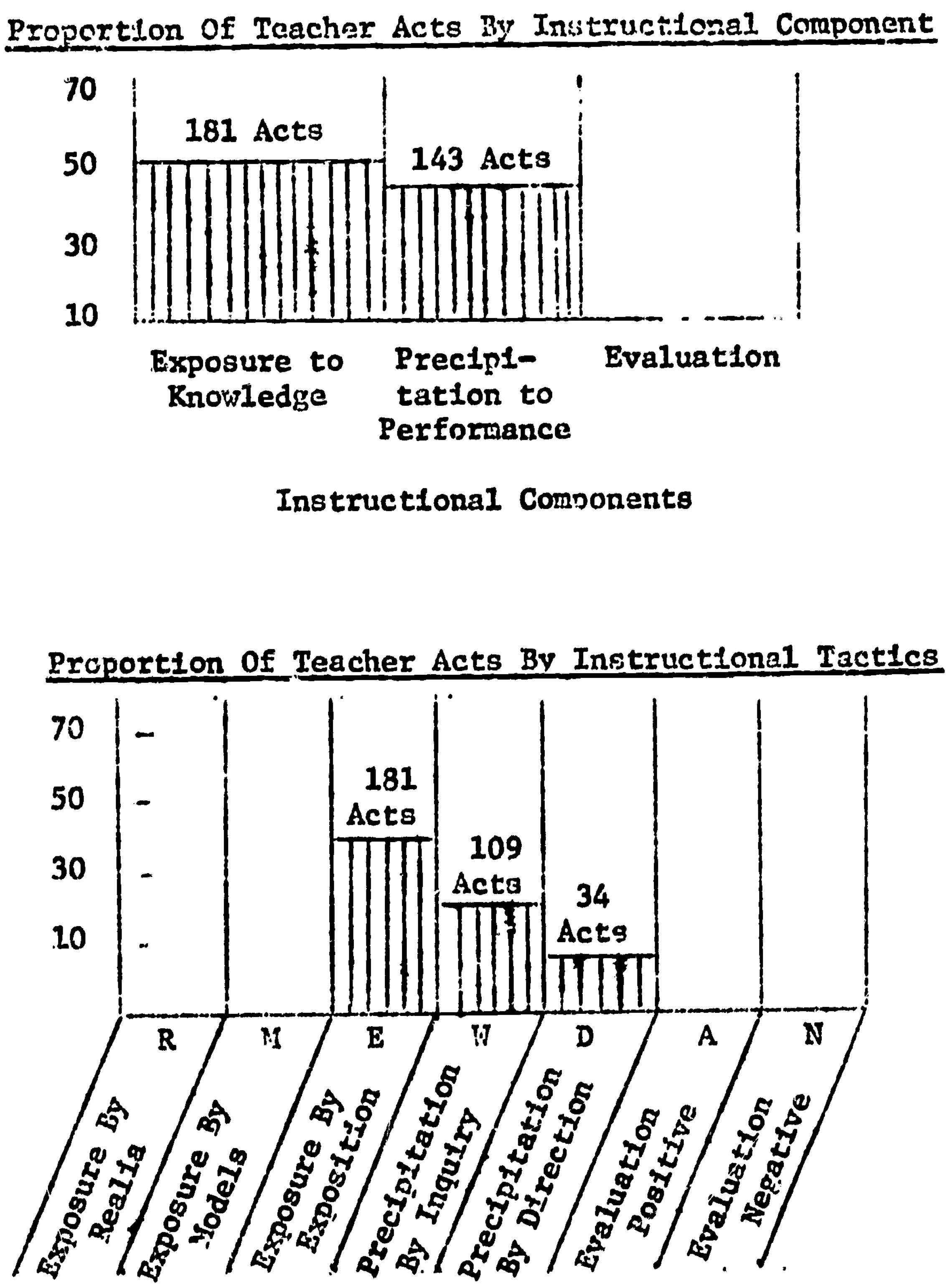


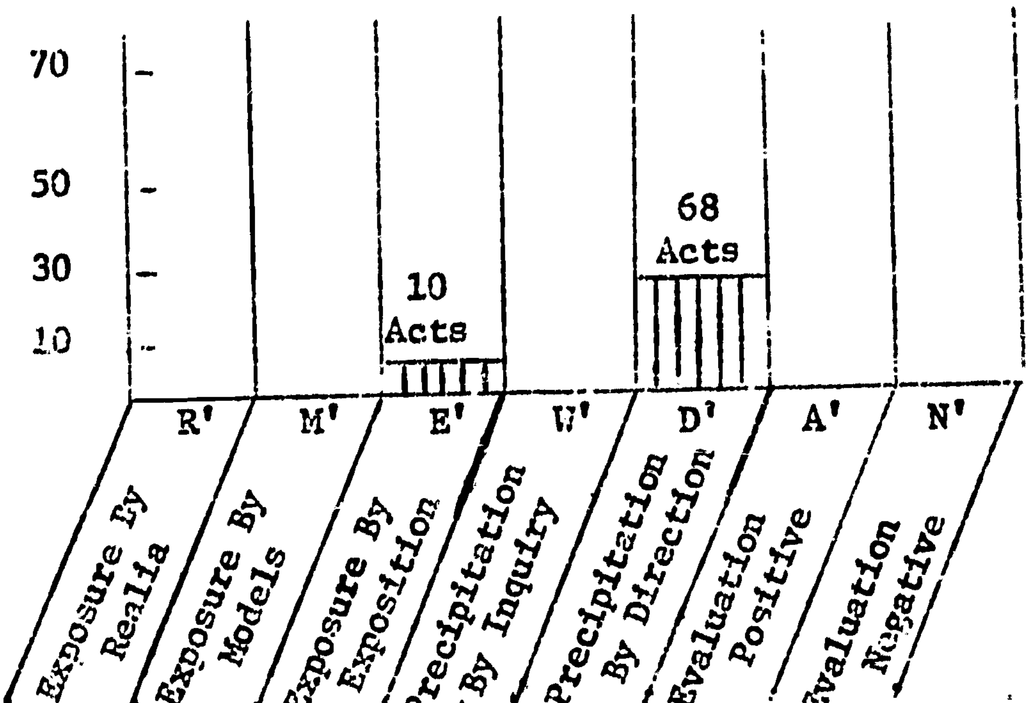
Figure 3. Components And Tactics Used In Instruction In The Discovery Program.

In comparison the information in Figure 3 shows that in the Discovery program the amount of exposure to knowledge and precipitation of performance was almost proportionally equal. This points out that the presenter in the Discovery program called on the learner to perform proportionally much more than did the Exposition presenter. If getting the learner to perform is valued as an objective, then the Discovery program outweighs the Exposition program in this aspect. The second bar graph in Figure 3 identifies the proportion of teacher acts by instructional tactic for the Discovery program.

Figure 4 identifies the proportion of teacher acts by the tactic used in managing for or facilitating the instruction (indirect influence behavior). Proportionally the tactics, used for facilitating the learning activities, are virtually the same in both instructional programs.

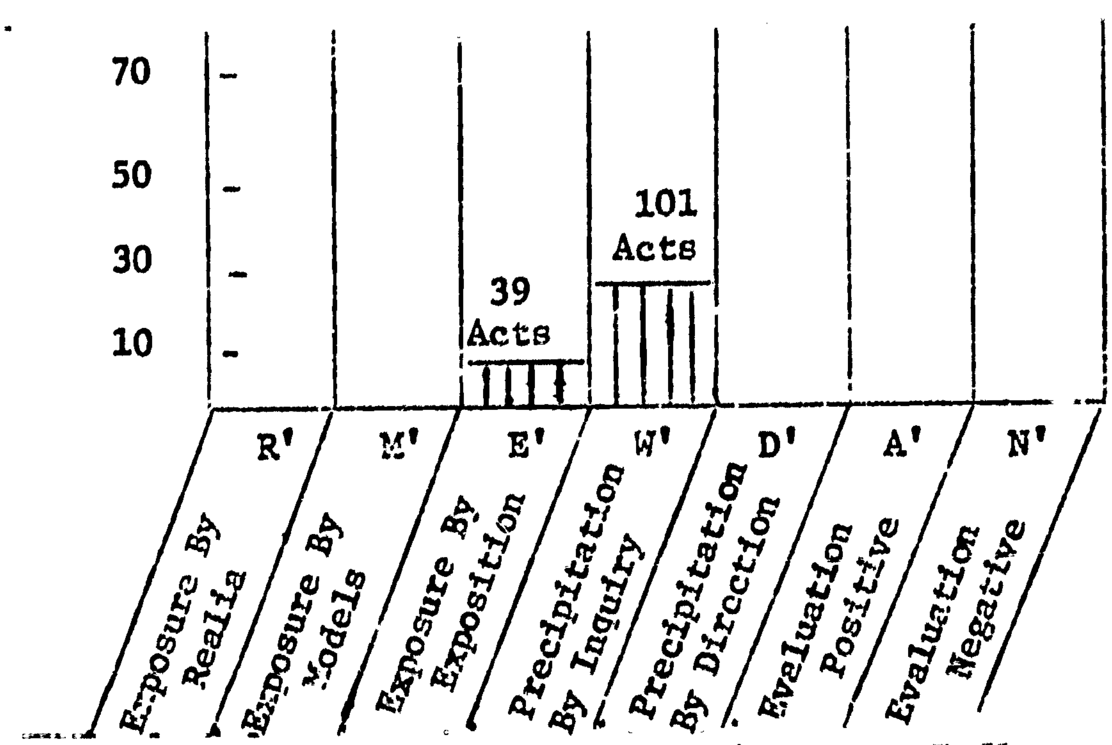
The analysis of the Discovery and Exposition programs has been presented to give the reader further descriptive information on the content of the two instructional programs. If a conclusion would be made from what was found, one would have to say that the Discovery program did precipitate learner performance proportionally much more than did the Exposition program during actual instruction where direct influence behavior occurred.

