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電腦程式對認知學習的影響—後設分析

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摘 要

雖然，關於學習電腦程式有助於認知能力之培養的主張早有人提出，但是，已知的研究報告卻顯示出相互矛盾的結果。為了對此一具爭議性的問題，提出較為可信的結論，本文綜合了已存在的，有關於電腦程式對認知學習的效果之研究，而成此一「後設分析」（meta-analysis）。筆者收集了來自三個論文索引的六十五篇研究報告，並將其數量資料（quantitative data）轉換於一等值的計量表（common scale）—有效水準（effect size）上。分析結果顯示了有58篇或是89%的加權分析有效水準（study-weighted effect size）為正數，且電腦程式組的分數高於控制組（指未使用電腦程式的學生）。而包含432個比較的整體加權分析有效水準之平均值（overall grand mean of the study-weighted effect sizes）為0.41。這說明了那些有電腦程式經驗的學生，他們在各種認知能力的表現，高於那些未曾有過電腦程式經驗的學生大約16個百分比分數（percentile points）。此外，在28個為此一研究所選擇的變數中，有14個（如：「發表方式」，「年級」，「研究之語言」，及「研究持續之時間」）顯示了對加權分析有效水準平均值在統計學上顯著影響。本研究結論建議：學習一種程式語言的效果，超越了該語言的內容。本研究結果也同時對老師們提供了一種能在課堂中培養學生認知能力的、溫和而有效的方法。

關鍵詞：1.認知能力（cogritive ability）
2.後設分析（meta-analysis）
3.問題解決（problem solving）
4.電腦程式（computer programming）

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EFFECTS OF COMPUTER PROGRAMMING ON COGNITIVE OUTCOMES: A QUANTITATIVE SYNTHESIS

電腦程式對認知學習的影響 -- 後設分析

BY

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ABSTRACT

Although claims regarding the cognitive benefits of computer programming have been made, results from existing empirical studies are conflicting. To make a more reliable conclusion on this issue, a meta-analysis was performed to synthesize existing research concerning the effects of computer programming on cognitive outcomes. Sixty-five studies were located from three sources, and their quantitative data were transformed into a common scale -- Effect Size. The analysis showed that 58 or 89% of the study-weighted effect sizes were positive and favored the computer programming group over the control groups. The overall grand mean of the study-weighted effect size for all 432 comparisons was 0.41; this suggests that students having computer programming experiences scored about 16 percentile points higher on various cognitive-ability tests than students who did not have programming experiences. In addition, fourteen of the twenty-eight coded variables selected for this study (e.g., type of publication, grade level, language studied, and duration of treatment) had a statistically significant impact on the mean study-weighted effect sizes. The findings suggest that the outcomes of learning a computer language go beyond the content of that specific computer language. The results also suggest to teachers a mildly effective approach for teaching cognitive skills in a classroom setting.

Effects of Computer Programming on Students' Cognitive Performance: A Quantitative Synthesis

Yuen-Kuang Cliff Liao

The teaching and learning of programming has become an important topic, especially among computer educators. Widespread availability of microcomputers has led to their increasing use in the nation's schools, with many schools devoting at least some computer time to instruction on programming. According to Pea and Kurland (1984a), several million precollege students in the U. S. receive instruction on computer programming each year.

One reason frequently cited by researchers and decision-makers in education for placing such a heavy emphasis on programming is its presumed impact on problem solving abilities beyond programming activities (e.g., Coburn, Kelman, Roberts, Snyder, Watt, & Weiner, 1982).

> [Programming] is a creative endeavor requiring planning, precision in the use of language, the generation and testing of hypotheses, the ability to identify action sequences that will realize specified objectives, careful attention to detail, and a variety of other skills that seem to reflect what thinking is all about. (Nickerson, 1982, p. 42)

Observations of expert adult programmers do indicate that programmers explicitly employ important problem solving strategies, such as decomposing problems into modules, using analogical reasoning, and systematically planing, coding, and debugging their programs (Nickerson, 1982). These complex cognitive skills may be transferable to other situations beyond programming activities, but there is no automatic guarantee of this.

