



Review

The effectiveness of educational technology applications for enhancing mathematics achievement in K-12 classrooms: A meta-analysis



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ABSTRACT

The present review examines research on the effects of educational technology applications on mathematics achievement in K-12 classrooms. Unlike previous reviews, this review applies consistent inclusion standards to focus on studies that met high methodological standards. In addition, methodological and substantive features of the studies are investigated to examine the relationship between educational technology applications and study features. A total of 74 qualified studies were included in our final analysis with a total sample size of 56,886 K-12 students: 45 elementary studies ($N = 31,555$) and 29 secondary studies ($N = 25,331$). Consistent with the more recent reviews, the findings suggest that educational technology applications generally produced a positive, though modest, effect ($ES = +0.15$) in comparison to traditional methods. However, the effects may vary by educational technology type. Among the three types of educational technology applications, supplemental CAI had the largest effect with an effect size of $+0.18$. The other two interventions, computer-management learning and comprehensive programs, had a much smaller effect size, $+0.08$ and $+0.07$, respectively. Differential impacts by various study and methodological features are also discussed.

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According to a report by the U.S. Department of Education (SETDA, 2010), American teenagers are still trailing behind their counterparts in other industrialized countries in mathematics. In the most recent PISA assessments, U.S. 15-year-olds had an average mathematics score below the average of countries in the Organization for Economic Cooperation and Development (OECD). Among the 33 other OECD countries, over half had higher average scores than the U.S., five had lower average scores, and 11 had average scores that were not substantially different from those of the U.S. Similar patterns were found in tests given in 2003 and 2006.

Importantly, the problem of students' performance in mathematics is not equally distributed. While many middle class schools in the U.S. do perform at world class standards, poor and minority students are much less likely to do so. On the 2011 National Assessment of Educational Progress (NAEP, 2011), only 17% of eighth graders eligible for free lunch scored at proficient or better, while 45% of middle class students scored this well. Among African American students, only 12% scored proficient or better, and the percentages were 17% for Hispanics and 18% for American Indians, compared to 44% for Whites and 54% for Asian-Americans. All of these scores have been improving over time, but the gaps remain.

In response to these and other indicators, policy makers, parents, and educators have been calling for reform and looking for effective approaches to boost student mathematics performance. One of the long-standing approaches to improving the mathematics performance in both elementary and secondary schools is the use of educational technology. The National Council of Teachers of Mathematics, for example, stated in its NCTM Principles and Standards for School Mathematics, "Technology is essential in teaching and learning mathematics; it influences the mathematics that is taught and enhances students' learning" (National Council of Teachers of Mathematics, 2011).

The use of educational technology in K-12 classrooms has been gaining tremendous momentum across the country since the 1990s. Many school districts have been investing heavily in various types of technology, such as computers, mobile devices, internet access, and interactive whiteboards. Almost all public schools have access to the internet and computers in their schools. Educational digital games have also been growing significantly in the past few years. To support the use of educational technology, the U.S. Department of Education provides grants to state education agencies. For example, in fiscal year 2009, the Congress allocated \$650 million in educational technology through the Enhancing Education Through Technology (E2T2) program (SETDA, 2010).

Given the importance of educational technology, it is important to know how best to use technology to enhance mathematics learning. The purpose of this review is to examine evidence from rigorous evaluations of alternative technology applications to determine which are best supported by the evidence in grade K-12 mathematics.

1. The "media effects" debate

The "media effects" debate, about whether media, in and of itself, affects learning, has been carried on for over three decades. Clark and Kozma (Clark, 1983; Kozma, 1994) highlighted the complicated relationships between technology, curricular content, and instructional methods (Nathan & Robinson, 2001). Clark (1983) vehemently argued that educational technology would never influence learning under any circumstances and that "media are mere vehicles that deliver instruction but do

not influence student achievement any more than the truck that delivers our groceries causes changes in our nutrition.” In other words, the impact of technology on student learning was due to instructional content and methods, but not technology per se. Kozma (1994) responded to Clark’s argument by saying the analogy of “delivery truck” creates an “unnecessary schism between medium and method.” Kozma believed that technology had an actual impact on student learning and played an important role in student learning.

The Clark-Kozma debate of the 1980s has been overtaken by the extraordinary developments in technology applications in education in recent years. Though it may be theoretically interesting to ask whether the impact of technology itself can be separated from the impact of particular applications, in practice, technology, content, and method are often intertwined and cannot be separated. As is the case for many educational interventions with many components, currently available technology applications can be seen as packages of diverse elements and evaluated as such. If a particular combination of hardware, software, print materials, professional development for teachers, and other elements can be reliably replicated in many classrooms, then it is worth evaluating as a potential means of enhancing student outcomes. Components of effective multi-element treatments can be varied to find out which elements contribute to effectiveness and to advance theory, but it is also of value for practice and policy to know the overall impact for students even if the theoretical mechanisms are not yet fully understood. Technology is here to stay, and pragmatically, the question is how to make the best use of the many technologies now available.