Proportion Of Teacher Acts By Tactics Used
In Managing Instruction In The Exposition Program*



Tactics Used To <u>Manage</u> Instruction (Indirect Influence Behavior)

Proportion Of Teacher Acts By Tactics Used
In Managing Instruction In the Discovery Program*



Tactics Used To <u>Manage</u> Instruction (Indirect Influence Behavior)

Figure 4. Proportion Of Teacher Acts By Tactic Used In The Exposition And Discovery Programs.

* The prime accompanying each letter symbol represents tactic was used to facilitate instruction by management.

Appendix B

Teacher-Learner Interactions: Praise Statements

The following statements were used to reinforce searching behavior in subjects in Treatments II and III. These statements are taken from Frase (11).

Very good! You're starting off very well.

Excellent, keep up the good work.

That's fine, you seem to understand the materials.

That's right! Very good.

Yes, you are quite right. You are doing very well.

Yes, very good.

Excellent, you are coming along fine.

Right again! You have demonstrated that you have mastered this introductory material.

Very good.

Right! Keep it up.

Very good, now you have it.

You are doing quite well with this more difficult material.

Very good. Your work is coming along very well.

You are right again. Very good.

That's it. Keep it up.

Good. Keep up the fine work.

Fine. You show that you understand the material.

Very good. Keep it up.

Excellent, you are doing very well.

You are right again, and the material is not simple now, by any means.

That's it. You have come along way.

Right again. Now you understand the materials quite thoroughly.

Right on the button!

That's it! Keep it up.

Excellent. You are making progress.

Very, very good.

Very good, indeed.

You are doing fine, so far.

Fine. You are doing quite well.

Right, and the material is fairly difficult.

Fine work. You are correct.

Appendix C

Teacher-Learner Interactions; Indirect Guidance

The following statements are examples of indirect guidance given to subjects in Treatment III.

Subfact 1

1. Remember, we are looking for the collection of symbols that make sense.

2. Which one is a complete English sentence?

3. Does this sentence make sense? (Point to specific multiple choice)

4. Which one of these does not make sense?

5. What do these sentences have in common with these?

6. What mathematical mark do these sentences have that the others don't?

Subfact 2

1. What difference do you see between these sentences?

2. What do you think is different about this sentence?

3. Read this sentence very carefully and tell me what you think about it.

4. Why do you think that statement is different?

Subfact 3

1. How is this one different from these?

2. How are these sentences the same?

3. Could you find other numbers that make this statement true?

4. Could you find other numbers that make this statement false?

Subfact 4

1. Do these name the same number on each side?
2. What are some of the other names for that number?
3. What are some other numbers that will make this a false statement?
4. Can you find any more numbers that make this a false statement?

Subfact 5

1. Are the frames the same shape?
2. Are the frames different shaped?
3. Do the frames contain the same numeral?
4. Do the frames contain different numerals?

Subfact 6

1. Have John and Mary used the frames the same?
2. What are John and Mary doing differently?
3. What do you think the rule is?
4. Can you think of any other rule?
5. Why is this sentence incorrect?

Subfact 7

1. Have we used the frames correctly here?
2. Are the frames filled the same?
3. Are the frames filled with different numerals?
4. Did you work from left to right?
5. Did you work from right to left?

6. Did you do the addition first, then multiplication?

7. Did you do the multiplication first, then addition?

8. Can you tell me which way this problem was worked?

9. Can you tell me the rule?

Subfact 8

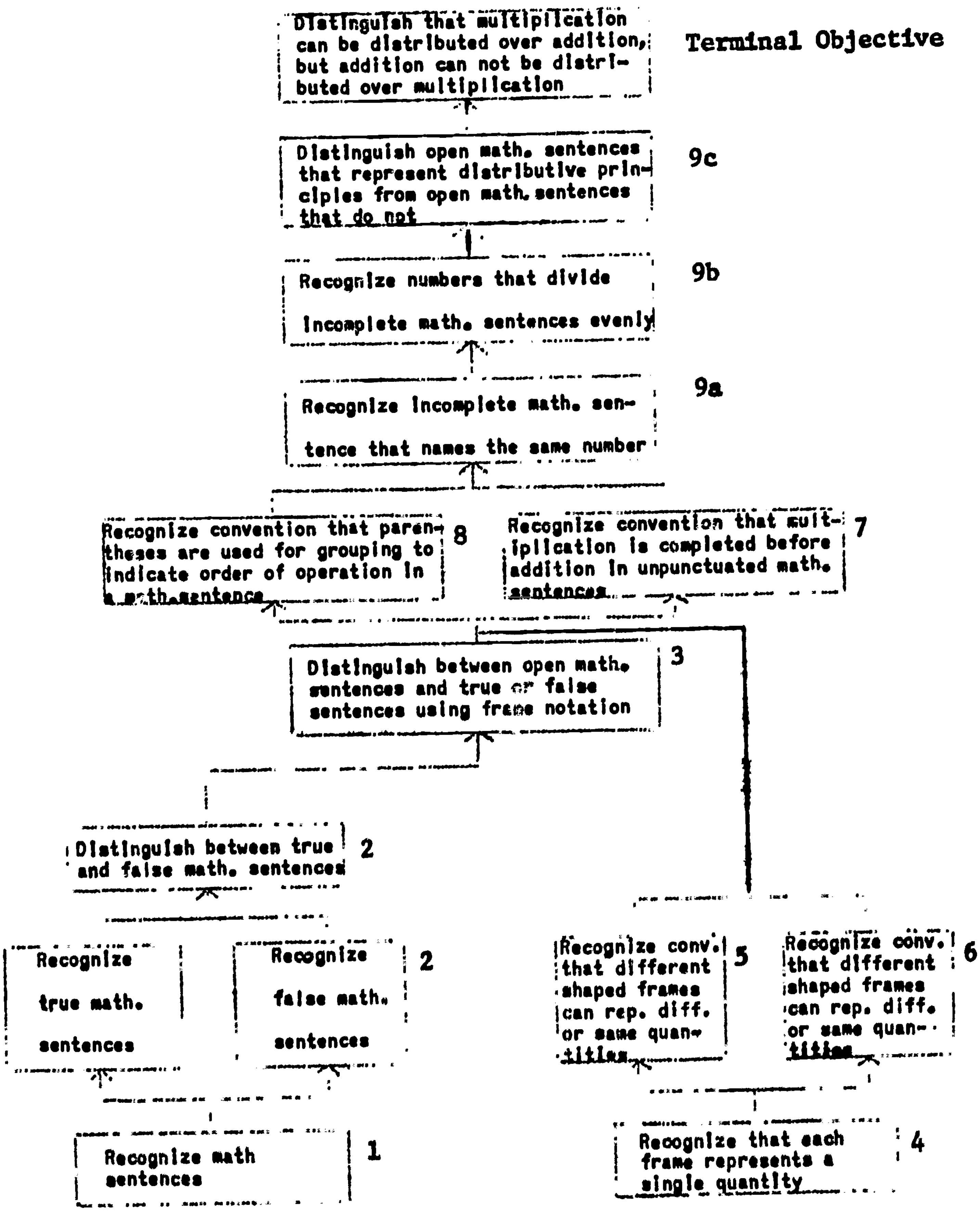
1. Did you remember to multiply, then add?