Several strong claims have been made about the positive benefits on cognitive skills of teaching computer programming to children. Papert (1980) believes that programming allows children to create their own learning environment. His most important claim is that learning Logo enhances problem solving skills by providing concrete experiences that promote thinking at a formal operational level. Formal operational thinking is described by Piaget (1952) as the ability to construct relationships, make inferences, and hypothesize. Other claims about Logo are that it can teach mathematics concepts, it alleviates a fear of mathematics, it provides an environment in which children feel in control and can succeed, it positively affects attitudes toward learning, and in general it teaches children how to learn (Papert, 1980; Watt, 1982). Lochhead and Clement (1979) specifically proposed that computer programming holds the promise of being an effective device for cognitive process instruction -- teaching how, rather than what, to think.

In spite of claims concerning the cognitive benefits of computer programming, research results are conflicting. Clements (1986, 1987), Clements and Gullo (1984), Ehrlick, Abbott, Salter, and Soloway (1984), Rieber (1986), and Statz (1974) all report significant gains in various aspects of cognitive abilities following programming experiences. Most of these studies investigated the effects of Logo on cognitive skills for students at the elementary school level. On the other side, Cheshire (1981), Dalton (1986), Horner and Maddux (1985), Kurland, Pea, Clement, and Mawby (1986), Pea and Kurland (1984b), and Rucinski (1986) have indicated that there is little, if any, transfer of learning from the programming situation to similar non-programming tasks. These studies varied in instructional levels (kindergarten through college) and instructional languages (Logo, BASIC, and some mixtures of programming languages).

Several syntheses of research on computer programming and problem solving ability have been done (e.g., Blume, 1984; Clements, 1985; Johanson, 1988). However, none of them used the meta-analytic approach. Owing to the contradictory evidence provided by existing research in the area, the results from this meta-analysis will help provide clearer conclusions.

Procedure

The research method used in this study is the meta-analytic approach which was similar to that described by Glass, McGaw, & Smith (1981). Their approach requires a reviewer to (a) locate studies through objective and replicable searches; (b) code the studies for salient features; (c) describe outcomes on a common scale; and (d) use statistical methods to relate study features to outcomes (Kulik, Kulik, & Bangert-Drowns, 1985).

The purpose of this study was to synthesize and analyze the research on effects of two instructional approaches. It is important to define these approaches to provide for proper selection of appropriate studies.

<u>computer programming</u> -- using the concepts of program, sequence of instructions, variables, recursion, etc., to write solutions to problems dealing with string processing operations.

<u>computer programming instruction</u> -- classes using computer languages, such as Logo, BASIC, Pascal as an instructional tool to teach students.

non-computer-programming instruction -- classes not using any computer rogramming as an instructional tool to teach students.

<u>Data Sources</u>

The studies considered for use in this meta-analysis came from three major sources and were published from 1969 to 1989. One large group of studies came from computer searches of Education Resources Information Center (ERIC). A second

group of studies came from Comprehensive Dissertation Abstracts. A third group of studies was retrieved by branching from bibliographies in the documents located through review and computer searches.
Sixty-five studies were located through these search procedures; 9 studies came from ERIC, 29 studies were retrieved from published journals, and 27 studies were from Comprehensive Dissertation Abstracts.

Several criteria were established for inclusion of studies in the present analysis.

1. Studies had to assess the relationship between computer programming and cognitive skills such as planning skills, thinking skills, reasoning skills, and metacognitive skills.
2. Studies had to take place in actual classrooms. These was no restriction on grade level.
3. Studies had to provide quantitative results from both computer-programming and non-computer-programming classes.
4. Studies had to be retrievable from university or college libraries by interlibrary loan or from ERIC, Dissertation Abstracts International, or University Microfiche International.
5. The studies were published prior to 1990.

