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THE INFLUENCE OF CHOICE OF MATERIALS AND PROMPTS AND FEEDBACK UPON THE ARITHMETIC PERFORMANCE OF FIRST-GRADE CHILDREN

by

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The present investigation was conducted in order to examine specific components or variables of academic instruction using objective criteria. In particular, the additional value of instructional prompts and feedback over attention and exposure to arithmetic worksheets, and the motivational value of choosing one's learning materials were each examined.

Forty first-grade children with low scores on the WISC, a standardized test of arithmetic achievement, were assigned randomly to one of the following five conditions: choice and instruction, no-choice and instruction, choice and no-instruction, no-choice and no-instruction, and no-contact control. Instruction groups received prompts and feedback in addition to the attention and exposure to materials given to the no-instruction groups. Children assigned to choice conditions selected colored worksheets and the objects that they would use for counting. The actual worksheet and instructional content of each session were held constant by presenting multi-colored varieties of the same worksheet for choice. Progress was assessed by six change and two within-session measures of performance. Change measures were calculated as the difference between pretest and posttest scores an the WRAT, an inventory of specific arithmetic skills, arithmetic attitude, reading attitude, teacher-rated arithmetic attitude, and

teacher-rated arithmetic achievement. Within-session measures included effort and accuracy. Data were analyzed using multivariate and univariate analyses of variance. Thirty-five (or thirty-four) twenty-minute arithmetic-teaching sessions were held for all but the no-contact groups. The instruction received and the particular worksheets used in each session were individualized; material was originally based on pretest scores, and children progressed through a sequence of units based on their daily performances.

Groups receiving instruction were superior at posttesting to the no-instruction groups on within-session accuracy and specific arithmetic achievement. The instruction groups also changed more positively on arithmetic attitude, but this measure was confounded by significant pretest score differences. Instruction groups decreased over time on within-session effort and accuracy; this likely reflected the increasingly difficult worksheets to which these children were advanced. Instructional prompts and feedback were more beneficial than attention and exposure to materials alone; this latter condition may have been worse than no contact at all. The effectiveness of instructional prompts and feedback supports the current use in many schools of small-group resource rooms. The utility of instruction was demonstrated by specific achievement measures; generalization to the class and to broad measures was not seen, although these measures are most frequently used in educational research. The effectiveness of prompts and feedback was contrary to educational innovations that encourage teacher nonparticipation.

The present study found no evidence that choice of materials was different from no choice of materials. This finding is contrary to the sparse literature that clearly led to the hypothesis that choice would motivate children positively. Free choice has been advocated for children and has often been assumed to be a desired ingredient of curriculum. There are several possible reasons for the obtained results consistent with previous findings. It is possible but unlikely that the manipulation was ineffective. Performance may not be motivated by choice alone when materials are held constant. Finally, the motivational value of choice may be learned and thus only evident in children beyond first grade.

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CHAPTER I

INTRODUCTION

Education today witnesses many new and experimental teaching methods and materials. They range from programs to be incorporated in traditional classrooms to complete reorganization of the school. "Open classrooms," which typically includes learning stations and much student movement, represents one example of the changes from the traditional self-contained classroom.

Research involving new procedures is scanty; most programs are justified with speculation or other verbal rhetoric (Myers & Klein, 1969). Well-conducted research involving new techniques is difficult and time-consuming to perform. Research requires school systems to allow programs designed by an experimenter and involves measures over extended periods of time. Results are attributable to all the variables that are manipulated, including unintentional ones such as class size and teacher interest. Even when effective research is performed, results may be of little importance for understanding the learning process. When a whole package of techniques is compared to traditional procedures, one cannot tease out the components of the new technique that are important and the ones that are extraneous (and yet possibly costly). And, of course, it might be the mere novelty of the new technique that produces a positive change.

Whole packages of new, open-classroom techniques have been compared with intact, traditional situations. Schools employing these new techniques are given labels such as innovative, progressive, or free, and often involve learning centers, ungraded classes, and open classrooms, as opposed to the traditional "sit still and be lectured" to arrangement. The new or free schools have arisen both within public schools and as private alternatives to them. Little real analysis of these schools is available, however (Cooper, 1976). Gardner (1966) provides a good summary of the early research. In general, it is found that the free schools gain some advantage over control schools on achievement and attitude measures. Some research has been very extensive; one study published in five volumes followed children for eight years. Indicative of the kind of findings obtained, the fifth volume (Chamberlain, Chamberlain, Drought & Scott, 1942) measured college performances of students graduated from a progressive school in comparison with those of traditional school graduates. The progressive school graduates had some advantage on measures of college performance, reasoning, curiosity, and participation.

Heathers (1967) compared traditional, self-contained classrooms with one package of innovations he labelled "dual progress" classrooms. He found no real differences on achievement, although most attitudes towards the new program were favorable. Killough (1973) found academic achievement of children in a non-graded, open school in which children from several primary grades were grouped together was significantly higher than that of children in a traditional, graded school. Burham (1972) found no differences in reading and arithmetic achievement after one year of primary school where children were enrolled in either traditional or open classrooms. Studies comparing new packages with traditional techniques vary in terms of how the free or experimental programs are defined and in what is controlled in so-called traditional classes. The independent variables are numerous, broad, and rarely specified in operational terms or measured at all. This research does suggest that newer, experimental programs may have slight advantages over traditional classes. Researchers now need to specify those components of new programs that individually contribute to increased performance.

A common general component of many of the new procedures is increased child "freedom," both in terms of more independent work and choice of activities. For example, Heathers (1967) describes one component of his "dual progress" plan as individualized, nongraded advancement in <u>elective</u> subjects. Montessori (1917/1965) is probably the best-known proponent of freedom in education. She believes that lessons should involve minimum lecture and maximum choice and activity by the child. Cooper (1976) notes that proponents of open schools attempt to free children to explore and learn on their own.

Instructional Prompts and Feedback

The role of instructional prompts in teaching--namely instruction, explanations, and guidance, and direct feedback from performance--has not been adequately explored. These variables appear basic, but research is complicated by many sources of variation; much of what children can learn themselves from materials will depend on their individual histories and the particular materials. The few studies that have explored instructional prompts and direct feedback have focused on variations among a few instructional packages or methods of feedback, or have compared instruction with no treatment. Instruction involves the teaching method employed by the teacher or tutor, which is one part of curriculum, the total educational environment.

One purpose of the present study was to examine the role of instructional prompts and performance feedback on the arithmetic performance of first-grade children. Three groups were compared on achievement, attitude, teacher rating, and session performance: (1) a control group receiving no extra arithmetic help, (2) two groups exposed to arithmetic materials and given encouragement and attention, and (3) two groups exposed to materials and given attention, feedback on problems contingent with performance, and instructional prompts.

Two studies each examined the role of different instructional feedback methods. Fink and Carnine (1975) compared (a) feedback with (b) feedback and graphing of feedback on arithmetic problems in an ABAB design. When children were requested to graph their worksheet scores, they made fewer errors than when they merely were told their worksheet scores. Also examining the role of feedback, McLaughlin and Malaby (1974) found that tokens awarded for good arithmetic performance resulted in greater accuracy and faster work than tokens awarded for poor performance.

Several investigators have looked at the relative importance of structured and traditional teaching methods. Ellson, Harris, and Barber (1968) reported concern with whether the effectiveness of programmed tutoring compared with no-treatment controls found in an earlier study was due merely to individual attention or to programmed techniques. They examined highly structured programmed tutoring in relation to directive tutoring in reading, using 10 achievement measures. The former instructional technique followed a highly specified and individualized program and the latter included activities and programs based on teacher recommendations and tutor judgment. It was found that directed tutoring had no differential effect on achievement, compared with the no-treatment control condition. The programmed tutoring group was significantly higher than the other groups over a full year, but no effect was found for only one semester of programmed tutoring. Ronshausen (1972) similarly examined the effects of programmed tutoring and directed tutoring in arithmetic on both achievement and attitude. She found directed tutoring superior to programmed tutoring, which was equivalent to a matched, no-contact control group on computational skills; no differences were found on measures of attitudes or arithmetic concepts. Ronshausen (1974) then compared programmed tutoring with a no-contact control group to determine if there was any benefit to the tutoring technique. She found some advantage due to tutoring after one semester of daily instruction.

Accountability in education demands research similar to the studies cited above, in which one method of instruction is compared with regular teaching or, more rarely, a no-treatment control. Process research examining the differential benefits of components of instructional packages is rare in educational or psychological literature. This tendency is probably due primarily to ethical considerations as well as to assumptions about important variables. It is usually assumed that instruction, which typically involves attention, feedback, instructional prompts, and instructional materials, increases achievement. Thus, it becomes unethical to deny children any component of instruction in order to examine these variables. Assumptions about instruction and selection of comparison groups are illustrated in the following quote from Ellson, Harris, and Barber (1968):

> An experimental comparison of programmed tutoring, which is a carefully planned instructional procedure with unstructured attention giving, would almost certainly favor programmed tutoring and would almost certainly be condemned as unfair, especially if the effects were measured by reading achievement tests. [It was therefore assumed] that "individual attention" meant the reading-related activities which a well-trained teacher would direct a non-professional to use in tutoring sessions designed to supplement classroom reading instruction. (p. 310)

A need is evident for more process research into the differential effectiveness of components of successful instruction techniques. At least one group of investigators has recognized this need; Strain, Shores, and Kerr (1976) plan to perform research in order to examine prompts and feedback independently; they found that an instructional package including both components successfully increased the social behavior of retarded children. Results from process research could help generate more effective and less costly teaching strategies and programs.

Choice in Instruction

In many new programs, children are given the freedom and responsibility to explore and learn independently from materials. Choice is usually assumed to enhance motivation. For example, Durkin (1974) said in her book on reading instruction:

> Children should be allowed to make some of the decisions about what they will do. Everyday observation identifies values to be derived from activities that are chosen by children rather than assigned by adults. (p. 69)

She further suggests that the ability to choose the right book may be "inborn" and that self-selection creates the interest and involvement necessary to learn reading (p. 74). The role of choice in learning is another poorly investigated variable. The importance of choice of materials for arithmetic attitude and for performance and learning of arithmetic behaviors was examined in the present investigation.

Choice in learning situations can actually involve antecedent materials, the particular task, or reinforcement conditions. The sparse literature related to each of these types of choice and to some related theory are reviewed below.

Dissonance theory (Jones & Gerard, 1967, p. 211) suggests that if a person has no preference between two similar items or activities and is required to choose between them, the chosen item or activity will increase in perceived value relative to the one not chosen. According to dissonance theory, when there is little objective reason for choosing or when there is danger of postdecision regret, a person has a need to justify a choice. This justification, which results from cognitively working through a decision, results in enhanced value of the chosen alternative and lower valuing of the nonchosen one (Jones & Gerard, 1967, pp. 211, 214). Theory thus supports the notion that one would be more motivated to perform or learn if materials are actively chosen, because cognitive processes following the decision would enhance the perceived value of the materials.

The present investigation examined the effect of choosing learning materials upon the arithmetic performance and attitudes of first-grade children. Several lines of investigation bear relevance to the motivational value of choice for learning. Brehm (1956) examined the effect of choice on the rating of objects. He asked college students to rate each of 12 appliances. Each subject then was given a choice between two objects to keep (either close in value on the initial rating or disparate). Finally, after reading literature on two objects (either the two involved in the choice or two others), the subject rated all 12 objects again. The object chosen increased in rating and the object refused decreased in rating independently of the literature read. This effect was most pronounced when the two choice objects were similar in initial rating. Subjects in a control group who were merely handed one object did not change their rating of that object. A second study by Brehm and Cohen (1959) found the same results with children. When school children were asked to rate 16 toys and then were given a choice between two _qualitatively_ similar or dissimilar toys, their subsequent rating of the chosen toy increased relative to that of the nonchosen toy. This effect was greatest when the choice objects were dissimilar. Note that in this study, children were given a choice between objects that _looked_ similar or dissimilar. In the previous study, adults chose between objects that were similar or dissimilar on the initial _rating_.

Two studies have controlled choice by using differential reinforcement and have then examined the subsequent effect on choice. On the basis of several studies, Osipow (1972) concluded that task success influences task preferences. Subjects chose the one most pleasing or most attractive of two similar stimuli. Then the stimuli were used in a choice paradigm where either the least or most preferred stimulus was reinforced most frequently. The subjects subsequently rated the stimuli; the ones associated with "success" (reinforcement) were most preferred. Osipow carefully pointed out that choice (action) is not the same as preference because choice is influenced by many situational variables.

Berancourt and Zeiler (1971) allowed nursery-school children to select initially from equally paying jobs. Then selection of nonpreferred jobs only was reinforced. Children under this condition selected the nonpreferred jobs. When reinforcement was reinstated for all jobs, however, the initial preferences were again exhibited; there was no increased preference for the rewarded jobs. The differences between the results of Osipow's and Berancourt and Zeiler's studies may be due to the children's actually performing their jobs in the latter, rather than merely ranking objects as in the former. In addition, the stimuli used by Osipow were more similar to each than were the jobs the children performed in Berancourt and Zeiler.

An expanding literature involves internally and externally perceived incentive for tasks. In dissonance theory, psychological reactance is defined as a motivational state that is aroused when freedom is threatened (Jones & Gerard, 1967, p. 500). De Charmes (1968, p. 269) similarly hypothesized that people strive to be origins of their own behavior and are motivated to be effective in producing changes in their environments. The behavior of an individual who perceives personal causality or control is assumed to be characteristically different from his behavior when that individual feels external causality (p. 319). Intrinsically motivated behavior is defined as behavior motivated by the need to feel competent and in control and is associated with concomitant feelings of free choice and commitment (p. 329).