It is important to emphasize that this review does not attempt to determine the unique contribution of technology per se but rather the effectiveness of educational programs or applications that incorporate the use of educational technology. Technological components, as Clark (1983, 1985a, 1985b) argued, are always confounded with curriculum content, instructional strategies, and other elements, but are adopted by schools as a whole, integrated package. Our intention is to synthesize research on the achievement outcomes of all such practical applications of technology in applied settings.

2. Working definitions of educational technology

Since the term “educational technology” has been used loosely in the literature, it is important to provide a working definition of the term. In this review, educational technology refers to a variety of technology-based programs or applications that help deliver learning materials and support learning process in K-12 classrooms to improve academic learning goals (as opposed to learning to use the technology itself). Examples include computer-assisted instruction (CAI), integrated learning systems (ILS), and technology-based curricula. Although hand calculators are an important technology tool in learning mathematics in today’s classrooms, they are not included in this review for two reasons. First, we consider calculators as a tool rather than an application. In addition, several reviews have already been conducted on the effects of student calculator use in grades K-12 (Burrill et al., 2002; Ellington, 2003; Hembree & Dessart, 1986).

In this review, we did not begin with a set of categories of technology applications in mathematics, but we derived from the literature three major types of technology applications: supplemental CAI programs, computer-managed learning systems, and comprehensive models. Supplemental CAI programs, such as *Jostens*, *PLATO*, *Larson Pre-Algebra*, and *SRA Drill and Practice*, provide additional instruction at students’ assessed levels of need to supplement traditional classroom instruction. Computer-managed learning systems included only *Accelerated Math*, which uses computers to assess students’ mathematics levels, assign mathematics materials at appropriate levels, score tests on this material, and chart students’ progress (Niemiec, Samson, Weinstein, & Walberg, 1987). Comprehensive models, such as *Cognitive Tutor* and *I Can Learn*, use computer-assisted instruction along with non-computer activities (such as teacher-led instruction and cooperative learning) as the students’ core approach to mathematics.

3. Previous reviews of educational technology on mathematics achievement

Research on educational technology has been abundant. In the past three decades, over twenty major reviews have been conducted in this area (e.g. Bangert-Drowns, Kulik, & Kulik, 1985; Christmann & Badgett, 2003; Hartley, 1977; Kulik, 2003; Kulik & Kulik, 1991; Ouyang, 1993; Rakes, Valentine, McGatha, & Ronau, 2010; Slavin & Lake, 2008; Slavin, Lake, & Groff, 2009). The majority of these examined a wide range of subjects (e.g., reading, mathematics, social studies, science) and grades from K to 12. Seven out of the 21 reviews focused only on mathematics achievement (Burns, 1981; Hartley, 1977; Lee, 1990; Li & Ma, 2011; Rakes et al., 2010; Slavin & Lake, 2008; Slavin et al., 2009). The majority of the reviews concluded that there were positive effects of educational technology applications on mathematics achievement, with an overall study-weighted effect size of +0.31 (Table 1). However, effect sizes ranged widely, from +0.10 to +0.62. Table 2 presents a summary of the findings for mathematics outcomes for these 21 major reviews.

Though several narrative and box-score reviews had been conducted in the 1970s (Edwards, Norton, Taylor, Weiss, & Duseldoph, 1975; Jamison, Suppes, & Wells, 1974; Vinsonhaler & Bass, 1972), their findings were criticized by other researchers because of their vote-counting methods (Hedges & Olkins, 1980). The reviews carried out by Hartley (1977) and Burns (1981) were perhaps the earliest reviews on computer technology that used a more sophisticated meta-analytic method. The focus of Hartley’s review was on the effects of individually-paced instruction in mathematics using four techniques: computer-assisted instruction (CAI), cross-age and peer tutoring, individual learning packets, and programmed instruction. Twenty-two studies involving grades 1–8 were included in his review. The average effect size for these grades was +0.42.

Table 1

Summary of major meta-analyses on effects of educational technology on mathematics achievement.