There were also several criteria for eliminating studies or reports cited by other reviews: (a) studies did not report sufficient quantitative data in order to estimate Effect Sizes; (b) studies reported only correlation coefficients -- r value or Chi-square value; (c) studies could not be obtained through interlibrary loans or from standard clearinghouses.

Outcome Measures

The instructional outcome measured most often in the 65 studies was student learning, as indicated on standard cognitive-skill tests at the end of the program of instruction. For statistical analysis, outcomes from a variety of different studies with a variety of different instruments had to be expressed on a common scale. The transformation used for this purpose was the one recommended by Glass et al. (1981); each outcome is coded as an Effect Size (ES), defined as the difference between the mean scores of two groups divided by the standard deviation of the control group. For those studies that did not report means and standard deviations, F values, t values, or proportion values were used to estimate the ES.

In most cases, the application of the formula given by Glass and his colleagues was quite straightforward. But in some cases, when more than one value was available for use in the formula of ES, the value which measured outcomes most correctly was selected. For example, some studies reported both differences on posttest measures and differences in pre-post gains, and some studies reported both raw-score differences between groups and covariance-adjusted differences between groups. In such cases, pre-post gains and covariance-adjusted differences were selected for estimating ES.

In addition, when studies used more than one type of control group (e.g., programming vs. CAI and programming vs. non-programming), only the comparison of programming and non-programming was retained. Also, when studies contained reports of experiments where treatments were compared with each other and no non-computer control group was involved (e.g., programming vs. CAI), Glass et al. (1981. p.124) proposed a process to split the ES into two components to estimate comparisons of each treatment to a hypothetical control group (e.g., programming vs. non-programming and CAI vs. non-CAI). In other cases, several subscales and subgroups were used in measuring a single outcome (e.g., those that reported separate data by language or grade). In such cases, each comparison was weighted in inverse proportion to the number of comparisons within the study (i.e., 1/n, where n = number of comparisons in the study) so that the overweighting of ES of a study could be avoided (see, for example, Waxman, Wang, Anderson, & Walberg, 1985, p. 230).

Variables Studied

Twenty-eight variables were coded for each study in the present synthesis. They were subject area, type of publication, year of publication, ability level, ethnicity, gender, grade level, history, instrumentation, instructor bias, maturation, mortality, multiple treatment, pretest equivalency, operational irrelevancy, reliability of measure, selection bias, selection interaction, statistical power, statistical regression, statistics, testing, type of design, type of computer, duration of treatment, instructional approach, language studied, and cognitive outcome (e.g., cognitive development, conceptual transfer, conditional logical reasoning skill, critical thinking skill, metacognitive skill, problem solving skill). Each of these variables was placed in one of the following set of characteristics: (a) study characteristics, (b) subject characteristics, (c) methodological characteristics, (d) program characteristics, and (e) outcome characteristics. Three variables in the study characteristics were coded because it is important to know how effects are related to sources of information over time. Next four variables in the subject characteristics were coded so that potential different effects for subjects with different background could be detected. Sixteen variables placed in the methodological characteristics were coded so that effects related to characteristics of research procedures could be detected. Six variables in the program characteristics were coded because it is critical to know how effects are related to nature and design of the primary research. The last variable placed in the outcome characteristics were coded so that potential different effects for different type of cognitive outcomes could be detected. Each variable was employed as a factor in an analysis of variance (ANOVA) to investigate whether there were significant differences within each variable on the effect size. The assignments for these independent variables in each characteristics are presented in Table 1.

Insert Table 1 About Here

Coder Reliability

To obtain reliable outcomes from coding, one investigator coded each of the studies on each of the variables. As a check for accuracy, a second investigator coded 5% of the studies independently. The intercoder agreement for these studies exceeded the 85% criterion.