Deci (1971, 1975) further examined and expanded upon de Charmes' theory, suggesting that external rewards for a behavior would cause a person to feel external influences and perceived external motivation. Although people will perform for external controls, the perceived external locus of control implies that due to striving for freedom, the person will not perform the influenced behavior in a free choice situation. When a person perceives that his behavior is intrinsically motivated, he will perceive himself as a cause of his behavior and will likely choose to repeat that behavior in a choice situation. Extrinsic rewards given for intrinsically motivated tasks will shift the perceived control from internal to external and lower the unreinforced performance in the future. Similarly, insufficient reward for a chosen activity will often cause the perception that one was internally motivated. In education, one could expand Deci's reasoning and hypothesize that children given the perception of choice of activities would feel more intrinsically motivated towards the chosen activity and thus perform more willingly and enthusiastically.

Evidence generally supports de Charmes' hypotheses (Deci, 1971, 1975; Notz, 1975). One well-designed and executed study involved preschool children. Lepper, Greene, and Nisbett (1973) measured the time spent by children coloring when several activities were available. Children who spent the most time coloring were asked to draw individually for the experimenter. Children received either no reward, an unexpected reward, or an expected reward for coloring. Their subsequent free choice for coloring was measured and the group receiving the expected reward colored significantly less than the other two groups. Notz (1975) cites a contradictory study where an unexpected reward reduced the intrinsic motivation of a task.

Several studies have investigated the effect of allowing a child to choose his own reinforcement criterion on performance. Bandura and Perloff (1967) found that for rate of performance of a simple motor task in children, self-selected criteria and self-administered reinforcement were equal to experimenter-selected criteria and experimenter-administered reinforcement, and that both were better than noncontingent or no reinforcement. Lovitt and Curtiss (1969) found that a 12-year-old boy in a special education class had a higher academic response rate when he determined his own achievement criterion than when the teacher determined the criteria. Glynn (1970) found that for ninth-grade girls, self-determined and experimenter-determined reinforcement were equal to each other and better than chance or than no reinforcement. Bolstad and Johnson (1972) found self-regulation slightly superior to external regulation in producing consistently lower rates of disruptive behaviors. Felixbrod and O'Leary (1973) found self-determined and externally determined reinforcement criteria equal for academic performance. Kanov (1973) found that self-selection of a reinforcement schedule was not more effective than a reinforcement schedule determined by controlling through matching the time and amount of reinforcement. In a multiple baseline design, Parks (1973) found that teacher-determined ratios of token reinforcement resulted in fewer correctly completed arithmetic problems than child-determined ratios; however, teachers reinforced less frequently, with the result that the effect of self-selection was confounded by the differential frequencies of reinforcement.

From these studies it appears that choosing one's reinforcement criterion (not the reinforcer itself) from among certain alternative criteria is at most slightly more effective than having the criteria externally imposed. Thus, choice of reinforcement criteria does not strongly enhance responding above externally determined amounts. This effect may be due to a ceiling effect of reinforcement; however, performance for reinforcement may be at a high value at which choice would not further increase behavior. In most of the cited studies, children selected the reinforcement criteria resulting in the several responses per reinforcement; this may have influenced any effect of choosing since all choices were, in effect, determined. In addition, choosing the criterion for reinforcement could be less important than choosing the reinforcer itself.

Kulkin (1972) looked directly at both choice of the reinforcer (not the criterion) and choice of materials on reading performance. He found that choice of reinforcement had no effect on performance, but choice of materials did positively affect performance.

The Present Investigation

The need for research studying components of new educational packages is evident. Concepts on which new educational programs are based, such as freedom and choice, must be objectified and studied. The present study allowed for the independent assessment of instructional feedback and prompts, and choice of materials, as well as their interaction. Specifically the main hypotheses tested were that (1) instruction including feedback and prompts compared with attention and exposure to materials alone, and (2) choice of arithmetic materials compared with assigned materials, would each motivate children toward better attitudes towards arithmetic and better performance and achievement in arithmetic. Improved arithmetic attitude should improve other school attitudes; reading attitude in particular was measured to assess this.

Children were taught in small groups outside the regular classroom. Experimental children were singled out for special attention and exposed to extra materials and attention similar to those for children who attend special resource classrooms for part of the school day. Arithmetic materials and instruction were chosen because arithmetic skills may be arranged in a linear hierarchy whereas reading involves many different skills at each achievement level.

CHAPTER II

METHOD

Subjects

The arithmetic subtest of the Wide Range Achievement Test (WRAT) was individually administered to 182 first-grade children in four classrooms in each of two schools in January, 1976. Their mean WRAT score was a 1.68 grade level with a standard deviation of .54 grade levels. Scores ranged from a PK.8 (eighth month of prekindergarten) to a 3.0 grade level. Five children who received Educable Mentally Retarded (EMR) resources and one chronically absent child were excluded from consideration before testing. The four children that fell more than two standard deviations below the mean WRAT score were excluded from the study. After these deletions, the six children in each of the eight classes with the lowest WRAT arithmetic scores were included as subjects. In the case of duplicate scores, random assignments was used to select the six children. The 48 subjects had an average WRAT grade equivalent score of 1.1 and their scores ranged from 0.7 to 1.8 grade levels. At the end of testing they ranged in age from 6.3 to 8.0 with a mean age of 6.9. There were 27 females and 21 males.

Experimental Variables

This study involved two experimental variables: instruction in arithmetic and choice of materials. The six subjects within each class were assigned randomly by drawing names; one child was in each of six conditions. Each experimental group thus included one child from each classroom. Four conditions were treatment groups, one group was a no-treatment control, and one child in each class was designated as a potential subject in case a child was lost to the experiment due to prolonged illness or to leaving the school. Only one of these extra children was actually used; one substitution for a control group child was made at the time of posttesting.

Experimental Conditions

The five groups included the four experimental conditions obtained by crossing choice or no choice of materials with instruction or no instruction (explained below), and one no-treatment control. Eight children, one child from each classroom, were included in each condition. Four children met with the experimenter at a time for experimental sessions. These conditions were described as follows:

(1) Choice of materials and arithmetic instruction. The eight children in this condition received a choice of objects and worksheet color and then were instructed using the chosen materials.

(2) Choice of materials and no arithmetic instruction. These eight children chose their objects and worksheets and then received attention but no direct instruction with the chosen materials.

(3) No choice of materials and arithmetic instruction. The eight children in this condition were given appropriate materials and then were taught using these materials.

(4) No choice of materials and no arithmetic instruction. These eight children were given appropriate materials and then received attention without prompts or feedback relevant to their materials.

(5) No-treatment control. The eight children in this condition were assessed before and after the experiment on all dependent variables, identical with the children in the treatment groups. They did not meet with the experimenter for training sessions.

Instruction in arithmetic. Two of the treatment groups (16 children) received arithmetic instruction, including instructional prompts and feedback. These children received individual instruction during every fourth minute of each experimental session. For the remainder of the time, these children were requested to work by themselves. A hierarchical sequence of arithmetic units was written that began with easy matching skills and progressed to carrying and borrowing skills. One two-sided worksheet was written to correspond to each unit. All children were assigned to a unit before each session based on individual performances. Appendix A includes a list of these units and a synopsis of the experimenter's instructions for each unit for children in the instruction condition. The experimenter instructed and practiced with problems similar to the worksheet problems using prompts and directions, and gave feedback on work performed during instruction and independent periods (to be conservative, any problem corrected due to the experimenter's feedback was scored as incorrect).

Children in the no-instruction conditions received the same amount of attention as children receiving instruction. The experimenter sat with each child for one of every four minutes and verbalized encouragement for working with the worksheets and materials but gave no prompts, instructions, or feedback relevant to the materials.

Appendix B includes a sample of dialogue from an instruction session and from a no-instruction group session.

Choice of materials. Before each session, each child was supplied with a colored worksheet and a number of small objects used for counting and other instructions. Children in the choice condition selected their objects and worksheets. Objects included Q-tips, picture dominos, poker chips, bottle caps, pinto beans, and paper clips. At the beginning of each session, the children in the two choice groups selected one of three available sets of objects. The available choices were varied each day, and no object was present on three successive days. Children in choice groups also selected among three different colored but identical worksheets. Three of the four colors (white, pink, blue or yellow) were available each day on a rotational basis; occasionally a particular colored worksheet was unavailable, and the three remaining colors were presented for selection.

Each child in no-choice groups was presented with a set of objects and a colored worksheet. These materials were determined by randomly pairing each child in a no-choice group with a child in a choice group that was in the same school but in a different experimental session. The color and object set that the child in the choice condition selected for a session was assigned to the yoked no choice child for the next session. For the first session and whenever a choice child was absent, the materials for the no-choice child were randomly chosen from the available options.

All materials were selected or presented immediately outside the experimental room door out of view of the other children.

Dependent Variables

All children in the five experimental groups were assessed before and after the treatment sessions on six change measures. In addition, children in the four treatment groups received performance scores for each session.

Change Measures. The children in the five experimental groups were assessed before and after the intervention on the WRAT, Arithmetic Inventory, arithmetic attitude, reading attitude, teacher rating of arithmetic achievement and teacher rating of arithmetic attitude. Children were pretested before being assigned to groups. Pretesting was performed by six female graduate students from the University of North Carolina at Greensboro. About half of the pretesting was performed by the experimenter; all the examiners were qualified and experienced at testing. Posttesting was performed by two female graduate students, one of whom had participated in pretesting. These examiners were not informed about the purpose of the experiment or the experimental conditions and performed all testing in the absence of the experimenter.

(1) Arithmetic performance.

a. The Wide Range Achievement Test (WRAT) is a nationally standardized test that gives the grade equivalent performance of a child in arithmetic, reading, and spelling. The arithmetic section of this test was individually administered, and the grade equivalent score was used in all analyses.

b. An Arithmetic Inventory (Appendix C) was devised by designing one item similar to each of the 61 study units. The test was detailed and designed to be sensitive to small increments in skills.

(2) Attitudes were assessed by orally asking children 18 questions that elicited dichotomous answers (i.e., yes or no; see Appendix D). Nine questions refer to arithmetic, and nine matched questions refer to reading. The arithmetic score used for analysis was the total number of the nine arithmetic questions answered positively; similarly, the reading score was the total number of reading questions answered positively.

(3) Teacher ratings were obtained by requesting each of the eight teachers to complete a short, written questionnaire on the children selected from their classrooms (Appendix E). This questionnaire contained one section with the same nine arithmetic attitude questions used to assess child attitudes. Teacher observation of arithmetic attitude was scored by subtracting the number of negative answers from the number of positive answers; this method was used because a few teachers did not respond appropriately to one or more items (teachers wrote notes, skipped questions, circled two answers, etc.). Of nine questions, a mean of 8.75 were scorable on each questionnaire.

The second section of the questionnaire provided a measure of teacher-judged achievement and included five questions. Teachers were asked to circle an answer along a five-point scale. The score used for analysis was obtained by assigning one to the poorest achievement rating, five to the highest achievement rating, and two, three, or four to intermediate ratings. The scores for the five items were averaged. Again, several teachers provided occasional answers that could not be scored; in these cases, the scorable answers were averaged. On the five questions, teachers averaged 4.9 scorable items on this scale.

Repeated Measures. Two within-session measures were scored and analyzed for the four treatment groups. For each session the number of worksheet problems attempted and the number correct were counted and recorded for each child.

The percentage of problems attempted and the percent of attempted problems correctly answered were then computed. An arcsine transformation on these scores was used for the statistical analyses (Winer, 1971, p. 400).

Individual items that received assistance in instruction conditions were scored incorrect to be conservative. Worksheets were designed to be progressively more difficult, but they were designed to be completed within experimental sessions. Two measures were thus obtained each session; one reflected accuracy (percentage of attempted problems correct), and one reflected effort (percentage of available problems attempted).

Procedure

Setting. Each of the two schools provided a room large enough for four children to work independently. Four desks placed in each room were arranged to minimize child interactions. Children received their materials immediately outside the experimental rooms.

Experimental sessions. Children were sent to the experimental room by their teachers in one school; the experimenter gathered the children from their classrooms at the second school. Four children who either received instruction or no instruction met together at one time. Two of these children were in the choice condition, and two received no choice. Children sat at desks in the experimental room and were called one at a time to meet with the experimenter immediately outside the door to receive materials. Children came first to get their materials on a rotational basis except when the privilege was given or withdrawn contingent on good or bad behavior travelling to the experimental room. Outside the room the children in the choice condition selected their materials, and the children in the no-choice conditions were handed their preselected materials.

During each session the experimenter rotated among the four children, spending one minute with each child until the session ended. During each session, each child received an equal number of one-minute intervals of either instruction or attention and was asked to work independently for the remainder of the time. Some quiet talking was tolerated but any discussion about arithmetic was discouraged. Sessions were run on Mondays, Wednesdays, and Fridays with a few changes due to schedule difficulties. Each session was scheduled for 25 minutes; after children travelled to the room and materials were dispensed, about 15 to 20 minutes usually remained. Fire drills, physical education, music, and art classes, school assemblies, and other "usual" school events occasionally interrupted sessions or shortened them. It was assumed that these events did not favor any particular group. Thirty-six sessions were planned for each group; thirty-five (sometimes thirty-four) sessions were actually held due to experimenter illness. Sessions were combined into six time blocks (of six sessions each) for analysis; the last one (occasionally two) blocks included only five sessions.