Authors	Years covered	Types of publication	Subjects covered	Grades	Number of studies (Math)	Effect size ^a
Hartley (1977)	1960–1975	Dissertation	Math	Elementary (grade 1–8)	22	+0.42
Burns (1981)	1960–1975	Dissertation	Math	Elementary and Secondary	32	+0.37
Bangert-Drowns et al. (1985)	1968–1982	Journal	Math and a variety of subjects	Secondary	22	+0.26
Bangert-Drowns et al. (1985)	1967–1982	Journal	Math and a variety of subjects	Elementary	17	+0.54
Niemiec, Samson, Weinstein, & Walberg (1987)	1968–1982	Journal	Math and a variety of subjects	Elementary	Unspecified	+0.28
Lee (1990)	1970–1988	Dissertation	Math and a variety of subjects	Elementary & secondary	72	+0.38
Kulik and Kulik (1991)	1966–1986	Journal	Math and a variety of subjects	Elementary to college	9	+0.39
Ryan (1991)	1984–1989	Journal	Math and a variety of subjects	Elementary	8	+0.30
Becker (1992)	1977–1989	Journal	Math and a variety of subjects	Elementary & secondary	11	+0.27
Ouyang (1993)	1986–1993	Dissertation	Math and a variety of subjects	Elementary	Unspecified	+0.62
Khalili and Shashaani (1994)	1988–1992	Journal	Math and a variety of subjects	Elementary to college	18	+0.52
Fletcher-Flinn and Gravatt (1995)	1987–1992	Journal	Math and a variety of subjects	Elementary to college	24	+0.32
Christmann, Badgett, and Lucking (1997)	1984–1994	Journal	Math and a variety of subjects	Secondary	13	+0.18
Liao (1998)	1986–1997	Journal	Math and a variety of subjects	Elementary to college	5	+0.13
Christmann and Badgett (2003)	1966–2001	Journal	Math and a variety of subjects	Elementary	12	+0.34
Kulik (2003)	1990–1996	Report	Math and a variety of subjects	Elementary	16	+0.38
Liao (2007)	1983–2003	Journal	Math and a variety of subjects	Elementary to college	12	+0.29
Slavin and Lake (2008)	1971–2006	Journal	Math	Elementary	38	+0.19
Slavin, Lake, and Groff (2009)	1971–2007	Journal	Math	Elementary	38	+0.10
Li and Ma (2011)	1990–2006	Journal	Math	Elementary to college	46	+0.28
Rakes et al. (2010)	1968–2008	Journal	Math	Elementary to college	36	+0.16

^a Effect sizes were extracted from elementary and secondary math studies only.

Like Hartley (1977), Burns' (1981) review was also on the impact of computer-based drill and practice and tutorial programs on students' mathematics achievement. Burns (1981) included a total of 32 studies in her review and came up with a similar effect size of +0.37. Other important reviews were conducted in the 1980s by Bangert-Drowns et al. (1985) and Kulik et al. (1985). Compared to the earlier reviews by Hartley (1977) and Burns (1981), both Kulik and Bangert-Drowns adopted much stricter inclusion criteria to select their studies. First, the studies had to take place in actual classroom settings. Second, the studies had to have a control group that was taught in a conventionally instructed class. Third, the studies had to be free from methodological flaws such as high attrition rates or unfair teaching of the criterion test to one of the comparison groups. Kulik et al. (1985) and Bangert-Drowns et al. (1985) included a total of 22 and 18 studies for the elementary and secondary mathematics reviews, respectively. They found a positive effect of computer-based teaching, with an effect size of +0.26 for elementary and +0.54 for secondary grades.

Two recent reviews by Slavin and his colleagues (Slavin & Lake, 2008; Slavin et al., 2009) applied even more stringent inclusion criteria than Kulik's to select only studies with high methodological quality. In addition to the key inclusion criteria set by Kulik and his colleagues, Slavin and his colleagues added the following criteria: a minimum of 12-week duration, evidence of initial equivalence between the treatment and control group, and a minimum of two teachers in each group to avoid possible confounding of treatment effect with teacher effect (see Slavin and Lake (2008) for a rationale). Slavin and Lake (2008), Slavin et al. (2009) included a total of 38 educational technology studies in their elementary review and 38 in a secondary review and found a modest effect size of +0.19 for elementary schools and a small effect size of +0.10 for secondary schools.

The two most recent reviews were conducted by Rakes et al. (2010) and Li and Ma (2011). In their meta-analysis, Rakes and his colleagues examined the effectiveness of five categories of instructional improvement strategies in algebra:

technology curricula, non-technology curricula, instructional strategies, manipulative tools, and technology tools. Out of the 82 included studies, 15 were on technology-based curricula such as *Cognitive Tutor*, and 21 were instructional technology tools such as graphing calculators. Overall, the technology strategies yielded a statistically significant but small effect size of +0.16. The effect sizes for technology-based curriculum and technology tools were +0.15 and +0.17, respectively. Similar to Rakes et al. (2010), Li and Ma (2011) examined the impact of computer technology on mathematics achievement. A total of 41 primary studies were included in their review. The review reports an average effect size of +0.28.