Results

The number of comparisons and the study-weighted effect sizes are reported in Table 2. Of the 65 studies included in the present synthesis, 58 or 89% of the study-weighted effect sizes were positive and favored the programming group, while 7 or 11% of them were negative and favored the non-programming group. The range of the study-weighted effect sizes was from -0.38 to 2.08. The overall grand mean for all 65 study-weighted sizes was 0.41. When this mean ES was converted to percentiles, the percentiles on students' cognitive performance were 66 for the programming group and 50 for the non-programming group. The standard deviation of 0.42 reflects the moderate variability of effect sizes across studies.

The effect sizes for the 432 comparisons are displayed in a scatter diagram in Figure 1. The diagram shows that despite several large effects, most of the effect sizes were small to moderate in magnitude. About 95% of ESs lie between -0.5 and 0.5, while less than 2% of the effect sizes were greater than 1.0.

Insert Figure 1 About Here

Table 3 lists the $\underline{F}$ values for the twenty-eight variables for all 432 ESs in the 65 studies. Descriptive statistics for the significant $\underline{F}$ values are presented in Table 4. Fourteen variables showed statistically significant impact. For each of these variables, a post hoc (Fisher's Protected LSD) test was performed.

Insert Table 3 & 4 About Here

For type of publication, ($\underline{F}(2,429) = 4.599$, $p<.05$) the post hoc test showed that the mean for studies coded as ERIC document was significantly greater than the mean for studies coded as published paper or as dissertation. There was no significant difference between studies coded as published paper and dissertation.

For grade level, ($F(5,426) = 3.854$, $p<.01$) the post hoc test showed that the mean of studies in which the subjects used were high school students was significantly lower than all other groups. There were no significant differences among the other five groups.

The post hoc test for history, ($F(2, 429) = 3.360$, $p<.05$) showed that studies in which the internal validity of history was adequately minimized was significantly greater than the studies in which the internal validity of history was identified as a possible threat. No significant difference on the mean comparison was found between studies coded as adequately minimized and unspecified.

For instrumentation, ($F(3, 428) = 3.998$, $p<.01$) the post hoc test showed the mean comparison for the studies in which the instruments used to measure students' cognitive performance were unknown was significantly higher than the studies using standardized instruments, local instruments, and mixed (standardized and local) instruments. In addition, the post hoc test also showed that the mean comparison for studies using standardized instruments was significantly higher than studies in which local instruments were used. No significant differences were found on the mean comparison between studies coded as local and mixed, and between studies identified as standardized and mixed as well.

The post hoc test for instructor bias, ($F(2,429) = 9.000$, $p<.001$) also showed that the mean comparison for the studies in which the instructor employed in both comparison groups were unspecified was significantly higher than the studies in which the same or different instructors were selected for both comparison groups. There was no significant difference on the mean comparison between studies for which the treatment and control groups had either the same instructors or different instructors.

For mortality, ($F(2,429) = 6.412$, $p<.01$) the post hoc test showed that the mean comparison for the studies in which the internal validity of mortality was identified as a possible threat was significant lower than the studies in which the internal validity of mortality was identified as adequately minimized or unspecified. No significant difference on the mean comparison was found between the studies of the latter groups.

The post hoc test for multiple treatment, ($F(2,429) = 6.795$, $p<.01$) showed that the mean comparison for the studies in which the internal validity of multiple treatment was identified as unspecified was significantly lower than the studies in which the internal validity of multiple treatment was identified as possible threat or adequately minimized. There were no significantly different effects on the mean comparison between studies identified as possible threat or adequately minimized.

The post hoc test for pretest equivalent ($F(5,426) = 3.550$, $p<.01$) showed that the mean comparison for studies in which the internal validity of pretest equivalent was coded as unspecified was significantly higher than the studies used statistical control, and statistical control and random assignment. In addition, the mean comparison for the studies in which random assignment were employed was significantly greater than the studies used statistical control. No significant difference on mean comparison were found among the studies used statistical control, statistical control and random assignment, gain score, and gain score and random assignment.