Assignment of children to worksheets and math units. Before math sessions began, each child was pretested on the Arithmetic Inventory which included one item for every worksheet and unit. Each child's arithmetic instruction began at the unit corresponding to the first error on the Inventory. Units covering each subsequent item on the Inventory that had been failed were then taught. At the point that two consecutive Inventory items were failed, units were assigned consecutively.

Children were advanced from one unit to the next scheduled unit when at least 50% of those worksheet problems were attempted and at least 80% of the problems were correctly performed. After three sessions with one worksheet, the child was moved to the next worksheet, even if the above criteria were not met.

Statistical Analyses and Predictions

The main hypothesis tested was that choice and instruction act incrementally to increase academic performance in arithmetic and improve arithmetic attitudes of first-grade children who score low in arithmetic achievement relative to classmates. The four classes of dependent variables, namely arithmetic achievement, attitudes, teacher ratings, and session performance, were each considered separately. These variables were not combined into one single multivariate analysis because there was no *a priori* reason to expect all these measures to have a similar source of variance and because the number of subjects that would then have been required would have been impractical.

Multivariate analyses of change scores is superior to multivariate analyses of covariance with pretest scores as the covariate because the latter covaries for the canonical correlate of the measures (a composite of all measures) rather than considering each pretest measure separately. Himmelfarb (1975) suggests testing all groups in a one-way analysis and in addition recommends excluding the control group for a factorial analysis. Since there were two components to each change measure, each multivariate analysis included two dependent variables. The number of subjects in each cell exceeded by two the minimum number recommended by Applebaum (personal communication, 1975). A .05 probability level was used to make decisions about significance.

To obtain change measures (WRAT, Arithmetic Inventory, arithmetic attitude, reading attitude, teacher rating of achievement, and teacher rating of attitude), each posttest score was subtracted from each pretest score. These scores were analyzed two at a time as described by multivariate analyses of variance, and then individually by univariate analyses of variance. First, 2 x 2 analyses were performed; instruction and no instruction were crossed with choice and no choice. Only the four treatment conditions were included in these analyses. It was predicted that choice and instruction main effects each would be significant but that the interaction would not be significant. Then one-factor (five-level) analyses were performed including all five experimental conditions. Tukey (a) post hoc tests were used to analyze particular group differences (Winer, 1971). It was predicted that the combined condition of choice and instruction would be superior to choice with no instruction and instruction with no choice, which would be superior to no instruction and no choice, which would be similar to the no-treatment control. The arithmetic achievement measures were expected to reflect these predictions most strongly, arithmetic attitude was expected to vary significantly and generalize to reading as an example of other school attitudes, and teachers were expected to observe these changes in arithmetic attitude and achievement.

Pretest scores were separately analyzed to test for initial group equivalence by performing a multivariate 2 x 2 (choice crossed with instruction excluding the control group) and one-factor (five-level with all experimental groups included) analyses of variance on each class of dependent variables (arithmetic achievement, attitudes, and teacher observations). Individual univariate analyses of variance were also performed on each of the change measures. Tukey (a) post hoc analyses were planned for any significant one-factor, five-level analysis.

To further analyze the acceptability of using change scores for analyses, a correlation between pretest scores and change scores for each dependent measure was performed. These correlations were tested for significance using a t test. Nonsignificant correlation would suggest that changes in scores did not systematically reflect pretest scores. Significant correlations would mean that part of the variance in change scores was due to differential pretest scores; any deviation in pretest scores would then have to be carefully examined.

For the repeated measures of in-class arithmetic performance, a multivariate analysis of variance and univariate analyses of variance were performed. Choice and no choice were crossed with instruction and no instruction, which included eight subjects in each condition, and both variables were crossed with observations over six-time interval blocks of five or six sessions each. Percentage of attempted problems solved correctly and percentage of available problems attempted were analyzed after an arcsine transformation (2 arcsin x) as recommended by Winer (1971, p. 400). The total number of problems performed correctly over six consecutive sessions was divided by the total number attempted problems to obtain the percentage of attempted problems solved correctly. The total number of attempted problems was similarly divided by the total number of available worksheet problems to get the percentage of available problems attempted. Children were occasionally absent; using ratios of scores totaled over the five or six sessions in each time block effectively resulted in average scores for those sessions of each time block that each child attended. No child attended fewer than two of six sessions. The average number of sessions attended by each child within each time block was 5.3. It was predicted that the main effect of time would be significant reflecting the increasing difficulty of arithmetic units.

CHAPTER III

RESULTS

Pre- and Postexperimental Measures

Pretest, posttest, and change scores on each of the six pre- and postexperimental measures (WRAT, Arithmetic Inventory, reading and arithmetic attitudes, and teacher ratings of achievement and attitude) for each of the 40 subjects are included in Appendix F. Group means and standard deviation presented in Table 1.

Pretest scores. Pretest scores were analyzed in order to check for initial equivalence of groups. Children were assigned to groups randomly, and pretest scores should have been equivalent. There was, of course, one chance in twenty that any particular analysis would be significant at least at the $p < .05$ level. The four treatment groups were analyzed using 2 x 2 analyses with instruction crossed with choice. The results of these analyses are presented in Appendix G. The choice x instruction interaction was not significant for any measure. The main effects of choice and of instruction were not significant for any dependent variable with one exception; the instruction effect of arithmetic attitude was significant ($p < .05$). Although the groups were randomly assigned, the groups that were to receive no instruction initially had significantly better attitudes towards arithmetic. Using Utility Indices (Gaebelein & Soderquist, 1974),

Table 1
Group Means and Standard Deviations for Pretest and Posttest Measures

	WRAT		Arithmetic Inventory		Reading Attitude		Arithmetic Attitude		Teacher Rated Attitude		Teacher Rated Achievement	
	Mean	Sd*	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd
Choice and Instruction Group												
Pretest	1.14	.29	24.88	5.08	6.75	2.49	4.75	3.11	.88	2.75	2.86	.68
Posttest	1.68	.27	34.50	5.24	7.63	1.51	6.63	1.92	2.25	4.13	3.13	.72
Change	.54	.32	9.63	2.50	.88	3.09	1.88	2.64	1.38	3.34	.26	.57
Choice and No Instruction Group												
Pretest	1.05	.34	20.50	10.03	7.75	1.58	7.50	1.41	2.63	4.27	3.01	.91
Posttest	1.54	.43	27.63	10.03	7.25	1.04	6.63	1.06	2.00	2.98	2.90	.47
Change	.49	.31	7.13	3.80	-.50	1.60	-.88	1.13	-.88	3.56	-.10	.74
No Choice and Instruction Group												
Pretest	1.08	.32	20.13	8.03	7.13	1.25	5.50	1.77	.38	5.24	2.80	1.01
Posttest	1.50	.56	32.00	9.23	7.25	1.58	5.88	1.46	1.88	5.44	2.93	.91
Change	.43	.36	11.88	5.64	.13	1.64	.38	2.13	1.50	2.56	.13	.54
No Choice and No Instruction Group												
Pretest	1.14	.35	24.38	10.51	7.13	1.64	6.00	2.14	1.75	3.19	3.03	.80
Posttest	1.58	.47	31.00	8.72	7.13	1.13	5.50	1.77	1.88	2.80	2.86	.77
Change	.46	.41	6.63	3.46	.00	2.27	-.50	1.41	.13	2.10	-.16	1.04
No Treatment Control												
Pretest	1.15	.20	24.75	4.68	6.63	1.92	6.00	2.00	2.13	2.48	3.23	.43
Posttest	1.46	.45	31.63	5.04	7.38	1.41	6.63	1.60	2.38	4.17	3.03	.57
Change	.29	.49	6.88	4.42	.75	1.17	.63	1.85	1.25	3.37	-.08	.43

*Sd--standard deviation

9.42 percent of the variance of arithmetic attitude pretest scores was due to group assignment. The five experimental groups (four treatment and one control group) were analyzed with one-factor (five-level) analyses of variance. These results are reported in Appendix G; none of these analyses was significant.

Change Scores. Changes in scores were analyzed by performing multivariate analyses with two dependent variables on the appropriate pairs of dependent measures (WRAT and Arithmetic Inventory scores, reading and arithmetic attitude scores, and teacher observation of attitude and achievement). Subsequent univariate analyses of variance were performed on each of the six dependent measures.

The change score data for the four experimental groups were analyzed in a 2 x 2 factorial design. Choice and no choice were crossed with instruction and no instruction in three multivariate and six subsequent univariate analyses of variance. The results from these analyses are presented in Appendix H.

The interaction of choice and instruction was not significant for any univariate or multivariate analysis. Also, the main effect of choice was not significant for any analysis. The main effect of instruction was significant for several measures. The multivariate analysis of variance for instruction on achievement was significant at the $p < .05$ level. The univariate analyses on the two achievement measures showed that this multivariate effect was due to the significant ($p < .01$) effect on the Inventory. These results indicated that the two groups that received instruction improved significantly more on the Arithmetic Inventory from pretesting to posttesting than the two groups who did not receive arithmetic instruction. Using Utility Indices (Gaebelein & Soderquist, 1974), the instruction grouping accounted for 17.08 percent of the variance of the Inventory change scores. The multivariate analysis of attitudes was also significant at the $p < .05$ level. The univariate analyses indicated that this effect was primarily due to the single measure of arithmetic attitude, which was significant at $p < .01$ and accounted for 15.16 percent of the variance. The groups receiving instruction changed their attitudes significantly further in a positive direction from pretesting to posttesting than groups that did not receive instruction. The univariate analysis of teacher-rated attitudes tended also to favor groups that received instruction with a probability level less than .10. Instruction grouping accounted for 6.22 percent of the variance of teacher-rated attitudes. Although teacher-rated achievement was not significant, 5.26 percent of the variance of this measure was accounted for by the instruction grouping.

The four experimental groups and the control group were then analyzed in one-factor (five-level) multivariate and univariate analyses of variance; these results are reported in Appendix H. None of the three multivariate or six univariate analyses was significant. However, the Inventory and arithmetic attitude analyses both resulted in probability levels less than .06. Tukey (a) analyses were performed on these two measures in order to determine which groups accounted for these marginal differences; these analyses are also included in Appendix H. No post hoc analyses were significant for the Inventory. For arithmetic attitude, the choice and instruction group was significantly higher than the choice and no-instruction group. For arithmetic attitude and arithmetic achievement, both instruction groups exceeded both no-instruction groups.

Correlation between pretest and change scores. In order to examine whether changes were influenced by the particular distribution of pretest scores, the correlations between pretest and change scores for each of the six dependent measures were computed (Appendix I). The obtained correlations were all negative and, except for WRAT scores, were all significant at least at a $p < .05$ level. These results indicated that for each measure, children who originally scored lowest tended to have larger change scores than children who scored higher. The correlations were smallest for the two achievement measures, intermediate for the teacher observations, and largest for the attitude measures. These results indicate that scores regressed towards the mean from pretesting to posttesting.

The major implication of these findings is with respect to the high correlation between pretest and arithmetic attitude change scores. Since the instruction groups had significantly lower pretest scores, and since low pretest scores were generally associated with larger change scores, one would expect instruction groups to change more than no-instruction groups. The highly significant change, however, does suggest that instruction groups may have changed more than the correlation would predict. The high correlation may result from the effectiveness of instruction combined with the change pretest distribution or, alternatively, the apparent effectiveness of instruction may have resulted from a tendency for lower children to change most, independent of conditions. The tendency for instruction groups to change more in teacher-reported arithmetic attitudes similarly may have been due to lower initial scores in teacher-reported arithmetic attitudes. Initial WRAT and Inventory scores for choice and no-choice groups were not different.

Comparisons of treatments and control groups. The no-contact control group provided some interesting comparisons. The control group's WRAT change score was .29; the means for the four experimental groups ranged from .43 to .54. This nonsignificant difference was the only suggestion that attendance at experimental arithmetic sessions had any advantage (or disadvantage) over no treatment. Among the 40 children pretested and posttested, three had negative WRAT change scores. Two of these children were in the control group and one was in the no-choice and no-instruction group. The control group did not differ from the no-instruction groups on the Inventory, but ranked lower than instruction groups. On both attitude measures, the control group ranked second behind the instruction and choice group. No-instruction groups had negative or zero mean attitude change scores; instruction and control groups had positive mean attitude change scores. On teacher observations of attitude and achievement, the control group means ranked below instruction group means and above no-instruction group means. In sum, the control ranked between the instruction groups and no-instruction group on most measures.