2. Did you work what was in the parentheses first?

3. Which example is right?

Appendix D

Subfacts Taught in Instructional Programs



Appendix E

Tables and Graphs

Table E1

Treatment Group Means for the
Prerequisite Skills Test, (Administered Before Training)

	Level*			
Treatments	1	2	3	Combined
I	11.86	15.17	17.25	14.93
II	10.85	15.56	17.85	14.66
III	9.00	15.29	17.57	13.73
Combined	10.57	15.32	17.61	

Table E2

Summary of the Analysis of Variance for the
Prerequisite Skills Test, (Administered Before Training)

Source	df	MS	F
Treatment (T)	2	5.14	1.94
Level (L)	2	333.02	125.65 **
T x L	4	6.05	2.28
Error	75	2.65	———

** $p < .01$

Table E3

Treatment Group Means for the
Prerequisite Skills Test, (Administered After Training)

	Level			
Treatments	1	2	3	Combined
I	13.43	17.83	18.50	16.89
II	16.54	18.89	19.31	18.17
III	16.25	17.14	17.71	17.00
Combined	15.68	18.00	18.68	

Table E4

Summary of the Analysis of Variance for the
Prerequisite Skills Test, (Administered After Training)

Source	df	MS	F
Treatment (T)	2	22.04	7.25 **
Level (L)	2	71.58	23.54 **
T x L	4	8.30	2.73 *
Error	75	3.04	-----

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

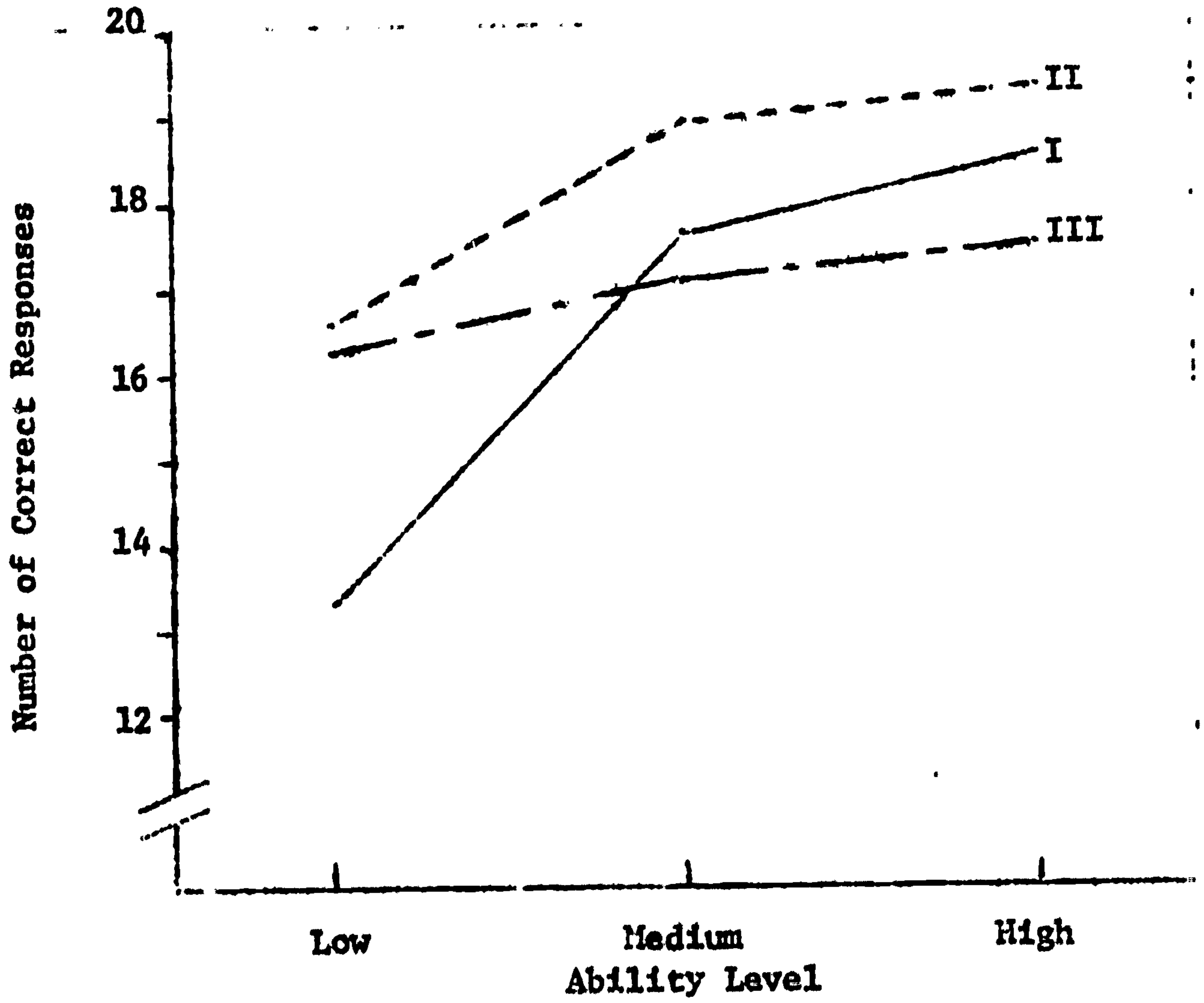


Figure E1. Profiles of Means Showing the Treatment x Levels Interaction. The Dependent Variable is the Prerequisite Skills Test (Administered After Training)

Ability Level	Treatment Comparison I-II	I-III	II-III
Low	*	*	
Medium			
High			

Figure E2. Summary of Individual Comparison Tests. Statistically Significant Difference Between Treatments ($p < .05$) are Shown by Asterisks.

Table E5

Treatment Group Means for the Prerequisite Skills Test, Change Scores

	Level			
Treatments	1	2	3	Combined
I	1.57	2.67	1.25	1.96
II	5.69	3.33	1.46	3.51
III	7.25	1.86	.14	3.27
Combined	5.11	2.68	1.07	

Table E6

Summary of the Analysis of Variance for the Prerequisite Skills Test, Change Scores

Source	df	MS	F
Treatment (T)	2	21.02	6.08 **
Level (L)	2	99.24	28.73 **
T x L	4	28.32	8.20 **
Error	75	3.45	----