For reliability of measure, ($\underline{F}(2,429) = 4.786$, $p<.01$) the post hoc test showed that the mean comparison for studies in which the actual figure was reported for the reliability of measure was significantly greater than the studies in which the reliability of measure was just adequately indicated, or unspecified. There weas no significant difference on mean comparison between studies of the latter two groups.

The post hoc test for selection bias, ($F(2,429) = 3.099$, $p<.05$) showed that the mean comparison for studies identified as unspecified on the internal validity of selection bias was significantly higher than the studies identified as possible threat. No significant difference on the mean comparison was found between the studies coded as adequately minimized and possible threat. There was also no significant difference on the mean comparison between studies identified as unspecified and adequately minimized.

For selection interaction, ($\underline{F}(2,429) = 6.774$, $p<.01$) the post hoc test showed that the mean comparison for studies in which the internal validity of selection interaction was identified as a possible threat was significantly lower than the studies identified as adequately minimized or unspecified on the internal validity of selection interaction. No significant difference on the mean comparison was found between the studies of the latter two groups.

The post hoc test for testing, ($\underline{F}(2,429) = 5.989$, $p<.01$) showed that the studies in which the internal validity of testing was identified as adequately minimized was significantly greater than the studies in which the internal validity of testing was identified as a possible threat or unspecified. No significant difference on the mean comparison was found between the studies of the latter two groups.

For duration of treatment, ($\underline{F}(4,427) = 4.621$, $p<.01$) the post hoc test showed that the mean for the studies in which the treatment period was less than 3 months was significantly greater than the studies in which the treatment period were 4-6 months, 7-12 months, or more than 12 months. There was no significant differences among studies with treatment durations of 4-6 months, 7-12 months, more than 12 months, and unspecified. Also, no significant difference was found for the studies with 0-3 months and unspecified duration of treatment.

For language studied, ($F(4,427) = 3.791$, $p<.01$) the post hoc test showed that the mean for studies in which Logo was used as the instructional programming language was significantly higher than the studies which used BASIC or mixed programming languages to instruct. No significant differences were found among studies which used Logo, Pascal, and unknown languages in instruction. Also there was no significant differences among studies using BASIC, Pascal, mixed, and unknown languages to instruct.

Discussion

The results of this meta-analysis indicate that computer programming has slightly positive effects on student cognitive outcomes; 89% of positive study-weighted ES values and 72% of positive ESs overall confirm the effectiveness of computer-programming instruction. The moderateness of the effect must be keep in mind, however; the overall mean ES of 0.41 indicates only the 66th percentile.

The analysis of coded variables suggests some interesting trends in the accumulated research base. First, the greater ES associated with ERIC documents is somewhat atypical of meta-analysis; usually the larger ES is associated with published articles (Glass, et al., 1981, p. 227). There may be some important programming studies that have not yet reached the stage of widespread dissemination through traditional publication outlets, perhaps because of the recentness of studies of programming. Because of the relative small number of studies coded as ERIC, however, this result may be somewhat unstable.

Second, the smallness of the ES associated with high school subjects raises some questions about possible differences between instruction for these students and instruction for other students. As noted below, if the instruction for high school students is over an extended period or involves more than one language, then one would expect the mean ES to diminish. High school teachers may need to rethink their instructional decisions related to programming courses. In particular, semester-long courses on BASIC may not serve students as well as shorter experiences with Logo or Pascal.

Third, the greater ES associated with studies of short duration may reflect a novelty effect or a decrease in positive attitude as students gradually become more familiar with computer technology. In order to maintain continued development of cognitive outcomes, it may be important to attend to instructional approaches which help retain students' positive attitudes toward the learning of cognitive skills. Just adding computer technology to the existing educational setting may only create short-term impact for the development of cognitive abilities. How to properly integrate the innovation into the existing curriculum so that the effects on cognitive skills can be retained is a critical mission for educators.