Within-Session Scores

For the four treatment groups, the percentage of attempted problems performed correctly (accuracy) and the percentage of available problems attempted (effort) were computed for each block of sessions and are included in Appendix J. These scores were analyzed using a repeated measure design with the repeated measures of percent correct and percent attempted averaged over blocks of six sessions (time), and with subjects nested within the factors of instruction and choice. Cell means for various combinations of these means are presented in Table 2. Cell means and standard deviations for each instruction x choice x time cell are included in Table 2; means for combined conditions are presented in Appendix K. The results of the multivariate analysis on the two measures and univariate analyses on each of the measures are presented in Appendix L. The main effect of instruction

Table 2

Means and Standard Deviations for Cells in Repeated Measures Analysis*

Choice and Instruction					No Choice and Instruction				
Blocks of Sessions	% Correct Mean	Sd**	% Attempted Mean	Sd	Blocks of Sessions	% Correct Mean	Sd	% Attempted Mean	Sd
B_1	2.7	.31	3.0	.19	B_1	2.7	.29	2.8	.25
B_2	2.7	.28	2.8	.51	B_2	2.8	.21	2.8	.39
B_3	2.8	.32	2.9	.40	B_3	2.8	.24	2.4	.35
B_4	2.5	.44	2.5	.57	B_4	2.6	.24	2.4	.55
B_5	2.6	.45	2.5	.47	B_5	2.4	.28	2.2	.51
B_6	2.4	.18	2.2	.61	B_6	2.5	.27	2.1	.57

Choice and No Instruction					No Choice and No Instruction				
Blocks of Sessions	% Correct Mean	Sd**	% Attempted Mean	Sd	Blocks of Sessions	% Correct Mean	Sd	% Attempted Mean	Sd
B_1	1.5	.74	2.7	.43	B_1	1.5	.67	2.7	.46
B_2	1.8	.68	2.8	.34	B_2	1.8	.54	2.7	.40
B_3	1.7	.54	2.7	.26	B_3	1.9	.44	2.7	.43
B_4	1.8	.67	2.8	.38	B_4	1.7	.41	2.9	.24
B_5	1.7	.55	2.8	.38	B_5	1.9	.78	2.9	.28
B_6	1.5	.51	2.8	.40	B_6	1.9	.59	2.7	.31

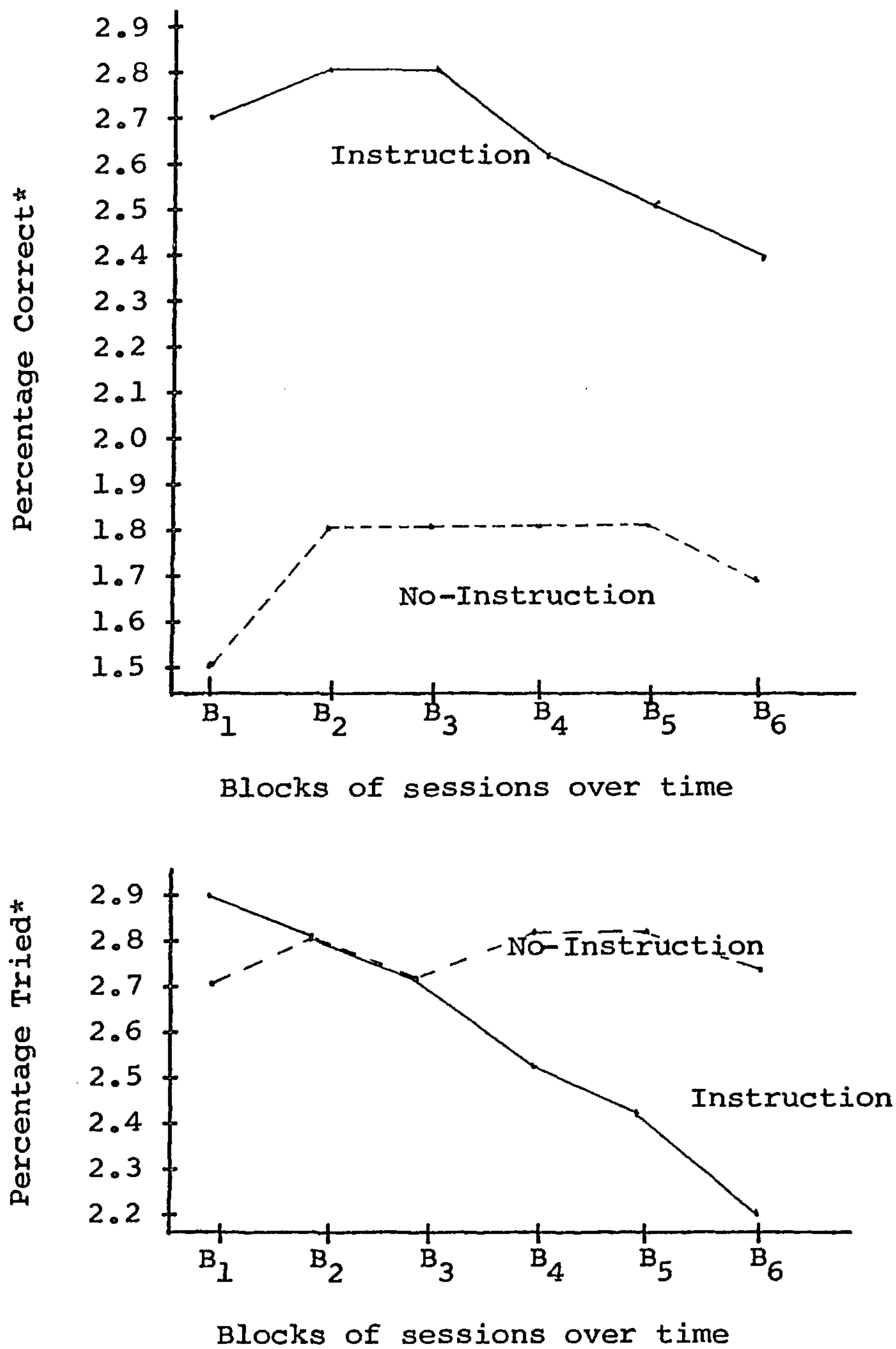
* Eight subjects are averaged over each block of sessions. Arcsine transformations of percentages are presented here.

** Sd--standard deviation.

was highly significant for the multivariate analysis and the accuracy measure and approached significance on the effort measure ($p < .07$). The instruction groups performed a higher percent of attempted problems correctly and tended to try a higher percentage of problems than the no-instruction groups. Since the instruction groups correctly solved more problems, they progressed more rapidly through the worksheets. Children in the instruction groups averaged 25.0 worksheets over all sessions; children in the no-instruction groups averaged 15.4 worksheets.

The main effect of time and the interaction of time with instruction were significant at the $p < .01$ level for the multivariate and both univariate analyses. For the main effect of time, Tukey (a) post hoc analyses were performed on each dependent variable (Appendix L). The first and last blocks of sessions had the smallest percentage correct for all subjects. The second and third blocks were significantly larger than the last block, and the third block of sessions was significantly larger than the first block. For the percentage attempted, subjects averaged highest in the first session block and decreased each subsequent block. The first and second sessions were significantly higher than the last session.

The main effects of instruction and time are further clarified by looking at the significant interaction between these two factors (see Figure 1). For percentage correct,



*Arcsine transformations of percentages were used.

Figure 1. Interaction between instruction and blocks of sessions over time, for percentage correct (upper panel) and for percentage tried (lower panel).

instruction groups were clearly superior to no-instruction groups over all time intervals. Both instruction and no-instruction conditions showed better performance on the second block of sessions than the first. The no-instruction groups then maintained the same average performance level, decreasing only slightly in the last time interval. The instruction groups' performance decreased continuously for the last three session blocks; the discrepancy between the patterns for instruction and no-instruction groups accounts for the significant interaction. For percentage tried, the instruction groups averaged larger scores in the first time interval and decreased each session. The no-instruction groups' performances remained stable over time. The main effect of time was therefore due solely to the instruction groups. The almost significant main effect of instruction was due to the average score of instruction groups being lower than the average scores of no-instruction groups; the interaction indicates more clearly the actual pattern of scores.

CHAPTER IV

DISCUSSION

The instruction groups in the current study were similar to resource room conditions in many schools. Children with special needs such as learning disabilities or educable mentally retarded are identified and leave their classrooms for a period each day for extra instruction in a resource room. Similarly, children in this investigation were singled out for special attention based on their test scores; they left their classes on a regular basis for small-group instruction, and they were taught using an individualized instruction program. The no-instruction groups differed from the instruction groups in that the former did not receive instructions, prompts, or feedback, but they did receive the same amount of social attention.

The combination of feedback and prompts that the instruction groups received was effective in improving arithmetic performance. Arithmetic performance was measured by accuracy on arithmetic problems during each session, the Arithmetic Inventory, the WRAT, and teacher ratings of achievement. Instruction groups consistently performed a higher percentage of problems correctly during the experimental sessions. On the Arithmetic Inventory, instruction groups improved in performance significantly more than the no-instruction groups; this finding indicates that performance on arithmetic problems generalized to performances on a test similar to the particular arithmetic problems performed during sessions. Achievement gains were not apparent on the WRAT, a less direct measure of arithmetic achievement. Its emphasis is on general arithmetic skills, and it has only a few items at each particular skill level; thus, it is relatively insensitive to small increments in arithmetic skills. Similarly, teachers did not report greater arithmetic improvement for children in instruction groups compared with children in no-instruction groups. It is possible that longer exposure to instruction would have produced significant differences on the WRAT and on teacher ratings. These results suggest that resource rooms and classroom instruction can measurably improve children's performances in the particular skills taught but that generalization to more general skills or other situations may be limited or take much longer to be evident. Specific arithmetic achievement tests such as the Inventory and classroom performances may provide better measures of small gains in specific skills.

The instruction groups changed more positively than no-instruction groups in arithmetic attitude. This measure, unfortunately, was confounded by the fact that the instruction group children were significantly lower on arithmetic attitude than no-instruction groups on the pretest measure. Correlational data indicated that children with low pretest attitudes changed further in a positive direction than children with high pretest attitudes. The grouping accounted for 9.42 percent of the variance of arithmetic attitude pretest scores and 15.16 percent of the variance of change scores suggesting a positive instructional group effect. It cannot be determined conclusively with these data whether the observed correlation reflected greater improvements in the instruction groups' attitude or whether the improvements in instruction groups were due to a general tendency for low attitude children to change positively regardless of group assignment. The change scores were significant at a .01 probability level whereas the pretest scores were significant at .05, thus suggesting more than a regression or other statistical phenomena. It is concluded that instruction affected arithmetic attitude positively, although the strength of this effect cannot be determined. There was no measured generalization of improved arithmetic attitudes to reading attitudes.

Child attitudes toward arithmetic as rated by teachers showed some tendency to change most positively for children in instruction groups; however, there was a slight tendency for children in instruction groups to have reportedly poorer attitudes than no-instruction groups at pretesting. At posttesting, children in the instruction and no-instruction groups had similar group means on teacher observed arithmetic attitudes.

Comparisons among the four treatment groups and the nocontact control group showed that, with the exception of WRAT scores, the mean control group scores ranked below instruction groups and above no-instruction groups. The control group did change positively on both achievement measures; these children received arithmetic instruction in their regular classrooms. The control group ranked lower than the treatment groups on the WRAT. Children in the no-instruction condition were singled out for special help in arithmetic and thus may have perceived that they performed arithmetic at a low level. These children also saw that they progressed little during sessions. Attention without instruction may therefore be worse than no extra attention. This finding would support the North Carolina and Federal policies that children must be carefully tested before attending special classes and that special classes instruct children according to individually prescribed programs (see Rules Governing Programs and Services for Children with Special Needs, 1976).

On the within-session measures of effort and accuracy, there were significant interactions of instructions with time. The instruction groups decreased over time on effort and accuracy; no-instruction groups stayed at a constant level on each measure. For accuracy, the instruction group was well above the no-instruction group; for effort, the mean of the no-instruction group had a tendency to be higher than the instruction group. The tendency for instruction group effort and accuracy to decrease over time probably reflects the increasingly difficult series of units. Instruction groups progressed more rapidly than no-instruction groups and thus received more difficult worksheets sooner. Worksheets were usually easily completed when children worked for the full session. Children in instruction groups did not attempt as high a percentage of problems on difficult worksheets as they did on initial, easier worksheets. Interestingly, none of the no-instruction children complained or commented upon the fact that they were receiving no instruction.

This investigation represents the first clear experimental demonstration that prompts and feedback are more effective than attention alone. Previous studies have compared types of instructional programs (Ellson, Harris, & Barber, 1968; Ronshausen, 1972) or feedback alone without examining attention or prompts. The effectiveness of instructional variables including prompts, feedback, attention, and exposure to materials has usually been assumed. The present study provided work for each child contingent on his performance and demonstrated that feedback and prompts were needed in addition to worked examples and units with problems of progressive difficulty. Other procedures may also be effective for teaching children; for example, the materials may be programmed to give feedback and instructional prompts. At least with the materials used in the present study, children were not able to learn without feedback and prompts.

Choice of materials was the second major variable of interest in the present investigation. Children in choice groups selected both their worksheets and their counting objects; children in no-choice groups were handed their materials each session. Children given a choice often spent several minutes examining each worksheet and selecting their objects; a few children would look back in the experimental room to match their choices with a peer. Few children appeared to notice that worksheets presented for choice differed only with respect to color. Children in no-choice groups occasionally seemed dissatisfied with their materials and asked for alternatives.

There was no evidence that choice was more beneficial than no choice.

Choice was expected to have motivational impact on arithmetic behavior; but, contrary to expectation, the attitude, achievement, and effort measures showed no effect of choice. Teachers rated no difference between children in choice and no-choice conditions, and there were no measured differences of accuracy or effort within sessions.

There are several possible explanations for the lack of observed effect of choice. The manipulation may have been ineffective, so that no choice was perceived by the children in choice groups. However, the observed behavior during choosing and the complaints by the no-choice children suggest that the observed results were not in fact due to a lack of perceived choice.

A second possible explanation is that choice of materials has either a transient or a null motivational effect on arithmetic performance. The only research study that examined choice of materials (Kulkin, 1972) found an immediate effect on the reading performance of fifth-grade children. The present investigation looked at changes over a relatively long period of time on first-grade children; however, no differences between choice and no-choice groups were evident even in the first block of sessions. Selection of reading passages may allow children to pick more interesting and thus motivating materials; the behavior of selecting itself may have had no effect.