** $p < .01$

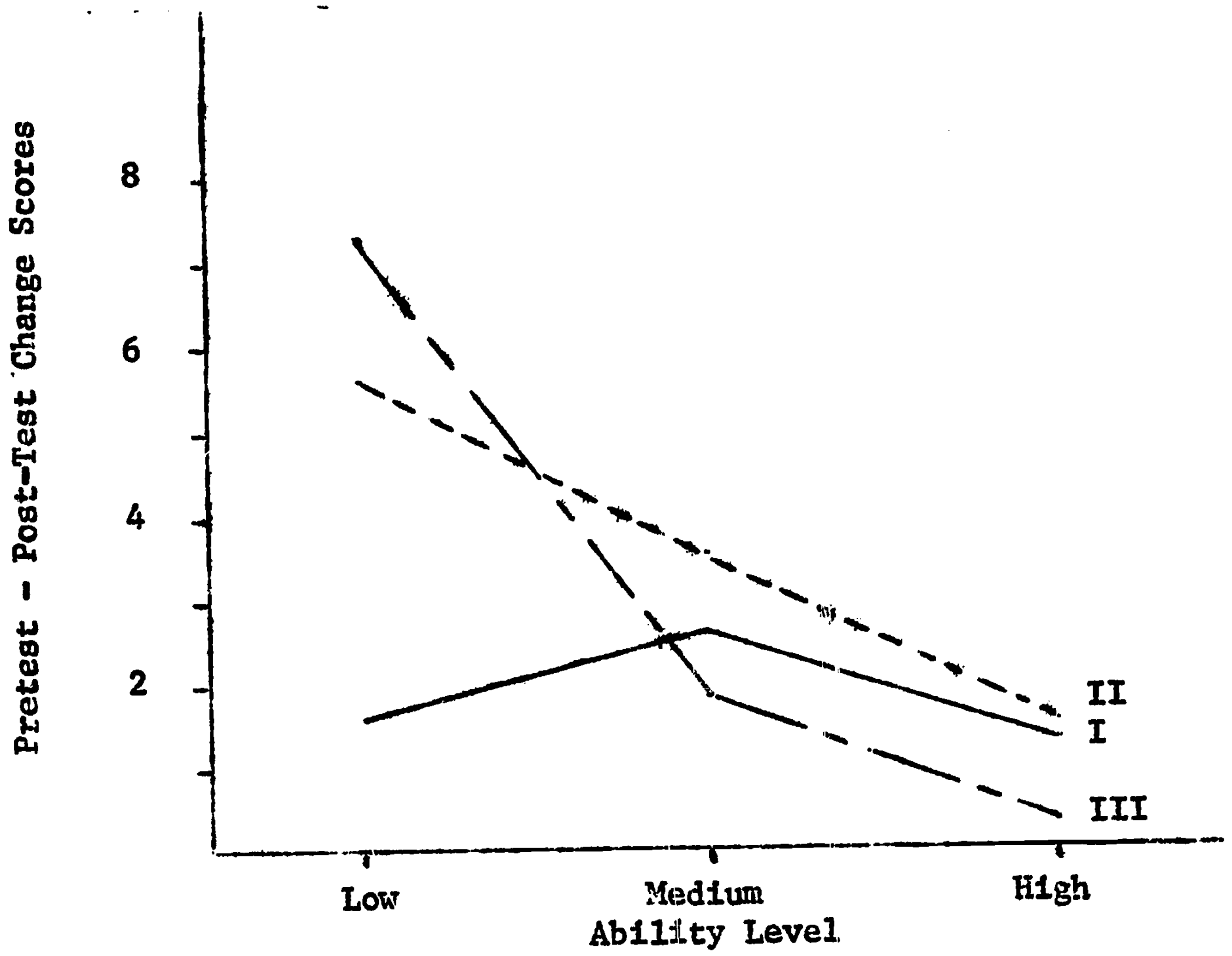


Figure E3. Profiles of Means Showing the Treatment x Levels Interaction. The Dependent Variable is the Prerequisite Skills Test Change Scores.

Treatment Comparison

Ability Level	I-II	I-III	II-III
Low	*	*	*
Medium			
High			

Figure E4. Summary of Individual Comparison Tests. Statistically Significant Differences Between Treatments ($p < .05$) are Shown by Astericks.

Table E7

Treatment Group Means for the Criterion Test, Subfact 1

Treatments	Level 1	Level 2	Level 3	Combined
I	40.00	39.58	38.13	39.26
II	34.31	38.89	39.62	37.32
III	38.13	40.00	40.00	39.32
Combined	36.82	39.46	39.29	

Table E8

Summary of the Analysis of Variance for the Criterion Test, Subfact 1

Source	df	MS	F
Treatment (T)	2	28.62	2.32
Level (L)	2	31.68	2.57
T x L	4	32.69	2.64 *
Error	75	12.31	----

* $p < .05$

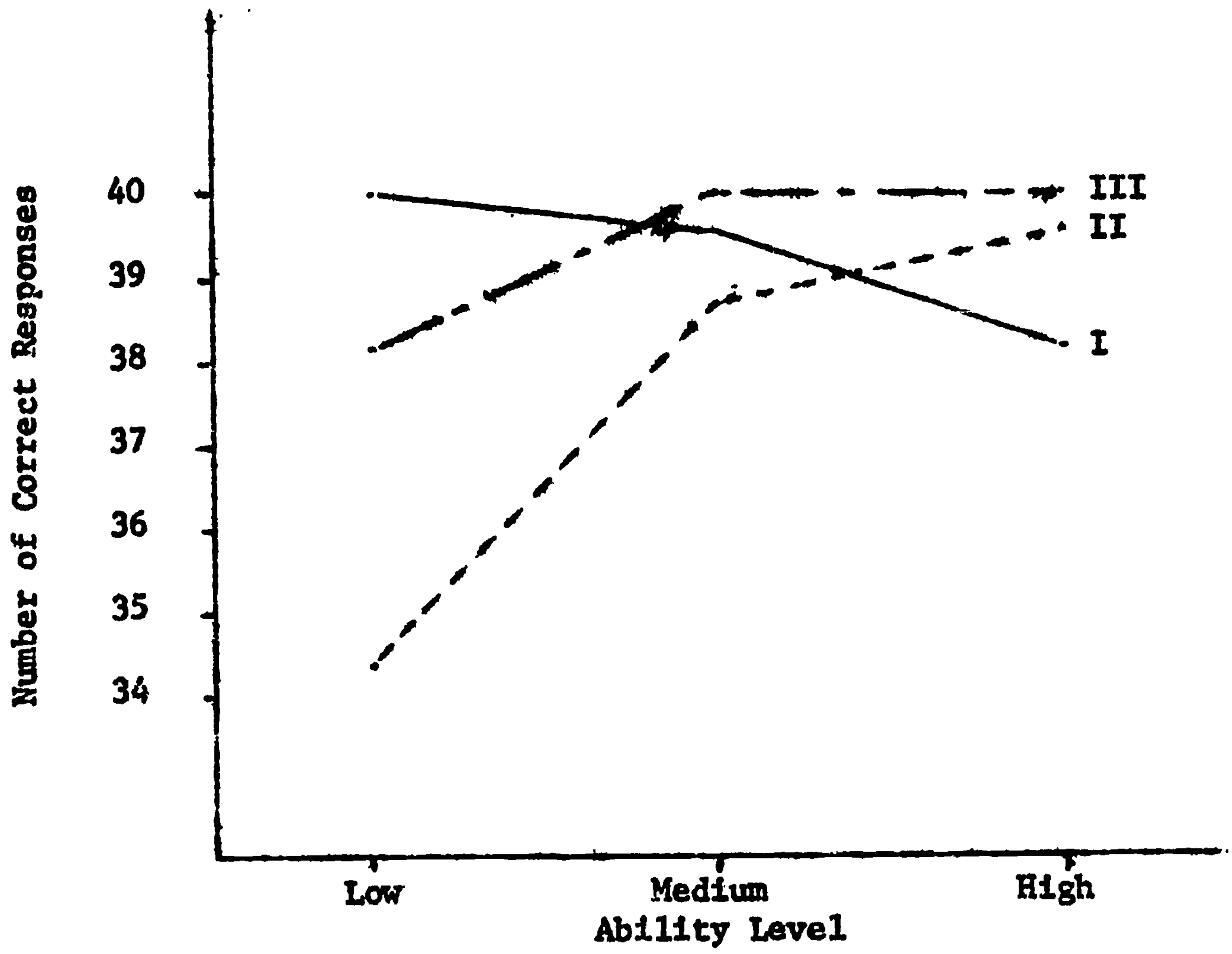


Figure E5. Profiles of Means Showing the Treatment x Levels Interaction. The Dependent Variable is the Criterion Test, Subfact 1.

Treatment Comparison

Ability Level	I-II	I-III	II-III
Low	*		*
Medium			
High			

Figure E6. Summary of the Individual Comparison Tests. Statistically Significant Differences Between Treatments ($p < .05$) are Shown by Asterisks.

Table E9

Treatment Group Means for the Criterion Test, Subfact 2

Treatments	Level 1	Level 2	Level 3	Combined
I	32.14	36.67	36.75	35.52
II	30.39	30.56	35.77	32.43
III	33.00	23.57	33.57	30.18
Combined	31.57	31.43	35.50	

Table E10

Summary of the Analysis of Varience for the Criterion Test, Subfact 2

Source	df	MS	F
Treatment (T)	2	159.32	3.44 *
Level (L)	2	179.31	3.87 *
T x L	4	114.88	2.48
Error	75	46.32	----

* $p < .05$

Table E11

Treatment Group Means for the Criterion Test, Subfact 3

	Level			
Treatments	1	2	3	Combined
I	42.86	44.58	44.38	44.07
II	43.08	46.67	46.54	45.29
III	35.50	38.57	43.57	39.05
Combined	40.86	43.75	45.18	

Table E12

Summary of the Analysis of Variance for the Criterion Test, Subfact 3

Source	df	MS	F
Treatment (T)	2	264.98	6.56 **
Level (L)	2	126.90	3.14 **
T x L	4	26.74	0.66
Error	75	40.39	——

* $p < .05$

** $p < .01$

Table E13

Treatment Group Means for the Criterion Test, Subfact 4

Treatments	Level 1	2	3	Combined
I	28.57	25.75	29.38	28.89
II	31.92	32.22	34.23	32.86
III	30.75	32.14	32.86	31.86
Combined	30.75	30.71	32.50	

Table E14

Summary of the Analysis of Variance for the Criterion Test, Subfect 4

Source	df	MS	F
Treatment (T)	2	115.81	6.97 **
Level (L)	2	20.28	1.22
T x L	4	2.46	0.15
Error	75	16.61	----