Fourth, the fact that Logo studies showed a greater ES than BASIC suggests that selection of an appropriate computer language has crucial impact on the cognitive effects of computer programming instruction. Of the three predominantly taught programming languages in educational settings -- Logo, BASIC, and Pascal -- Logo appears to have greater support, especially when compared to BASIC (e.g., Clements, 1985; Ginther & Williamson, 1985; Papert, 1980; Taylor, 1985). However, previous research comparing the effects of teaching Logo and BASIC to students on their cognitive performance reported no significant difference between groups (Dvarskas, 1983; Milojkovic,1983; Reed, Palumbo, & Stolar, 1987/88; Shaw, 1986). This synthesis compared 39 studies, with 294 effect sizes, using Logo and

29 studies, with 101 effect sizes, using BASIC. (Some studies reported both Logo and BASIC comparisons) With such compatible numbers in the groups, the result ought to be considered stable. One possible explanation for the significant difference between Logo and BASIC is that as a programming language, Logo is more structured than BASIC.

Computer educators have established the following criteria for selecting programming languages to affect cognitive abilities: The language should (a) allow a top-down approach for solving problems, and its command structure, (b) promote modularity, and (c) have limited logical constructs (Lockard, 1985/86; Reed, Palumbo, & Stolar, 1987/88). Although BASIC can be taught in a structured way, it is certainly easier to use Logo and Pascal. The aim of structured programming is to subdivide a program into natural parts so that each part can be treated separately. Subprograms can then be written for each part. Working with small parts simplifies the programming procedures (Bass, 1985). Also, according to Papert (1980, p. 103), structured programming allows bugs to be confined and more easily trapped and eliminated. The results of the present study appear to be evidence to support their view.

Fifth, the significant impacts for 10 coded variables among the methodolgical characteristics indicate that the size of the effects may be to a large extent attributable to the research design and/or methodological strategies employed by the primary researcher(s). The significant effects for six variables (i.e., history, instrumentation, mortality, reliability measure, selection interaction, and testing) seem to suggest a pattern that the better the primary sesearch design (e. g., adequately minimize internal validity threats), the greater the positive impacts on the overall ES. however, given the limitations of uneven cell sizes, the pattern sould not be overgeneralized.

Conclusions

The results from this study suggest that the outcomes of learning a computer language extend beyond the content of that specific computer language. Students are able to acquire some cognitive skills such as reasoning skills, logical thinking and planning skills, and general problem solving skills through computer programming activities. Since fostering students' cognitive abilities is usually part of our educational goal, the results provide to teachers a mildly effective approach for teaching students cognitive skills in the classroom setting. Left unanswered is the question of whether computer programming is as efficient, or any more efficient, at developing these cognitive abilities than other possible instructional approaches that teachers might use. Studies of this question will require further clarification of the exact nature of the cognitive abilities most likely to be developed through programming. This meta-analysis points out only that improvements in cognitive outcomes are possible. That information by itself is useful.

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Table 1. The Assignments of Independent Variables in Each Characteristics

Characteristics	Variables
Study Characteristics	Subject Area Type of Publication Year of Publication
Subject Characteristics	Ability Level Ethnicity Gender Grade Level
Methodological Characteristics	History Instrumentation Instructor Bias Maturation Mortality Multiple Treatment Pretest Equivalency Operational Irrelevancy Reliability of Measure Selection Bias Selection Interaction Statistical Power Statistical Regression Statistics Testing Type of Design
Program Characteristics	Type of Computer Duration of Treatment Instructional Approach Language Studied
Outcome Characteristics	Cognitive Outcome