In the present study, selection did not result in more or less interesting materials but only the perception that the child had chosen the materials. Thus, children may be able to select reinforcing or interesting reading passages, but the act or perception of selection alone when final materials are held constant may not alter motivation. Choice of materials may, similar to choice of reinforcement criteria, have little or no effect on performance in learning situations. A study in which children chose reading titles but all received the same passage (with different titles) might help answer this question.

An additional conceivable explanation for the disparate results of this study and Kulkin's investigation may be rooted in developmental and learning phenomena. Choice may be associated with positive, reinforcing materials and activities in learning situations and thus acquire motivational properties rather gradually. Hence, choice may be motivational only to older, school-wise children and to adults. For example, Kulkin found choice of materials for fifth-grade children to be effective. Brehm and Cohen (1959) found that choice of toys by sixth-grade children increased toy ratings, and Brehm (1956) found that choice of objects by adults increased subsequent ratings of the objects, suggesting some motivational value of choice. This reasoning is contrary to the idea suggested both by Durkin (1974) and Deci (1975) that choice is innately desirable. Further investigation of the motivational value of choice at different age levels may answer some of these questions.

The present research findings on choice suggest that the common notion that choice of classroom activities is motivational and causes positive affect towards chosen tasks and, therefore, better performances may not always be true, at least for first graders. An expanding literature (i.e., de Charmes, 1968; Deci, 1971, 1975) suggests that choice of an object or task increases the motivational value of that choice. When one perceives that he is responsible for his behavior, subjective rating, or choice of a particular object or activity, he attributes the action to himself and not to external agents. In effect, the person likes the activity (or object) better for having chosen it; after all, who would freely choose something he did not like (Deci, 1975)? The present study tested the application of this theory, supported by controlled research studies, to a classroom, field situation. With all other variables held constant, choice of materials did not increase arithmetic performance or arithmetic attitude. More field studies would further clarify the value of using choice to motivate young children in ongoing school situations.

CHAPTER V

SUMMARY

Research in education has focused primarily on instructional packages rather than instructional processes. For example, "free schools" have frequently been compared with traditional instruction; the former usually show a small advantage. The instructional packages included under the label of free schools vary in emphasis and design, and the variables used to assess child progress are usually general and broad. The present investigation was concerned with examining specific components or variables of instruction in the learning process, using objective criteria. In particular, the additional value of instructional prompts and feedback over exposure to materials and attention, and the motivational value of choosing one's materials were examined. Progress was assessed using the following eight measures: general achievement, specific achievement, attitudes towards math, attitudes towards reading, teacher rating of math attitudes, teacher rating of math achievement, within-session accuracy, and within-session effort.

With the exception of a few studies that have investigated variables such as the kind of feedback given for performance, research involving instructional technique has compared new programs with regular programs or occasionally with no-treatment controls. Instruction in this investigation consisted of three levels: no-contact (control), exposure to materials and attention (no-instruction), and exposure to materials, attention, feedback, and instructional prompts (instruction). At posttesting the instruction groups were superior to the no-instruction groups on within-session accuracy and specific arithmetic achievement. The instruction groups also changed more positively on arithmetic attitude, but this measure was confounded by significant pretest score differences. It was concluded that the instruction groups did change more positively on arithmetic attitudes, but the strength of this effect was not determined.

Instruction groups decreased over time on measures of within-session effort and accuracy; this likely reflects the increasingly difficult worksheets to which these children were advanced. The no-instruction group ranked at or below the control group on five of the six measures (control groups were not assessed on the two within-session measures), and the instruction group ranked at or above the control group on all measures. This study provides evidence that instructional prompts and feedback are more beneficial than attention and exposure to materials alone; this latter condition may be worse than no contact at all. The kind of instruction given to children in resource rooms includes prompts and feedback and so should benefit children on specific measures of achievement and attitude similar to the instructional group in this study. The no-instruction condition involved children being singled out for special attention who may therefore have perceived that they were special and have noticed that they did not progress well in arithmetic.

The motivational role of choice in the learning process has been poorly investigated. Dissonance theory (Jones & Gerard, 1967) suggests that if a person is given a choice, the chosen object increases in subjective value. This would imply that chosen arithmetic materials are more valuable and thus probably more motivational to children. De Charmes (1968) argues that people strive to be original; thus the act of choosing is itself motivational. Deci (1971, 1975) expanded de Charmes' hypothesis and provided supporting evidence. Several studies have examined the result of choosing one's reinforcement criteria on performance; results are equivocal. Kulkin (1972) examined the role of choice of materials and choice of reinforcement on the reading performance of fifth-grade children; choice of materials increased reading performance, but the latter condition was not effective.

The present study provided no evidence that choice was different from no choice of materials. These results could have stemmed from an ineffective manipulation; however, the children's behavior suggested that they perceived a choice. Choice of materials may not effectively motivate performance. It is likely that in Kulkin's study, choice resulted in materials that actually were more interesting to each individual child. A final explanation for the obtained results is that children may learn to value choice, so that first-grade children may not yet have acquired this value.

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Appendix A
Units of Instruction

Concept	Worksheets	Tasks with materials
1. same thing	circle object same as first two objects, circle both if same	hold up object, "find one that is the same"
2. goes together	circle object that goes with first (fish and bowl)	name things that could go with object
3. matching 1-1	draw lines from fish to bowls, etc.	pair up sets of objects that are the same
	place one object over each mark on sheet	
4. same/different	draw lines from fish to bowls, draw in extra when needed to match	pair two sets of objects to see if same. Add to make same.
5. number 1	name numeral 1 circle sets of one draw lines from 1 to sets of one	respond to "give me one," "hold up one" count 1 out loud to "how many"
	count 1 out loud	
6. number 2	(similar to 1)	
7. discriminate 1&2	circle sets of 1 or 2 by identifying numeral draw lines from 1 to sets of one and 2 to sets of two count out sets of 1 & 2	responds to "give me one," "give me two," "hold up one," hold up two" count sets of 1 & 2 to "how many"
8. number 3	(similar to 1)	
9. discriminate 1,2,3	(similar to 1&2)	
10. equal sets: 1+1=2	match separated sets with joined sets (x x with xx)	conservation of objects--two objects apart are like two together
	place objects in spaces:	
11. equal sets 1+2=3	(similar to 1+1)	

Concept	Worksheets	Tasks with materials
12. equal sets 2+1=3	(similar to 1+1)	
13. equal sets 2+1= 1+2=1+1+1=3	(similar to 1+1)	
14. number 4	(similar to 1)	
15. discriminate 1,2,3,4	(similar to 1&2)	
16. more	circle set with more drawing matching lines if needed	make sets with more objects identify which of two sets has more
17. less	(similar to more)	
18. more, less, same	circle set with more, x set with less, box sets with same	identify by request whether a set has more, less, or same
19. >,<	put correct symbol on line between two sets.	symbol on paper, generate sets on either side that fit
2 . how many more	dr w enough to make sets same, count how many drawn	add objects until same, count how many
21. numbers 5 & 6	(similar to 1)	
22. discriminate 1 - 6	(similar to 1 & 2)	
23. numbers 7-10	(similar to 1)	
24. discriminate 1-10	(similar to 1 & 2)	
25. more, less, same, how many more	ring set with same circle set with more, x set with less, box set with same draw objects so same	add objects so same answer how many more to make same

Concept	Worksheets	Tasks with materials
26. plus up to 4	1 + 2 = ______ draw balls, etc. and answer how many	place objects under numerals and write how many
27. all the same through 10	2 and 1 matches 3, etc. use pictures with spaces match pictures without spaces	objects spaced apart match objects together
28. plus up to 7	(similar to 4)	
29. plus up to 10	(similar to 4)	
30. first	read word and symbol circle 1st in row write symbol	point to 1st object in row
31. second	(similar to 1st)	
32. discriminate first and second	circle 1st or 2nd in response to written symbol	point to 1st or 2nd in row
33. third - fifth	(similar to 1st)	
34. fourth - tenth	(similar to 1st)	
35. discriminate	(similar to 1st and 2nd)	
36. review plus to 10	(similar to plus to 10)	
37. numerals 11 and 12	(similar to 1)	
38. numerals 13 - 19	(similar to 1)	
39. discriminate 11 - 19	(similar to 1 & 2)	
40. disciminate 1 - 19	(similar to 1 & 2)	
41. plus to 19	(similar to plus to 4)	
42. missing added up to	1 + ____ = 4 draw balls, etc. and answer how many	place objects under numerals and write how many

Concept		Worksheets	Tasks with materials
43.	missing addend up to 10		(similar to up to 4)
44.	missing addend up to 19		(similar to up to 4)
45.	numerals 20 - 29 discriminate 20 - 29		(similar to numeral 1 and discriminate 1 & 2)
46.	discriminate 1 - 29		(similar to 1 & 2)
47.	subtract up to 4	3 - 1 = ____ draw balls, etc. and answer how many	place objects under numerals and write how many
48.	subtract up to 10		(similar to up to 4)
49.	subtract up to 19		(similar to up to 4)
50.	subtract up to 29		(similar to up to 4)
51.	numerals 30 - 39 discriminate 30 - 39		(similar to numeral 1 and discriminate 1 & 2)
52.	discriminate 1 - 39		(similar to 1 & 2)
53.	add in columns 1 digit	1 +3 , etc. using drawings	add in columns using objects to count
54.	add in columns 2 digit and 1 digit, no carry, to 39	11 + 7 etc. using drawings	add in columns using objects to count
55.	add in columns 2 digit numbers, no carry, to 39	12 +10, etc. add one's and then ten's draw objects to check answers	add in columns and use objects to check answers

Concept	Worksheets	Tasks with materials
56. numerals 40 - 49 discriminate 1 - 49		(similar to numeral 1 and discriminate 1 & 2)
57. add in columns 2 digits, no carry, to 49		(similar to add to 39)
58. carry 1 to tens, 1 digit + 2 digits	15 + 7 , etc. add one's, show how to carry the one, and then add tens.	use objects to check answers and help count
59. carry 1 to tens, 2 digit + 1 digit		(similar to 1 digit + 2 digit)
60. carry 1 to tens, 2 digit + 2 digit		(similar to 1 digit + 2 digit)
61. discriminate carry from not carry	give problems both involving carrying and not	use objects to check answers and help count

Appendix B
Sample of Session Dialogue

Instruction Group

Four children (T, K, D, and Tr) were in attendance on this day with the experimenter (Ex).

T- I want a drink of water. (begin with Tim)

Ex- No. Ready to start? Where's your pencil? Where is it? (inaudible)

Ex- What does this say?

T- Three plus seven.

Ex- Three plus what number is...

T- Seven.

Ex- Ok. Do you remember how to do that?

T- No.

Ex- Yes you do, how many circles do you draw?

T- Three, four,...umh, six.

Ex- Don't go over there, keep drawing.

T- OK. Six, Seven, Eight, Nine.

Ex- Don't go up there just yet, OK?

T- Do what? Ten, eleven.

Ex- How many are you supposed to draw?

T- Seven and its done.

Ex- Show me where its....

T- No. (inaudible)

Ex- That's not correct, come on.

T- I don't know what to do.

K- Let me see; I can't do that either. (moved to K)

Ex- Let me help you with the problem. There's supposed to be just seven right there and then you cross out three.

K- There is seven.

Ex- You have to circle first; circle the first and circle the second in each row.
OK. I'll be there to help in just a minute. OK? (to Tr)

K- One and one.

T- We'll have all I want.

Ex- Would you please sit down and stop?

T- I don't feel well. I've got some so...Brrrrr.

Ex- Can you cross out three and see what is left?

T- Hey.

Ex- Good. Which one do you want?

K- One.

Ex- Karen? One plus one. Two, what is two take away one?

T- Hey come here. I'm having a little problem. (Karen inaudible)

Ex- Right.

T- That one's not seven.

Ex- Three take away one is...

K- Two. (move to T)*

Ex- Very good Karen. OK, Tim, did you draw seven circles?

T- Yea.

Ex- Did you cross out 3?

T- Yea.

Ex- Did you count how many were left?

* T was a particular behavior problem and the experimenter would occasionally see him out of turn momentarily when he began good behavior.

T- Yea.

Ex- Did you put it in the box?

T- Here's the (inaudible)

Ex- OK. Good. You're through with that. Put it away.

K- Hey Tim, me and yous got the same sheet. (moved to D)

Ex- Those down here are a little easier.

D- That's the box that I'm writing around.

Ex- OK. Now, Do you know what to do with this?

D- No.

Ex- What's three plus one? Three plus one, what is it?

D- I don't know what it says here.

Ex- How about this?

D- It says "one."

Ex- Um-huh. So three plus one is four and that makes it right. What is two plus....

D- (Inaudible)

Ex- Two plus any number? How do you do it? Remember? Draw it here. One, two, three, alright.

D- (Inaudible)

Ex- How many are crossed out?

K- Don't mess with this.

Ex- How many are left?

K- Don't play, don't bother.

Ex- What's two plus two?

D- It's four.

Ex- Right. Now you do these. Karen this is your seat here.

D- One, two, three.

Ex- Very good. This is the first.... (Moved to Tr)

D- One, two, three, four, five, six, seven, eight, nine.

Ex- You try to do the work by yourself. First you take... Dorothy, sit.

T- That's not Dorothy's.

Ex- OK. I'm going to hide this paper now and you write "first."

(Tr. inaudible)

D- N...N...Now I right here.

Ex- Let me write...

T- "St," First.

D- Got 'er.

K- We got the same paper.

Ex- Tim you do your paper. Karen will you do your paper?

T- That's cause our...

Ex- Can you remember. Tell you what. You look at this and you practice it and when I come back by I'll have you try again. OK?

T- I haven't finished my homework yet. (moved to Tim)

Ex- Three plus what number went in. There, we already went through.