** $p < .01$

Table E15

Treatment Group Means for the Criterion Test, Subfact 5

Treatments	Level 1	2	3	Combined
I	34.57	36.83	36.25	36.07
II	35.00	35.00	36.54	35.57
III	36.50	38.57	34.29	36.45
Combined	35.32	36.68	35.89	

Table E16

Summary of the Analysis of Variance for the Criterion Test, Subfact 5

Source	df	MS	F
Treatment (T)	2	5.88	0.26
Level (L)	2	15.13	0.67
T x L	4	20.57	0.91
Error	75	22.68	——

Table E17

Treatment Group Means for the
Criterion Test, Subfact 6

Treatments	Levels 1	2	3	Combined
I	25.00	25.83	26.25	25.74
II	25.39	26.67	28.08	26.71
III	26.13	27.14	28.57	27.23
Combined	25.50	26.43	27.68	

Table E18

Summary of the Analysis of Variance for the
Criterion Test, Subfact 6

Source	df	MS	F
Treatment (T)	2	15.64	1.12
Level (L)	2	29.60	2.12
T x L	4	1.44	0.10
Error	75	13.98	——

Table E19

Treatment Group Means for the Criterion Test, Subfact 7

	Level			
Treatments	1	2	3	Combined
I	26.43	28.33	23.75	26.48
II	26.46	26.67	27.69	26.97
III	24.88	27.86	30.00	27.45
Combined	26.00	27.68	27.14	

Table E20

Summary of the Analysis of Variance for the Criterion Test, Subfact 7

Source	df	MS	F
Treatment (T)	2	11.83	0.64
Level (L)	2	20.19	1.10
T x L	4	41.12	2.24
Error	75	18.37	——

Table E21

Treatment Group Means for the Criterion Test, Subfact 8

Treatments	Level 1	2	3	Combined
I	23.57	23.33	23.13	23.33
II	20.77	25.33	23.46	22.94
III	20.00	23.57	24.29	22.50
Combined	21.25	24.03	23.57	

Table E22

Summary of the Analysis of Variance for the Criterion Test, Subfact 8

Source	df	MS	F
Treatment (T)	2	3.41	0.18
Level (L)	2	51.48	2.79
T x L	4	19.23	1.03
Error	75	18.61	----

Table E23

Treatment Group Means for the Criterion Test, Subfact 9, (Part 1)

Treatments	Level 1	2	3	Combined
I	16.57	17.33	14.38	16.26
II	13.54	16.67	17.31	15.74
III	17.88	16.43	20.00	18.09
Combined	15.54	16.89	17.14	

Table E24

Summary of the Analysis of Variance for the Criterion Test, Subfact 9, (Part 1)

Source	df	MS	F
Treatment (T)	2	37.64	1.53
Level (L)	2	10.26	0.42
T x L	4	36.62	1.49
Error	75	24.62	----

Table E25

Treatment Group Means for the Criterion Test, Subfact 9 (Part 2)

	Level			
Treatments	1	2	3	Combined
I	14.86	16.00	16.88	15.97
II	11.85	11.67	15.00	12.97
III	15.88	13.57	17.14	15.54
Combined	13.75	17.22	16.07	----

Table E26

Summary of the Analysis of Variance for the Criterion Test, Subfact 9 (Part 2)

Source	df	MS	F
Treatment (T)	2	84.03	2.72
Level (L)	2	50.46	1.64
T x L	4	9.38	.30
Error	75	30.84	----

Table E27

Treatment Group Means for the
Criterion Test, Subfact 9, (Part 3)

Treatments	Level 1	Level 2	Level 3	Combined
I	19.57	24.75	23.13	22.93
II	17.85	21.11	19.23	19.20
III	24.00	15.71	21.43	20.55
Combined	20.04	21.32	20.89	

Table E28

Summary of the Analysis of Variance for the
Criterion Test, Subfact 9, (Part 3)

Source	df	MS	F
Treatment (T)	2	70.50	3.59 *
Level (L)	2	5.09	0.26
T x L	4	107.53	5.48 **
Error	75	19.61	----

* $p < .05$

** $p < .01$

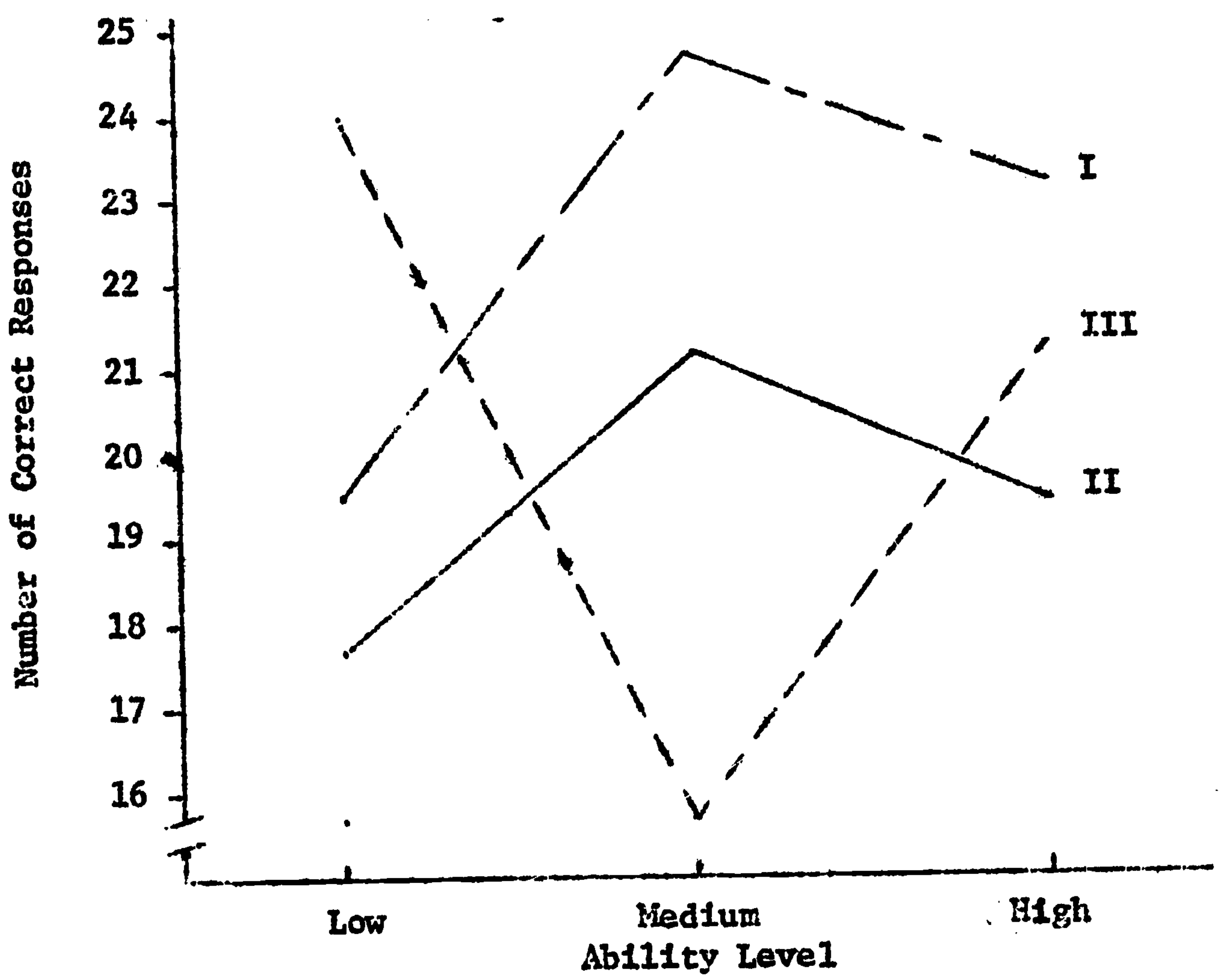


Figure E7. Profiles of Means Showing the Treatment x Levels Interaction. The Dependent Variable is the Criterion Test, Subfact 9 (Part 3).

	Treatment Comparisons		
Ability Level	I-II	I-III	II-III
Low		*	*
Medium		*	*
High			

Figure E8. Summary of the Individual Comparison Tests. Statistically Significant Differences Between Treatments ($p < .05$) are Shown by Asterisks.