Table 2. Number of Comparisons and Study-weighted Effect Sizes

Author(s)	No of Comparisons	Study-Weighted Effect Sizes
Baker, 1987	1	0.716
Battista et al., 1986	12	0.153
Bebell, 1988	8	0.304
Blubaugh, 1984	4	0.442
Blume et al., 1988	2	-0.043
Cheshire, 1981	2	-0.377
Clements, 1987	1	0.845
Clements et al., 1984	13	0.613
Clements et al., 1986	20	0.228
Cole, 1971	2	0.276
Dalton, 1986	2	0.840
Degelman et al., 1986	1	0.520
Dvarskas, 1983	8	0.468
Ehrlich et al., 1984	2	0.020
Fickel, 1986	2	0.477
Ford, 1984	6	-0.005
Foster, 1972	2	0.575
Gallini, 1985	2	0.979
Hart, 1983	1	1.068
Hatfield, 1969	1	0.152
Hlawati, 1985	8	0.513
Horan, 1985	2	-0.040
Horner et al., 1985	2	0.031
Janson et al., 1987[a]	1	0.076
Janson et al., 1987[a]	1	0.827
Janson et al., 1987[a]	1	0.252
Jones, 1988	2	0.595
Kurland et al., 1986	26	0.051
Lehrer, 1989	11	1.295
Lehrer et al., 1986	8	0.303
Lehrer et al., 1988	12	0.495
Lehrer et al., 1987	6	-0.279
Leitman, 1986	2	0.173
Macgregor, 1989	2	0.428
Mann, 1986	1	0.606

Table 2. Cont'd

Author(s)	No of Comparisons	Study-Weighted Effect Sizes
Mansilla, 1987	2	0.251
Many et al., 1988	2	0.387
Mayer et al., 1986	7	0.043
Mayer et al., 1987	2	0.656
McCoy et al., 1987	3	0.512
McGrath, 1988	24	0.152
Miller et al., 1988	3	0.754
Milner, 1973	1	0.886
Milojkovic, 1983	32	-0.064
Mitterer et al., 1986	18	0.022
Mohamed, 1985	2	0.387
Odom, 1984	1	0.383
Ortiz, 1987	4	0.818
Pea et al., 1984(b)[b]	8	0.486
Pea et al., 1984(b)[b]	104	0.247
Reed et al., 1987(a)	1	0.201
Reed et al., 1987(b)	2	0.317
Reding, 1981	4	-0.160
Rieber, 1986	1	1.575
Ronan, 1971	2	0.276
Rose, 1985	2	0.434
Rucinski, 1986	2	0.283
Seidman, 1981	11	0.114
Shaw, 1986	2	0.023
Silk, 1988	2	0.284
Statz, 1974	8	0.469
Taitt, 1985	2	2.076
Thompson et al., 1988	1	1.004
Turner et al., 1988	1	0.207
Vanlengen, 1988	1	0.054
Overall grand mean		0.410
Overall grand SD		0.422

Note. Total N of Studies = 65. Total N of Comparisons = 432

[a]Three studies reported in one article.

[b]Two studies reported in one article.

Table 3. Results of the ANOVA for Coded Variables

Variables	df	F-Value	P-Value
Study Characteristics			
Subject	3, 428	0.896	.4454
Type of Publication	2, 429	4.599	.0117*
Year of Publication	7, 424	1.964	.0648
Subject Characteristics			
Ability Level	5, 426	0.284	.9211
Ethnicity	4, 427	1.639	.1682
Gender	2, 429	1.102	.3351
Grade Level	5, 426	3.854	.0027**
Methodological Characteristics			
History	2, 429	3.360	.0377*
Instrumentation	3, 428	3.998	.0092**
Instructor Bias	2, 429	9.000	.0002***
Maturation	2, 429	0.140	.8699
Mortality	2, 429	6.412	.0022**
Multiple Treatment	2, 429	6.795	.0016**
Pretest Equivalent	5, 426	3.550	.0048**
Operational Irrelevance	2, 429	0.402	.6695
Reliability of Measure	2, 429	4.786	.0098**
Selection Bias	2, 429	3.099	.0484*
Selection Interaction	2, 429	6.774	.0016**
Statistical Power	1, 430	2.623	.1077
Statistical Regression	2, 429	1.370	.2578
Statistics	3, 428	2.350	.0754
Testing	2, 429	5.989	.0032**
Type of Design	5, 426	1.639	.1541
Program Characteristics			
Type of Computer	2, 429	1.589	.2079
Duration of Treatment	4, 427	4.621	.0016**
Instructional Approach	2, 429	0.638	.5301
Language Studied	4, 427	3.791	.0031**
Outcome Characteristics			
Cognitive Outcome	6, 425	1.517	.1774