T- I told you seven.

Ex- Uh-uh, What's three plus seven?

K- Ten.

Ex- What's three plus seven?

T- I don't know and that's the problem with me.

Ex- Wait a minute. Remember when I asked you to draw seven circles and you wouldn't do it?

T- I did it.

Ex- OK, let me see you draw seven circles.

T- One, two, three, four, five, six.

Ex- Let me see you draw them. Draw seven circles.

T- It's already done.

Ex- No, there's more than seven. I want you to draw seven.

T- It ain't more than seven.

Ex- Draw them. They're all (inaudible) I can't see them. When you draw them let me know and I'll help then.

K- Tim. (moved to K)

Ex- Oh, you're doing a good job. OK.

D- OK. Is that right?

EX- Did you draw seven circles? Cross out three?

D- Is that right? Is that right?

Ex- Yes.

D- Is that supposed to be there?

Ex- How many are left?

K- Two.

(and so forth)

No instruction group

Four children (M, P, K, F) were in attendance on this day with the experimenter (Ex). (Start with M)

Ex- Mark, you got started on your work. That's good.

F- (sings)

K- Next time Paula, Mark, Mark.

Ex- Can you do this one for me? I'm sure that you can do it. You're supposed to put in the missing addend.

F- Six and nine is twelve.

K- Fondra, don't drink your milk. Don't drink your milk yet.

F- How is it? Miss Graves said don't drink your milk yet 'til you finish your work, then you drink your milk.

Ex- You work on that part. You did a real good job on this part.

K- See me, awww.

Ex- You started on your work too! (Move to P) You're supposed to draw lines to the sets with the same number. Very nice work. Real good. Work on the other side.

K- What's this got a hole in it for? (groove on desk for pencil)

Ex- Very good.

K- Why's this got a hole in it for?

F- What?

K- This. Why's it got a hole in it for?

F- What's this?

P- It's a pencil holder.

K- I know but it's a hole...

Ex- What are you doing on here?

P- Draw a house.

Ex- OK.

K- One, two, three, four, five, six

P- (Inaudible)

Ex- Did you? (move to K)

Ex- Oh, I'm glad to see you starting on this. That's good. You're supposed to do plus.

P- I ain't gonna never fall off this stage again.

F- (Inaudible)

Ex- Uh-huh.

K- That's ten.

Ex- Very good, that's right.

K- Why you put dots on paper like this?

Ex- Well, I don't know, it just didn't come through very well.

K- That's real good.

Ex- Uh-huh. Very good, Kim, you're good at that adding.

F- I finished my work. (move to F)

Ex- You finished? Let me see. OK, it says up here to draw lines to the set with the same number. Is that what you did?

K- Uh-oh! There they are.

Ex- Very good Fondra. Draw lines to sets with the same number.

F- Wanna see this?

Ex- OK.

F- This is a game.

Ex- A game? What kind of game?

F- You know.

P- What kind of game?

Ex- A tic-tac-toe kind of game?

F- Uh-huh. Look, see. See that?

Ex- Uh-huh.

F- (inaudible)

Ex- Tell me who wins.

Ex- Whoops! I'll get that for you, Mark. (Moved to M)

K- Oh.

Ex- Did you finish this side? Oh Mark, you did a good job. Very nice. They all look right. That's real good. Can you work on this side? It's the same thing. This side says, "Fill in the missing addend."

M- OK.

Ex- Think hard about that one.

Ex- What are you doing Paula? (Moved to P)

P- (inaudible)

Ex- What?

P- Something.

Ex- Something. What are you doing with those paper clips? You drew a house.

P- Of course. I told you I would

Ex- I know, but I didn't see it.

P- (inaudible)

Ex- What?

P- I always do a house.

Ex- Uh-huh, you always do.

P- Last Sunday was my Grandmother's birthday.

Ex- Was it? Did she have a good birthday?

P- I have two grandmothers.

Ex- Uh-huh.

K- Fondra?

F- Huh?

K- You're supposed to drink your milk.

F- Huh?

K- You're supposed to drink your milk.

P-- My mommy carried me.

Ex- She what?

P- My mommy carried me. I did it with her.

Ex- Did you? Are you going to do anything else for her?

K- That's good. My daddy won't. My daddy won't let me go with my momma cause she both of them breaking up. Guess who's Mom.

Ex- You see what else you can do on this.

Ex- Hey, Kim. (moved to K)

K- Guess who's mom.

Ex- Who?

K- I am cause my sister, my little sister's my other sister's five and I'm older.

Ex- Right.

K- I know. I-I-I...

Ex- You're supposed to help a whole lot. Let me see your work. That looks very good, Kim. Right?

F- (singing)

Ex- Where'd you learn how to do this?

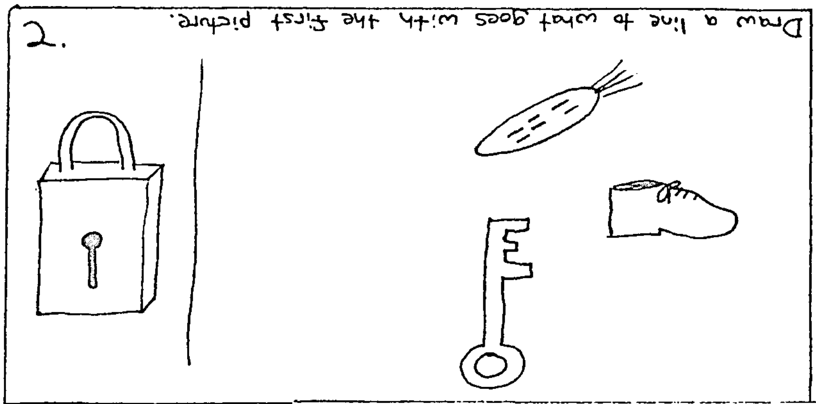
K- It's easy. I...that's three. That's easy. I...

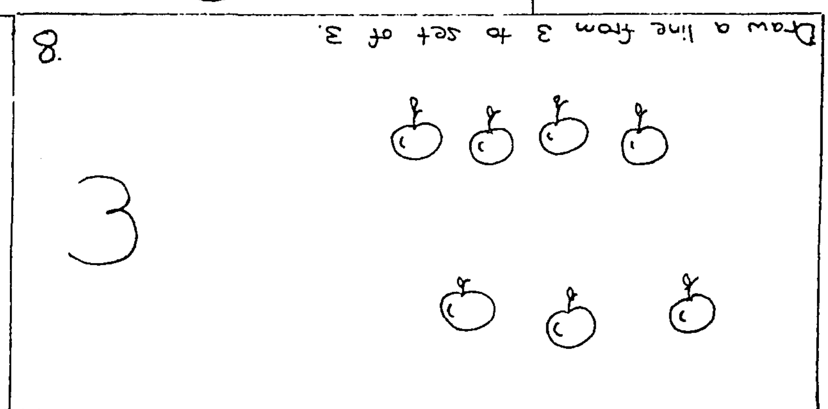
Ex- That's right. It's hard but I'm sure you can get it. You think about it. See if you can get this one.

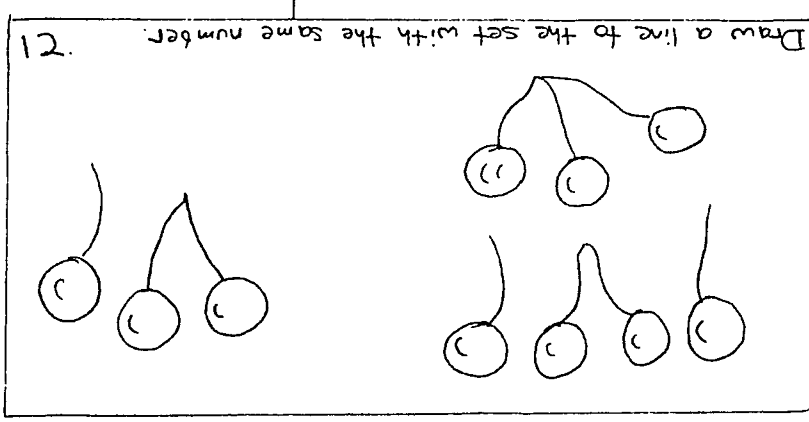
(and so forth)

Appendix C

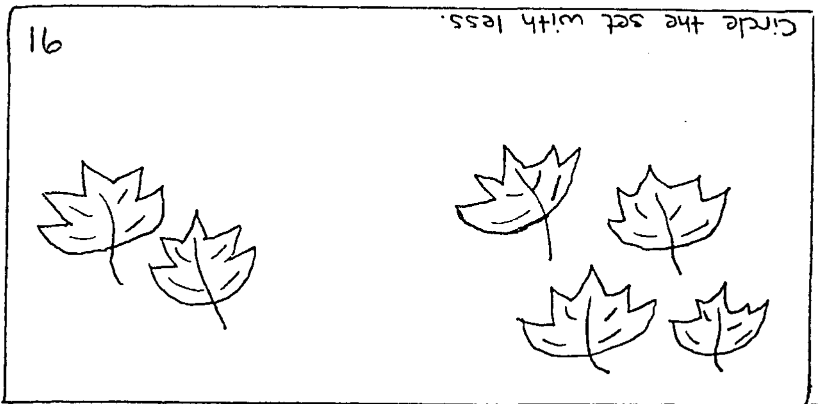
Arithmetic Inventory*

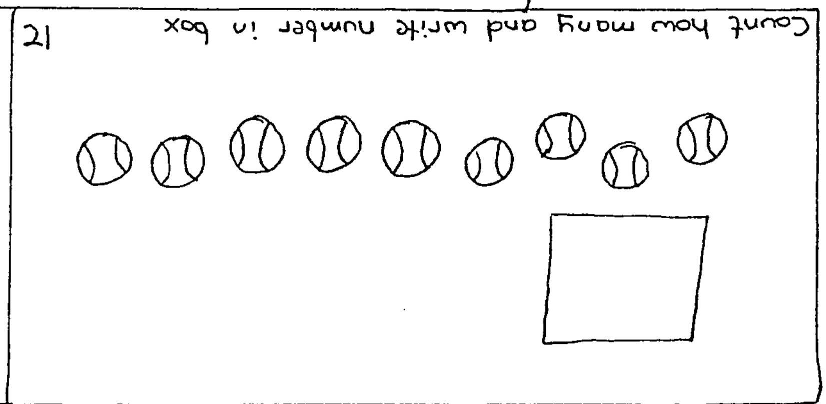


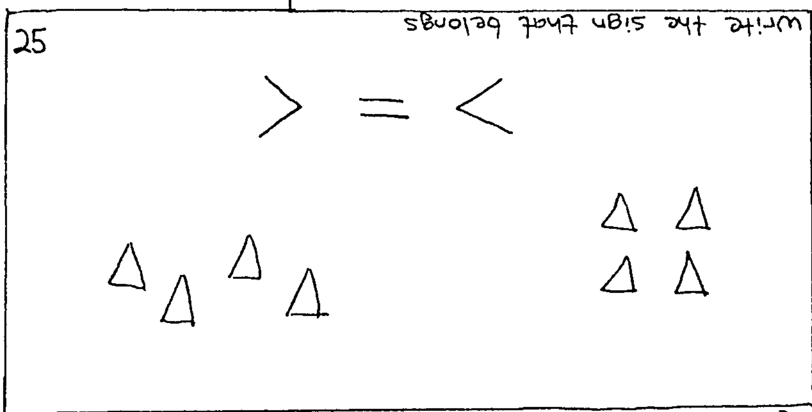


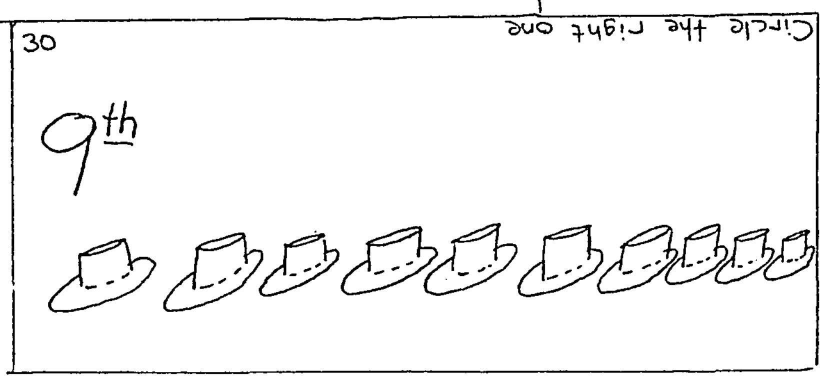


* A representative sample of items are presented from the Inventory.


16
Circle the set with less.


21
Count how many and write number in box


25
Write the sign that belongs
> = <


30
Circle the right one
9th

Draw this many circles (point)

33.

17

Write the answer in the box

35. 12 + 4 = □

36. 3 + □ = 5

Write the answer under the line

43.
```
  5
- 3
___
```

46.
```
 18
+ 6
___
```

52.
```
 16
+21
___
```

Write the number that comes next

53.