Table E29

Treatment Group Means for the Review Quiz, Subfacts 1-8

	Level			
Treatments	1	2	3	Combined
I	142.86	146.25	144.38	144.81
II	141.54	133.89	140.39	139.14
III	133.13	140.71	147.14	140.00
Combined	139.46	140.89	143.21	------

Table E30

Summary of the Analysis of Variance for the Review Quiz, Subfacts 1-8

Source	df	MS	F
Treatment (T)	2	258.63	1.39
Level (L)	2	164.58	0.88
T x L	4	230.37	1.24
Error	75	186.18	----

Table E31

Treatment Group Means for the General Criterion Test

	Level			
Treatments	1	2	3	Combined
I	127.14	138.33	138.75	135.56
II	130.00	124.44	138.85	131.86
III	130.63	129.29	145.71	135.00
Combined	129.46	131.61	140.54	------

Table E32

Summary of the Analysis of Variance for the General Criterion Test

Source	df	MS	F
Treatment (T)	2	148.89	1.16
Level (L)	2	1094.14	8.51 **
T x L	4	256.34	1.99
Error	75	128.56	----

** $p < .01$

Table E33

Treatment Group Means for the
Transfer Test I, Part I, (Word Problems)

	Level			
Treatments	1	2	3	Combined
I	4.29	5.75	6.25	5.52
II	3.08	6.22	6.31	5.09
III	4.25	3.14	7.00	4.77
Combined	3.71	5.25	6.46	----

Table

Summary of the Analysis of Variance for
Transfer Test I, Part I, (Word Problems)

Source	df	MS	F
Treatment (T)	2	2.40	0.20
Level (L)	2	45.99	3.89 *
T x L	4	12.60	1.07
Error	75	11.82	----

* $p < .05$

Table E35

Treatment Group Means for the
Transfer Test I, Part 2, (Distributive Examples)

	Level			
Treatments	1	2	3	Combined
I	4.86	7.25	8.63	7.04
II	4.08	8.56	7.54	6.51
III	3.63	3.71	8.57	5.23
Combined	4.14	6.79	8.11	----

Table E36

Summary of the Analysis of Variance for
Transfer Test I, Part 2, (Distributive Examples)

Source	df	MS	F
Treatment (T)	2	18.29	2.48
Level (L)	2	108.24	14.70 **
T x L	4	19.45	2.64 *
Error	75	7.37	----

* $p < .05$

** $p < .01$

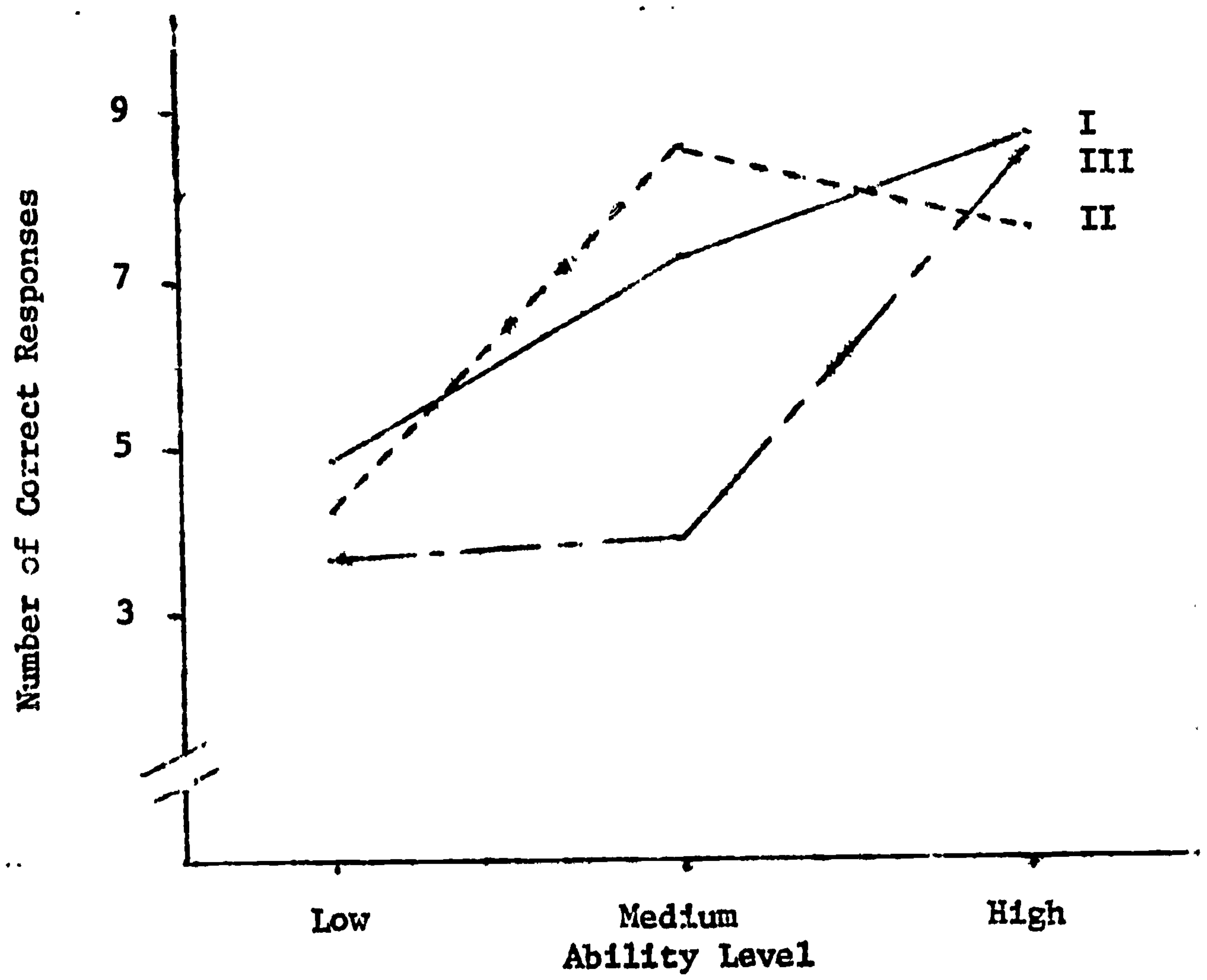


Figure E9. Profiles of Means Showing the Treatment x Levels Interaction. The Dependent Variable is the Transfer Test I, Part 2 (Distributive Examples).

Treatment Comparisons

Ability Level	I-II	I-III	II-III
Low		*	*
Medium			
High			

Figure E10. Summary of the Individual Comparison Tests. Statistically Significant Differences Between Treatments ($p < .05$) are Shown by Asterisks.

Table E37

Treatment Group Means for the Transfer Test II (Number Puzzles)

Treatments	Level 1	2	3	Combined
I	63.29	85.92	69.75	75.26
II	63.85	85.33	68.85	71.23
III	45.50	49.43	81.43	58.18
Combined	58.46	66.61	72.25	-----

Table E38

Summary of the Analysis of Variance for Transfer Test II (Number Puzzles)

Source	df	MS	F
Treatment (T)	2	1575.85	1.77
Level (L)	2	2210.30	2.48
T x L	4	1567.27	1.76
Error	75	889.58	----

Table E39

Treatment Group Means for the
Trial 1 of the Savings Transfer Test I (Meanings of Operations)

	Level			
Treatments	1	2	3	Combined
I	11.71	15.92	14.50	14.41
II	12.46	12.33	16.39	13.89
III	10.38	13.29	18.00	13.73
Combined	11.68	14.11	16.20	-----

Table E40

Summary of the Analysis of Variance for
Trial I of the Savings Transfer Test I (Meanings of Operations)

Source	df	MS	F
Treatment (T)	2	.74	0.02
Level (L)	2	149.01	4.74 *
T x L	4	34.12	1.09
Error	75	31.44	-----

* $p < .05$

Treatment Group Means for the
Trial 2 of the Savings Transfer Test I (Meanings of Operations)

	Level			
Treatment	1	2	3	Combined
I	12.29	18.75	18.88	17.11
II	12.92	17.22	18.85	16.23
III	12.88	17.43	20.29	16.68
Combined	12.75	17.93	19.21	-----

Table E42

Summary of the Analysis of Variance for
Trial 2 of the Savings Transfer Test I (Meanings of Operations)

Source	df	MS	F
Treatment (T)	2	1.98	0.09
Level (L)	2	315.95	13.69 **
T x L	4	5.76	0.25
Error	75	23.08	----

** $p < .01$

Table E43

Treatment Group Means for
Trial 3 of the Savings Transfer Test I, (Meanings of Operations)