* $p < .05$. ** $p < .01$. *** $p < .001$.

Table 4. Means and Standard Deviations for Significant F- values

Variables	Mean of ES	SD	N of ES	N of Study
Type of Publication				
Published	0.192	0.303	184	29
Dissertation	0.228	0.367	115	27
ERIC	0.530	0.501	133	9
Grade Level				
Mixed primary (K-3)	0.400	0.283	69	8
Mixed middle (4-6)	0.283	0.422	136	24
Mixed upper (7-8)	0.306	0.291	15	9
Mixed high school	0.052	0.124	67	9
College	0.354	0.616	20	9
Mixed other	0.181	0.163	125	6
History				
Possible threat	0.101	0.243	37	5
Adequately minimized	0.382	0.533	57	11
Unspecified	0.237	0.328	338	49
Instrumentation				
Local	0.133	0.260	198	15
Standardized	0.275	0.386	226	45
Mixed	0.172	0.186	6	3
Unspecified	0.862	0.292	2	2
Instructor Bias				
Same	0.225	0.299	73	11
Different	0.162	0.345	178	36
Unspecified	0.491	0.340	181	18
Mortality				
Possible threat	0.063	0.142	71	10
Adequately minimized	0.289	0.384	340	49
Unspecified	0.367	0.468	21	6

Table 4. Cont'd.

Variables	Mean of ES	SD	N of ES	N of Study
Multiple Treatment				
Possible threat	0.343	0.315	134	17
Adequately minimized	0.307	0.414	197	30
Unspecified	0.094	0.227	101	18
Pretest Equivalent				
Statistical Control	0.127	0.297	207	22
Random Assignment	0.361	0.272	72	11
Statistical Control & Random Assignment	0.219	0.478	104	13
Gain Score	0.288	0.162	24	7
Gain Score & Random Assignment	0.276	0.000	1	1
Unspecified	0.523	0.303	24	11
Reliability Measure				
Adequately Minimized	0.121	0.269	42	8
Actual figure	0.338	0.384	139	34
Unspecified	0.164	0.326	251	23
Selection Bias				
Possible Threat	0.132	0.208	66	16
Adequately Minimized	0.253	0.374	306	34
Unspecified	0.343	0.456	60	15
Selection Interaction				
Possible threat	0.068	0.141	49	8
Adequately minimized	0.369	0.475	102	15
Unspecified	0.258	0.345	281	42
Testing				
Possible threat	0.130	0.228	81	13
Adequately minimized	0.365	0.404	239	30
Unspecified	0.186	0.360	112	22

Table 4. Cont'd.

Variables	Mean of ES	SD	N of ES	N of Study
Duration of Treatment				
0-3 months	0.351	0.424	149	34
4-6 months	0.126	0.215	82	15
7-12 months	0.123	0.205	164	10
More than 12 months	0.063	0.225	27	3
Unspecified	0.465	0.536	10	3
Language Studied				
Logo	0.356	0.362	294	39[a]
BASIC	0.139	0.353	101	29
Pascal	0.241	0.307	7	4
Mixed	0.003	0.015	26	2
Unspecified	0.203	0.122	4	2

Note. [a]The total number of studies is 76, because some studies reported comparisons for more than one language.

Figure 1. Scatter Diagram of Effect Sizes