39 40 □

Appendix D

Child Attitude Questionnaire*

____1(M) Do you have a hard time doing your arithmetic work? (yes, **no**)

____2(M) Do you hate arithmetic? (yes, **no**)

____3(M) Do you like numbers? (**yes**, no)

____4(M) Is arithmetic fun or not fun? (**fun**, not fun)

____5(R) Do you like reading OK? (**yes**, no)

____6(R) Do you have to read too much in class? (yes, **no**)

____7(R) Do you like books? (**yes**, no)

____8(R) Is reading hard or easy? (hard, **easy**)

____9(M) Is arithmetic your best subject? (**yes**, no)

____10(R) Is reading fun or not fun? (**fun**, not fun)

____11(M) Would you rather get blocks to count with or to stack high? (**count**, stack)

____12(R) Do you have a hard time doing your reading? (yes, **no**)

____13(R) Do you hate reading? (yes, **no**)

____14(R) Would you rather have a book to read or a book for coloring? (**reading**, coloring)

____15(M) Do you like arithmetic OK? (**yes**, no)

____16(R) Is reading your best subject? (**yes**, no)

____17(M) Do you have to do too much arithmetic in class? (yes, **no**)

____18(M) Is arithmetic hard or easy? (hard, **easy**)

*The order of the questions was determined by a table of random numbers. For statistical analysis, the arithmetic questions (prefixed with an M) were separated from the reading questions (prefixed with an R). Arithmetic and Reading questions correspond one-to-one. Underlined answers were scored plus.

Appendix E

Teacher Questionnaire

Child Attitudes Towards Math

____1. Does (s)he have a hard time doing his(her) arithmetic work? (yes, <u>no</u>)

____2. Does (s)he hate arithmetic? (yes, <u>no</u>)

____3. Does (s)he like numbers? (<u>yes</u>, no)

____4. Is arithmetic fun or not fun for him(her)? (<u>fun</u>, not fun)

____5. Is arithmetic his(her) best subject? (<u>yes</u>, no)

____6. Would s(he) rather count with blocks or stack them high? (<u>count</u>, stack)

____7. Does (s)he like arithmetic OK? (<u>yes</u>, no)

____8. Does (s) he think (s)he has too much arithmetic in class? (yes, <u>no</u>)

____9. Is arithmetic hard or easy for him(her)? (hard, <u>easy</u>)

Child Achievement and Behavior in Math

____1. Does he perform up to his potential in math in your opinion?
Never Seldom Occasionally Often Always

____2. Has this child shown accelerated learning of math during the last month?
Worse No change Slight Fair Much

____3. Does this child attempt all math problems given to him?
Never Seldom Occasionally Often Always

____4. Does he participate in math circles?
Never Seldom Occasionally Often Always

____5. How does this child rank in math relative to the other children in his class?
Low Somewhat low Average Somewhat high High

Appendix F

Individual Pretest, Posttest, and Change Scores on Pre- and Postexperimental Measures

		WRAT	Arithmetic Inventory	Reading Attitude	Arithmetic Attitude	Teacher Rating/Attitude	Teacher Rating/Achievement
Choice and Instruction							
Angela	Pre	1.2	29	8	6	-1	2.8
	Post	1.9	40	9	7	3	3.4
	Change	.7	11	1	1	4	.6
Darin	Pre	0.9	22	7	9	-1	2.6
	Post	1.6	32	8	8	3	3.6
	Change	.7	10	1	-1	4	1.0
Sharon	Pre	0.9	17	8	1	-1	2.6
	Post	1.2	28	4	3	-3	2.2
	Change	.3	11	-4	2	-2	-.4
Jill	Pre	1.4	22	8	3	6	3.5
	Post	1.8	35	8	9	7	4.0
	Change	.4	13	0	6	1	.5
Tonya	Pre	1.6	26	7	2	-1	4.2
	Post	1.6	34	8	7	5	3.6
	Change	0	8	1	5	6	-.6
Tim	Pre	1.2	33	6	7	-1	2.0
	Post	2.1	44	8	6	-5	2.0
	Change	.9	11	2	-1	-4	0
Steph	Pre	1.2	28	1	2	3	2.6
	Post	1.6	33	8	5	5	2.8
	Change	.4	5	7	3	2	.2

		WRAT	Arithmetic Inventory	Reading Attitude	Arithmetic Attitude	Teacher Rating/Attitude	Teacher Rating/Achievement
Choice and Instruction							
Tracy	Pre	0.7	22	9	8	3	2.6
	Post	1.6	30	8	8	3	3.4
	Change	.9	8	-1	0	0	.8
No Choice and Instruction							
Brian	Pre	1.2	20	7	4	3	3.8
	Post	1.9	37	5	7	5	3.0
	Change	.7	17	-2	3	2	-.8
Karen	Pre	1.4	34	7	7	-3	1.8
	Post	2.2	42	8	6	-3	2.2
	Change	.8	8	1	-1	0	.4
Susan	Pre	0.9	22	5	6	1	4.0
	Post	1.0	31	7	5	5	3.4
	Change	.1	9	2	-1	4	-.6
Dorothy	Pre	1.0	24	8	8	9	3.0
	Post	1.0	32	9	6	6	3.4
	Change	0	8	1	-2	-3	.4
Christy	Pre	.9	21	7	5	2	2.6
	Post	1.6	29	5	4	3	2.8
	Change	.7	8	-2	-1	1	.2
Scotty	Pre	.6	7	8	3	-2	2.0
	Post	.6	14	7	7	1	2.4
	Change	0	7	-1	4	3	.4

		WRAT	Arithmetic Inventory	Reading Attitude	Arithmetic Attitude	Teacher Rating/Attitude	Teacher Rating/Achievement
No Choice and Instruction							
Johnny	Pre	1.0	12	6	4	-9	1.4
	Post	1.8	28	8	4	-9	1.6
	Change	.8	16	2	0	0	.2
Reginald	Pre	1.6	21	9	7	2	3.8
	Post	1.9	43	9	8	7	4.6
	Change	.3	22	0	1	5	.8
Choice and No Instruction							
Tim	Pre	1.2	21	9	9	3	3.0
	Post	1.2	30	7	8	1	3.2
	Change	0	9	-2	-1	-2	.2
Fondra	Pre	.9	9	5	7	-3	2.3
	Post	1.2	19	7	7	-1	2.4
	Change	.3	10	2	0	2	.2
Paula	Pre	.6	10	9	9	-3	1.2
	Post	1.2	22	9	6	-1	2.6
	Change	.6	12	0	03	2	1.4
Bobby	Pre	1.4	25	9	8	9	3.8
	Post	1.8	26	6	6	6	3.6
	Change	.4	1	-3	-2	-3	-.2
Ronald	Pre	.7	13	6	5	1	3.0
	Post	1.2	15	7	5	1	2.6
	Change	.5	2	1	0	0	-.4
Sally	Pre	1.6	39	7	6	7	3.6
	Post	2.2	47	6	6	-1	3.4
	Change	.6	8	-1	0	-8	-.2

		WRAT	Arithmetic Inventory	Reading Attitude	Arithmetic Attitude	Teacher Rating/Attitude	Teacher Rating/Achievement
Choice and No Instruction							
Ricky	Pre	1.0	27	8	8	3	3.2
	Post	2.1	35	8	8	2	2.4
	Change	1.1	8	0	0	-1	-.8
Monica	Pre	1.0	20	9	8	4	4.0
	Post	1.4	27	8	7	7	3.0
	Change	.4	7	-1	-1	3	-1.0
No Choice and No Instruction							
Wendy	Pre	1.2	19	9	8	2	3.2
	Post	1.6	31	6	6	1	3.0
	Change	.4	12	-3	-2	-1	-.2
Kim	Pre	.9	30	5	5	3	3.6
	Post	1.8	34	7	6	5	2.8
	Change	.9	4	2	1	2	-.8
Mark	Pre	1.4	26	7	3	2	2.8
	Post	1.6	33	9	4	-1	2.2
	Change	.4	7	2	1	-3	-.6
Edward W.	Pre	1.0	17	9	8	4	3.8
	Post	.9	22	6	9	2	3.6
	Change	-.1	5	-3	1	-2	-.2
Jessica	Pre	1.2	26	6	3	-3	3.4
	Post	1.4	30	6	3	-1	2.8
	Change	.2	4	0	0	2	-.6

		WRAT	Arithmetic Inventory	Reading Attitude	Arithmetic Attitude	Teacher Rating/Attitude	Teacher Rating/Achievement
No Choice and No Instruction							
Dee Dee	Pre	1.8	44	5	6	7	3.6
	Post	2.2	46	8	5	7	3.4
	Change	.4	2	3	-1	0	-.2
Randall	Pre	.9	25	8	8	1	1.4
	Post	2.1	35	8	6	1	3.7
	Change	1.2	10	0	-2	0	2.3
Edward	Pre	.7	8	8	7	-2	2.4
	Post	1.0	17	7	5	1	1.4
	Change	.3	9	-1	-2	3	-1.0
No Treatment							
Gary	Pre	1.2	19	8	8	1	3.0
	Post	1.0	25	8	8	1	2.4
	Change	-.2	6	0	0	0	-.6
Camilla	Pre	1.0	19	9	7	-1	3.6
	Post	1.6	33	9	7	5	3.2
	Change	.6	14	0	0	6	-.4
Shereial	Pre	1.2	26	7	5	3	3.0
	Post	2.2	30	9	9	7	3.8
	Change	1.0	4	2	4	4	.8
La Tonya	Pre	1.4	27	6	5	4	3.8
	Post	1.4	31	8	7	4	2.6
	Change	0	4	2	2	0	-.2
Joyce	Pre	1.4	21	5	2	-1	3.4
	Post	1.0	28	6	4	-5	3.2
	Change	-.4	7	1	2	4	-.2

		WRAT	Arithmetic Inventory	Reading Attitude	Arithmetic Attitude	Teacher Rating/Attitude	Teacher Rating/Achievement
No Treatment							
Michele	Pre	.9	25	3	7	5	2.8
	Post	1.6	34	5	6	1	2.8
	Change	.7	9	2	-1	-4	0
Dee Dee	Pre	.9	30	8	8	1	2.6
	Post	1.0	30	7	7	-1	2.4
	Change	.1	0	-1	-1	-2	-.2
Brad	Pre	1.2	31	7	6	5	3.6
	Post	1.9	42	7	5	7	3.8
	Change	.5	11	0	-1	2	.2

Appendix G
Results from Analyses on Pretest Scores

Multivariate Analyses		Univariate Analyses	
Results from 2 x 2 Analyses (C x I)			
Choice x Instruction Interaction			
WRAT, Inventory	$- p < .36$	WRAT	$- p < .52, \Omega^2 < 0$
		Inventory	$- p < .17, \Omega^2 = 3.13$
Attitudes: Reading, Arithmetic	$- p < .37$	Reading Attitudes	$- p < .44, \Omega^2 < 0$
		Arithmetic Attitudes	$- p < .16, \Omega^2 = 3.06$
Teacher Rating: Attitudes, Achievement	$- p < .96$	Teacher-Rated Attitudes	$- p < .90, \Omega^2 < 0$
		Teacher-Rated Achievement	$- p < .90, \Omega^2 < 0$
Choice Main Effect			
WRAT, Inventory	$- p < .95$	WRAT	$- p < .91, \Omega^2 < 0$
		Inventory	$- p < .89, \Omega^2 < 0$
Attitudes: Reading, Arithmetic	$- p < .89$	Reading Attitudes	$- p < .85, \Omega^2 < 0$
		Arithmetic Attitudes	$- p < .63, \Omega^2 < 0$
Teacher Rating: Attitudes, Achievement	$- p < .86$	Teacher-Rated Attitudes	$- p < .63, \Omega^2 < 0$
		Teacher-Rated Achievement	$- p < .94, \Omega^2 < 0$

Multivariate Analyses		Univariate Analyses	
Instruction Main Effect			
WRAT, Inventory	$p < .99$	WRAT	$p < .91$, $\Omega^2 < 0$
		Inventory	$p < .98$, $\Omega^2 < 0$
Attitudes: Reading, Arithmetic	$p < .14$	Reading Attitudes	$p < .44$, $\Omega^2 < 0$
		Arithmetic Attitudes	$p < .05$, $\Omega^2 = 9.42$
Teacher Rating: Attitudes, Achievement	$p < .56$	Teacher-Rated Attitudes	$p < .28$, $\Omega^2 = .78$
		Teacher-Rated Achievement	$p < .54$, $\Omega^2 < 0$
Results from 1 factor (5 level) Analyses (5 Experimental Conditions)			
WRAT, Inventory	$p < .40$	WRAT	$p < .90$
		Inventory	$p < .55$
Attitudes: Reading, Arithmetic	$p < .87$	Reading Attitudes	$p < .96$
		Arithmetic Attitudes	$p < .48$
Teacher Rating: Attitudes, Achievement	$p < 1.00$	Teacher-Rated Attitudes	$p < 1.00$
		Teacher-Rated Achievement	$p < .95$

Appendix H
Results from Analyses on Change Scores

Multivariate Analyses		Univariate Analyses	
Results from 2 x 2 Analyses (C x I)			
Choice x Instruction Interactions			
WRAT, Inventory	$p < .56$	WRAT	$p < .73$, $\Omega^2 < 0$
		Inventory	$p < .34$, $\Omega^2 < 0$
Attitudes: Reading, Arithmetic	$p < .31$	Reading Attitude	$p < .44$, $\Omega^2 < 0$
		Arithmetic Attitude	$p < .18$, $\Omega^2 = .21$
Teacher Rating: Attitudes, Achievement	$p < .91$	Teacher Rated: Attitudes	$p < .68$, $\Omega^2 < 0$
		Achievement	$p < .89$, $\Omega^2 < 0$
Choice Main Effect			
WRAT, Inventory	$p < .67$	WRAT	$p < .59$, $\Omega^2 < 0$
		Inventory	$p < .54$, $\Omega^2 < 0$
Attitudes: Reading, Arithmetic	$p < .71$	Reading Attitude	$p < .88$, $\Omega^2 < 0$
		Arithmetic Attitude	$p < .42$, $\Omega^2 < 0$
Teaching Rating: Attitudes, Achievement	$p < .82$	Teacher Rating: Attitudes	$p < .59$, $\Omega^2 < 0$
		Achievement	$p < .71$, $\Omega^2 < 0$