Treatments	Level 1	Level 2	Level 3	Combined
I	14.29	17.33	19.00	17.04
II	13.00	18.56	19.77	16.94
III	12.25	17.14	20.29	16.36
Combined	13.11	17.68	19.68	-----

Table E44

Summary of the Analysis of Variance for
Trial 3 of the Savings Transfer Test I, (Meanings of Operations)

Source	df	MS	F
Treatment (T)	2	2.01	0.08
Level (L)	2	289.96	11.97 **
T x L	4	7.22	.30
Error	75	24.22	-----

** $p < .01$

Table E45

Treatment Group Means for
Trial 4 of the Savings Transfer Test I, (Meanings of Operational)

	Level			
Treatments	1	2	3	Combined
I	15.29	18.83	19.00	17.96
II	13.23	19.11	20.00	17.26
III	13.75	18.71	20.43	17.45
Combined	13.89	18.89	19.82	-----

Table E46

Summary of the Anslysis of Variance for
Trial 4 of the Savings Transfer Teat I, (Meanings of Operations)

Source	df	MS	F
Treatment (T)	2	.54	0.02
Level (L)	2	246.70	10.95 **
T x L	4	6.90	0.31
Error	75	22.53	-----

** $p < .01$

Table E 47

Treatment Group Means for
Trial 4 - Trial 1 Change Scores on
Savings Transfer Test I (Meanings of Operations)

	Level			
Treatments	1	2	3	Combined
I	3.57	2.91	4.50	3.55
II	.77	6.78	3.62	3.37
III	3.38	5.43	2.43	3.73
Combined	2.22	4.78	3.57	

Table E 48

Summary of the Analysis of Variance for
Trial 4 - Trial 1 Change Scores on Savings Transfer Test 1
(Meanings of Operations)

Source	df	MS	F
Treatment (T)	2	.04	----
Level (L)	2	41.04	2.17
T x L	4	36.18	1.91
Error	75	18.92	----

Table E49

Treatment Group Means for
Trial I of the Savings Transfer Test II

Treatments	Level 1	2	3	Combined
I	35.71	35.42	33.13	34.81
II	27.69	35.00	34.23	32.00
III	35.00	33.57	36.43	35.00
Combined	31.79	34.82	34.46	-----

Table E50

Summary of the Analysis of Variance for
Trial I of the Savings Transfer Test II

Source	df	MS	F
Treatment (T)	2	65.21	0.59
Level (L)	2	29.18	0.26
T x L	4	78.61	0.71
Error	75	110.48	----

Table E51

Treatment Group Means for
Trial 2 of the Savings Transfer Test II

Treatments	Level 1	2	3	Combined
I	27.14	36.25	28.13	30.59
II	26.54	23.33	32.69	28.00
III	30.00	30.00	38.57	32.73
Combined	27.68	30.54	32.86	-----

Table E52

Summary of the Analysis of Variance for
Trial 2 of the Savings Transfer Test II

Source	df	MS	F
Treatment (T)	2	196.60	1.65
Level (L)	2	182.64	1.53
T x L	4	243.06	2.04
Error	75	119.09	----

Table E53

Treatment Group Means for
Trial 3 of the Savings Transfer, Test II

	Level			
Treatments	1	2	3	Combined
I	29.29	40.00	31.25	34.62
II	36.15	28.89	38.08	35.00
III	36.88	34.29	40.71	37.27
Combined	34.64	35.00	36.79	———

Table E54

Summary of the Analysis of Variance for
Trial 3 of the Savings Transfer Test II

Source	df	MS	F
Treatment (T)	2	92.07	0.89
Level (L)	2	52.14	0.50
T x L	4	295.81	2.85 *
Error	75	103.63	———

* $p < .05$

Table E55

Treatment Group Means for
Trial 4 of the Savings Transfer Test II

	Level			
Treatments	1	2	3	Combined
I	27.14	37.08	34.38	33.70
II	35.00	37.78	39.62	37.43
III	36.88	32.14	41.43	36.82
Combined	35.57	36.07	38.57	-----

Table E56

~~Summary of the Analysis~~ of Variance for
Trial 4 of the Savings Transfer Test II

Source	df	MS	F
Treatment (T)	2	168.49	1.32
Level (L)	2	195.10	1.53
T x L	4	127.60	1.00
Error	75	127.22	----

Table E57

Treatment Group Means for
Trial 5 of the Savings Transfer Test II

Treatments	Level 1	2	3	Combined
I	35.71	39.17	36.25	37.41
II	36.15	43.33	43.08	40.57
III	41.25	39.29	46.43	41.96
Combined	37.50	40.54	41.96	----

Table E58

Summary of the Analysis of Variance for
Trial 5 of the Savings Transfer Test II

Source	df	MS	F
Treatment (T)	2	183.34	2.19
Level (L)	2	121.22	1.45
T x L	4	81.64	0.97
Error	75	83.73	----

Table E59

Treatment Group Means for
Trial 5 - Trial 1 Change Scores on the Savings Transfer Test II

	Level			
Treatments	1	2	3	Combined
I	.00	3.75	3.12	2.59
II	8.46	8.33	8.85	8.57
III	6.25	5.71	10.00	7.27
Combined	5.71	5.71	7.50	-----

Table E60

Summary of the Analysis of Variance for
Trial 5 - Trial 1 Change Scores on the Savings Transfer Test II

Source	df	MS	F
Treatment (T)	2	302.96	2.23
Level (L)	2	38.50	0.28
T x L	4	22.74	0.17
Error	75	135.85	----

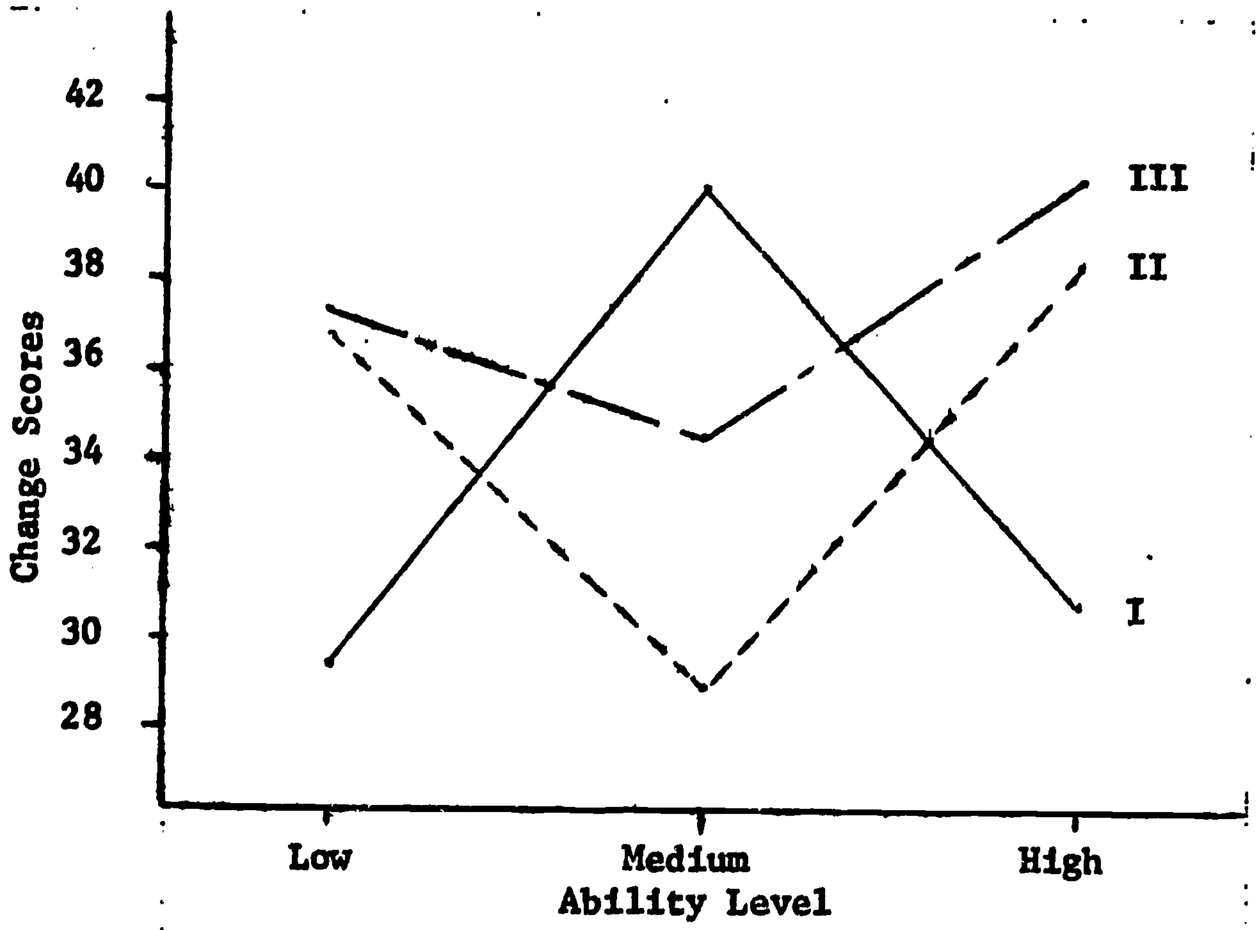


Figure E11. Profiles of Means Showing the Treatment X Levels Interaction. The Dependent Variable is Trial 3 of the Savings Transfer Test II. No Comparison Between Treatments of Each Level was Significant ($p < .05$).

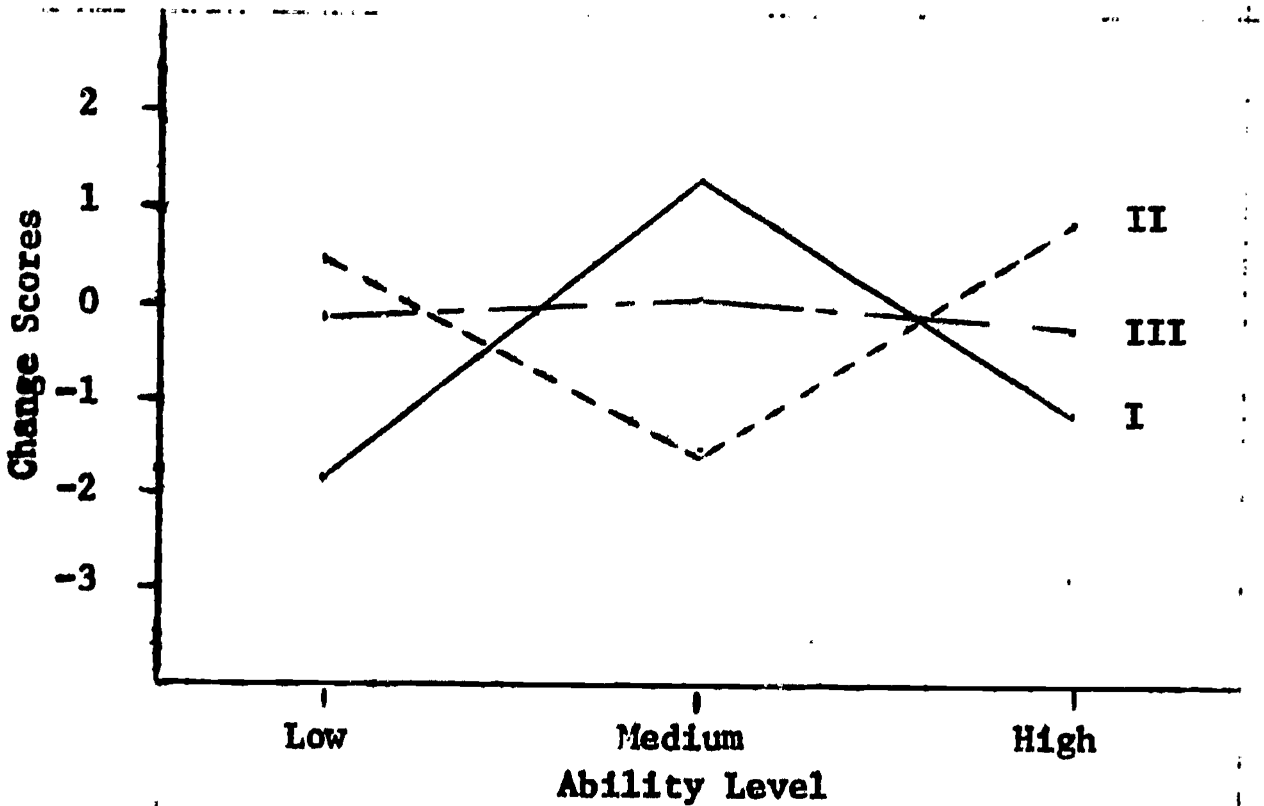


Figure E12. Profiles of Means Showing the Treatment X Levels Interaction. The Dependent Variable is Transfer Test I, Part 2 (Distributive Problems) Change Scores. No Comparison Between Treatments at Each Level was Significant ($p < .05$).

Table E61

Treatment Group Means for the
General Criterion Test Given Three Months After Instruction

	Level			
Treatments	1	2	3	Combined
I	121.25	132.78	118.00	126.11
II	115.91	119.44	121.82	119.03
III	118.33	112.86	136.67	119.38
Combined	117.62	122.40	123.16	------

Table E62

Summary of the Analysis of Variance for the
General Criterion Test Given Three Months After Instruction

Source	df	MS	F
Treatment (T)	2	147.92	0.68
Level (L)	2	194.70	0.90
T x L	4	484.27	2.23
Error	56	217.39	----

Table E63

Treatment Group Means for the
General Criterion Test Change Scores
(Given Three Months After Instruction)

Treatment	Level 1	Level 2	Level 3	Combined
I	- 5.00	- 5.56	-21.00	- 9.70
II	-13.18	- 5.00	-16.82	-12.09
III	-14.17	-16.43	-16.67	-15.63
Combined	-11.90	- 8.38	-17.90	-----

Table E64

Summary of the Analysis of Variance for the
General Criterion Test Change Scores
(Given Three Months After Instruction)

Source	df	MS	F
Treatment (T)	2	115.10	0.52
Level (L)	2	398.23	1.79
T x L	4	137.34	0.62
Error	56	222.19	----

Table E65

Treatment Group Means of Transfer Test I, Part 1, (Word Problems) (Given Three Months After Instruction)

	Level			
Treatments	1	2	3	Combined
I	3.00	5.11	5.80	4.83
II	2.81	5.00	5.27	4.32
III	1.40	2.43	4.75	2.69
Combined	2.50	4.32	5.30	----

Table E66

Summary of the Analysis of Variance for Transfer Test I, Part 1 (Word Problems) (Given Three Months After Instruction)

Source	df	MS	F
Treatment (T)	2	15.02	1.94
Level (L)	2	35.04	4.53 *
T x L	4	1.91	.25
Error	56	7.74	----

* $p < .05$

Table E67

Treatment Group Means for
Transfer Test I, Part 1, (Word Problems) Change Scores
(Given Three Months After Instruction)

	Level			
Treatments	1	2	3	Combined
I	-2.25	- .22	- .60	- .78
II	- .36	-1.22	- .73	- .74
III	-1.40	- .71	-4.50	-1.87
Combined	-1.00	- .72	- .95	-----

Table E68

Summary of the Analysis of Variance for
Transfer Test I, Part 1 (Word Problems) Change Scores
(Given Three Months After Instruction)

Source	df	MS	F
Treatment (T)	2	10.73	1.36
Level (L)	2	7.52	.95
T x L	4	11.40	1.44
Error	56	7.92	----

Table E69

Treatment Group Means for
Transfer Test I, Part 2, (Distributive Examples)
(Given Three Months After Instruction)

	Level			
Treatments	1	2	3	Combined
I	3.50	8.00	7.60	6.89
II	4.36	7.22	8.27	6.58
III	1.60	3.71	8.75	4.31
Combined	3.50	6.52	8.20	----

Table E70

Summary of the Analysis of Variance for
Transfer Test I, Part 2, (Distributive Examples)
(Given Three Months After Instruction)

Source	df	MS	F
Treatment (T)	2	19.84	2.76
Level (L)	2	109.03	15.19**
T x L	4	12.01	1.67
Error	56	7.18	----

** $p < .01$

Table E71

Treatment Group Means for
Transfer Test I, Part 2, (Distributive Examples) Change Scores
(Given Three Months After Instruction)

	Level			
Treatments	1	2	3	Combined
I	-2.75	1.11	-1.20	- .39
II	.45	-1.33	.82	.06
III	- .20	.00	- .25	- .13
Combined	- .35	- .08	.10	----

Table E72

Summary of the Analysis of Variance for
Transfer Test I, Part 2, (Distributive Examples) Change Scores
(Given Three Months After Instruction)

Source	df	MS	F
Treatment (T)	2	4.72	.84
Level (L)	2	3.02	.54
T x L	4	17.29	3.08 *
Error	56	5.61	----

*$p < .05$