Multivariate analyses		Univariate analyses	
Instruction Main Effect			
WRAT, Inventory	$p < .04$	WRAT	$p < .96$, Ω^2 0
		Inventory	$p < .01$, $\Omega^2 = 17.08$
Attitudes: Reading, Arithmetic	$p < .03$	Reading Attitude	$p < .35$, Ω^2 0
		Arithmetic Attitude	$p < .01$, $\Omega^2 = 15.16$
Teacher Rating: Attitude, Achievement	$p < .13$	Teacher-Rated Attitude	$p < .09$, $\Omega^2 = 6.22$
		Teacher-Rated Achievement	$p < .23$, $\Omega^2 = 5.26$
Results from 1 factor, 5 level Analyses (5 Experimental Conditions)			
WRAT, Inve tory	$p < .21$	WRAT	$p < .74$
		Inventory	$p < .06$
Attitudes: Reading, Arithmetic	$p < .23$	Reading Attitude	$p < .66$
		Arithmetic Attitude	$p < .06$
Teacher Rating: Attitude, Achievement	$p < .71$	Teacher-Rated Attitude	$p < .47$
		Teacher-Rated Achievement	$p < .72$

Post Hoc Analyses on Significant Variables from 1-Factor, 5-Level Analyses

Inventory

	NC NI (53)†	NI (55)	C NI (57)	C I (77)	NC I (95)
N NC NI	-	2	4	24	42
NT	-	-	2	22	40
C NI	-	-	-	20	38
C I	-	-	--	-	18

critical value at $p < .05$ for differences between cell totals = 47.2

Arithmetic Attitude

	C NI (07)	NC NI (-4)	NC I (3)	NT (5)	CI (15)
C NI	-	3	10	12	22**
NC NI	-	-	7	9	19
NC I	-	-	-	2	12
NI	-	-	-	-	10

C I - Choice and Instruction Group
C NI - Choice and No-Instruction Group
NC I - No-Choice and Instruction Group
NC NI - No-Choice and No-Instruction Group
NT - No-Treatment Control
† numbers in parentheses represent group score totals
** significant at $p < .01$

Appendix I

Correlations between Pretest and Change Scores

Measure	Correlation Coefficient	t value	Significance
WRAT	-.19	1.19	non-significant
Inventory	-.29	1.90	$p < .05$
Reading Attitudes	-.81	8.48	$p < .01$
Arithmetic Attitudes	-.73	6.63	$p < .01$
Teacher-Rated Attitudes	-.42	2.85	$p < .01$
Teacher-Rated Achievement	-.57	4.24	$p < .01$

Appendix J

Individual Subject Percentage Correct and Percentage Attempted for Each Block of Sessions*

Choice and Instruction	Session Block 1	Session Block 2	Session Block 3	Session Block 4	Session Block 5	Session Block 6
Tonya						
% correct	100.0	79.8	97.9	91.8	58.6	80.0
% attempted	100.0	100.0	94.7	95.7	95.1	98.8
Stephanie						
% correct	90.9	95.65	97.5	82.2	77.2	83.0
% attempted	99.0	68.66	87.1	64.0	82.1	67.9
Tim						
% correct	95.1	93.0	90.8	92.1	90.6	87.8
% attempted	96.3	75.0	78.4	69.7	65.4	35.0
Tracy						
% correct	92.2	100.0	100.0	100.0	97.6	84.8
% attempted	100.0	100.0	100.0	100.0	100.0	88.4
Angela						
% correct	76.6	91.8	81.2	67.5	97 1	84.7
% attempted	98.2	93.8	100.0	69.7	59.6	50.4
Darin						
% correct	99.0	93.5	96.7	79.3	97.5	76.9
% attempted	100.0	100.0	100.0	87.9	88.0	72.2

* Arcsine transformations of these percentages were used for statistical analyses.
Percentage correct = Total number correct x 100/Total number attempted in each block of sessions.
Percentage attempted = Total number attempted x 100/Total number of worksheet problems in each block of sessions.

	Session Block 1	Session Block 2	Session Block 3	Session Block 4	Session Block 5	Session Block 6
Choice and Instruction						
Jill						
% correct	98.1	99.0	100.0	100.0	100.0	95.8
% attempted	94.5	100.0	100.0	100.0	97.5	95.2
Sharon						
% correct	91.7	97.6	100.0	81.9	96.8	89.4
% attempted	100.0	100.0	100.0	100.0	93.9	97.7
No-Choice and Instruction						
Christy						
% correct	95.2	97.9	100.0	97.9	98.8	95.3
% attempted	94.4	98.0	95.8	94.9	79.2	81.5
Scotty						
% correct	71.7	92.2	88.6	96.1	89.9	70.5
% attempted	97.1	80.4	90.8	95.0	95.8	81.3
Reginald						
% correct	98.9	96.7	95.5	77.8	91.8	87.8
% attempted	97.9	96.8	88.6	70.6	53.5	53.8
Johnny						
% correct	94.2	100.0	93.8	89.9	73.8	80.9
% attempted	91.2	99.0	88.9	96.7	74.8	82.7
Brian						
% correct	97.3	100.0	100.0	99.1	90.5	97.5
% attempted	100.0	100.0	97.9	100.0	98.8	90.8
Susan						
% correct	95.2	97.1	95.7	94.1	75.8	87.6
% attempted	97.6	100.0	92.7	91.1	81.5	100.0

	Session Block 1	Session Block 2	Session Block 3	Session Block 4	Session Block 5	Session Block 6
No-Choice and Instruction						
Karen						
% correct	98.9	94.8	97.4	91.8	97.8	89.7
% attempted	100.0	100.0	72.6	51.8	46.4	39.4
Dorothy						
% correct	93.7	95.7	94.9	95.1	94.7	95.5
% attempted	88.8	79.5	61.7	57.8	94.2	54.3
Choice and No-Instruction						
Ronald						
% correct	30.5	22.0	44.6	40.5	29.5	48.4
% attempted	85.4	97.6	90.2	77.1	72.6	96.8
Ricky						
% correct	49.4	50.8	59.1	18.1	69.0	27.1
% attempted	95.3	100.0	95.8	98.8	91.3	96.6
Sally						
% correct	98.2	100.0	99.0	100.0	96.9	94.3
% attempted	99.1	100.0	100.0	100.0	100.0	93.8
Monica						
% correct	33.3	57.9	66.7	65.6	45.0	39.5
% attempted	100.0	96.6	94.0	91.4	95.6	97.0
Fondra						
% correct	10.0	50.0	42.4	44.2	46.7	49.1
% attempted	64.5	100.0	86.8	92.9	97.8	100.0

	Session Block 1	Session Block 2	Session Block 3	Session Block 4	Session Block 5	Session Block 6
Choice and No-Instruction						
Paula						
% correct	92.7	84.0	94.9	100.0	100.0	100.0
% attempted	92.7	84.0	94.9	100.0	100.0	100.0
Timmy						
% correct	82.8	88.0	60.9	72.6	78.2	39.0
% attempted	100.0	84.7	97.8	100.0	100.0	100.0
Bobby						
% correct	40.4	60.1	29.9	66.4	53.5	50.7
% attempted	95.9	98.5	84.5	87.9	97.4	67.6
No-Choice and No-Instruction						
Wendy						
% correct	45.2	65.2	78.6	66.7	78.6	75.5
% attempted	100.0	100.0	94.2	100.0	100.0	100.0
Edward						
% correct	19.5	20.5	37.5	28.0	22.1	36.1
% attempted	67.5	90.1	100.0	97.3	93.2	92.4
Kim						
% correct	93.3	93.3	96.5	70.8	100.0	98.8
% attempted	81.7	89.9	87.6	98.6	96.4	98.8
Mark						
% correct	71.9	80.9	66.4	70.6	88.0	68.9
% attempted	98.0	90.6	94.4	95.1	100.0	75.3
Jessica						
% correct	13.2	44.4	56.0	35.9	3910	39.1
% attempted	100.0	100.0	100.0	100.0	96.6	93.0

	Session Block 1	Session Block 2	Session Block 3	Session Block 4	Session Block 5	Session Block 6
No-Choice and No-Instruction						
Randall						
% correct	13.0	46.7	46.6	45.2	20.6	72.0
% attempted	92.0	73.6	61.3	95.4	85.8	93.9
Dee Dee						
% correct	38.7	78.4	61.3	83.2	86.4	29.3
% attempted	100.0	100.0	95.3	100.0	100.0	89.1
Ed						
% correct	60.8	46.9	66.3	64.5	62.4	79.0
% attempted	98.8	90.6	97.7	90.5	98.9	96.9

Appendix K
Means for Combined Groups for Repeated Measures Analyses*

	% Correct	% Attempted	Number per Cell
Total	2.2	2.6	192
Instruction	2.6	2.6	96
No-instruction	1.7	2.8	
Choice	2.1	2.7	96
No-choice	2.2	2.6	
B_1**	2.1	2.8	32
B_2	2.3	2.8	
B_3	2.3	2.7	
B_4	2.2	2.6	
B_5	2.2	2.6	
B_6	2.1	2.5	
Instruction and Choice	2.6	2.6	48
Instruction and No-Choice	2.6	2.5	
No-instruction and Choice	1.7	2.8	
No-instruction and No-choice	1.8	2.8	
Choice and B_1	2.1	2.8	16
B_2	2.3	2.8	
B_3	2.3	2.8	
B_4	2.1	2.7	
B_5	2.2	2.6	
B_6	1.9	2.5	

* Arcsine transformation of percentages were used.
** Blocks of 6 (occasionally 5) consecutive sessions.

	% Correct	% Attempted	Number per Cell
No-choice and B_1	2.1	2.8	
B_2	2.3	2.8	
B_3	2.3	2.6	
B_4	2.2	2.6	
B_5	2.2	2.6	
B_6	2.2	2.4	
Instruction and B_1	2.7	2.9	
B_2	2.8	2.8	
B_3	2.8	2.7	
B_4	2.6	2.5	
B_5	2.5	2.4	
B_6	2.4	2.2	
No-instruction and B_1	1.5	2.7	
B_2	1.8	2.8	
B_3	1.8	2.7	
B_4	1.8	2.8	
B_5	1.8	2.8	
B_6	1.7	2.7	

Appendix L

Results from Analyses of Within-Session Measures*

Multivariate Analysis on Percentage Correct and Percentage Attempted

Source of ** variance	Degrees of Freedom	F	Probability
I	1	27.60	$p < .0001$
C	1	.60	$p < .5627$
I x C	1	.32	$p < .7317$
S W/I x C	28	-	-
B	5	4.50	$p < .0001$
C x B	5	.59	$p < .8184$
I x B	5	4.68	$p < .0001$
I x C x B	5	1.03	$p < .4167$
S x B W/I x C	140	-	-

* Arcsine transformations of percentages were used in analyses.

** I - Instruction
C - Choice
B - Blocks of sessions over time
S - Subjects

Univariate analysis on percentage correct

Source of Variance**	Degrees of Freedom	Sums of Squares	F	Probability
I	1	38.88	40.32	$p < .0001$
C	1	.17	.18	$p < .6781$
I x C	1	.06	.06	$p < .8126$
S W/I x C	28	27.00	-	-
B	5	1.75	4.36	$p < .0013$
C x B	5	.33	.81	$p < .5452$
I x B	5	1.45	3.63	$p < .0044$
I x C x B	5	.46	1.16	$p < .3340$
S x B W/I x C	140	11.23	-	-

Univariate analysis on percentage attempted

Source of Variance**	Degrees of Freedom	Sums of Squares	F	Probability
I	1	1.80	3.48	$p < .07$
C	1	.37	.71	$p < .41$
I x C	1	.35	.67	$p < .42$
S W/I x C	28	14.49	-	-
B	5	5.04	5.04	$p < .0005$
C x B	5	.19	.35	$p < .8788$
I x B	5	3.65	6.87	$p < .0001$
I x C x B	5	.51	.96	$p < .5563$
S x B W/I x C	140	14.89	-	-

** I - Instruction
C - Choice
B - Blocks of sessions overtime
S - Subjects

Tukey (a) Post Hoc Analysis for Blocks of Sessions over Time or Percentage Correct

	B_6(65.88)[f]	B_1(66.45)	B_5(69.34)	B_4(69.37)	B_2(73.14)	B_3(74.02)
B_6	-	.57	3.46	3.49	7.26*	8.14**
B_1	-	-	2.89	2.92	6.69	7.57*
B_5	-	-	-	.03	3.80	4.68
B_4	-	-	-	-	3.77	4.65
B_2	-	-	-	-	-	.88

* Significant at $p < .05$ (critical value for between cell totals = 6.56)

** Significant at $p < .01$ (critical value for between cell totals = 7.80)

Tukey (a) Post Hoc Analysis for Blocks of Sessions over Time or Percentage Attempted

	B_6(78.42)[f]	B_5(82.89)	B_4(84.68)	B_3(85.08)	B_2(89.01)	B_1(89.68)
B_6	-	4.47	6.26	6.66	10.59†	11.26†
B_5	-	-	1.79	2.19	6.12	6.79
B_4	-	-	-	.40	4.33	5.00
B_3	-	-	-	-	3.93	4.60
B_2	-	-	-	-	-	.67

† Significant at $p < .01$ (critical value for between cell totals = 8.98)

f - numbers in parenthesis represent cell totals