4. Problems with previous reviews

Though reviews over the past 30 years have produced suggestive evidence of the effectiveness of educational technology applications on mathematics achievement, the results must be interpreted with caution. As is evidenced by the substantial variations in average effect sizes across reviews, it makes a great deal of difference which procedures are used for study inclusion and analysis. Many evaluations of technology applications suffer from serious methodological problems. Common problems include a lack of a control group, limited evidence of initial equivalence between the treatment and control group, large pretest differences, or questionable outcome measures. In addition, many of these reviews included studies that had a very short duration. Unfortunately, studies with poor methodologies tend to report much higher effect sizes than those with more rigorous methods (see Slavin & Smith, 2009; Slavin & Madden, 2011), so failing to screen out such studies inflates the average effect sizes of meta-analyses. In the following section, we discuss some of these problems and the issues associated with them.

4.1. No control group

As mentioned earlier, many previous reviews included studies that did not have a traditionally taught control group. Earlier reviews such as those by Hartley (1977) and Burns (1981) are prime examples, where a high percentage of their included studies did not have a traditional control group. Though reviews after the 1980s employed better inclusion criteria, some still included pre-post designs or correlational studies in their selection. For example, in his dissertation, Ouyang (1993) examined a total of 79 individual studies in an analysis on the effectiveness of CAI on mathematics achievement. He extracted a total of 267 effect sizes and came up with an overall effect size of +0.62 for mathematics. Upon closer examination, however, 60 of these effect sizes (22%) came from pre-post studies. Lacking a control group, of course, a pre-post design attributes any growth in achievement to the program, rather than to normal, expected gain. Liao (1998) is another case in point. In his review, he included a total of 35 studies to examine the effects of hypermedia on achievement. Five of these studies were one-group repeated measures without a traditional control group. What he found was that the average effect size of these five repeated measures studies ($ES = +1.83$) was much larger than that of studies with a control group ($ES = +0.18$).

4.2. Brief duration

Including studies with brief durations could also potentially bias the overall results of meta-analyses, because short-duration studies tend to produce larger effects than long-duration studies. This may be true due to novelty factors, a better controlled environment, and the likely use of tests biased toward content studied by experimental but not control students. In particular, experimenters often create highly artificial conditions in brief studies that could not be maintained for a whole school year, and which contribute to unrealistic gains. Brief studies may advantage experimental groups that focus on a particular set of objectives during a limited time period, while control groups spread that topic over a longer period. In their review, Bangert-Drowns et al. (1985) included a total of 22 studies that looked at the impact of computer-based education on mathematics achievement in secondary schools. One third of these studies (32%) had a study duration ranging from 2 to 10 weeks. In a review in secondary schools (Kulik et al., 1985), a similar percentage (33%) of short-duration studies was also included. In both reviews, effect sizes were much higher for the brief experiments. In evaluating the effectiveness of micro-computer applications in elementary schools, Ryan (1991) examined 40 studies across several subject areas, including mathematics, with an overall effect size of +0.31. However, 29 out of the 40 included studies (73%) had a duration of less than 12 weeks. In their 1991 updated review, Kulik and Kulik (1991) included 53 new studies, covering students from elementary school to college. However, out of the 53 added studies, over half had a duration of less than 12 weeks. Eleven of them were only one-week experiments, which again had much more positive effect sizes than the longer studies.

4.3. No initial equivalence

Establishing initial equivalence is of great importance in evaluating program effectiveness. Some reviews included studies that used a post-test only design. Such designs make it impossible to know whether the experimental and control groups were comparable at the start of the experiment. Since mathematics posttests are so highly correlated with pretests, even modest (but unreported) pretest differences can result in important bias in the posttest. Meyer and Feinberg (1992) had this to say with regards to the importance of establishing initial equivalence in educational research, "It is like watching a baseball game beginning in the fifth inning. If you are not told the score from the previous innings nothing you see can tell you who is winning the game." Several studies included in the Li and Ma (2011) review did not establish initial equivalence (e.g.,

Funkhouser, 2003; Wodarz, 1994; Zumwalt, 2001). In his review, Becker (1992) found that among the seven known studies of the WICAT integrated learning system, only one provided some evidence on the comparability of comparison populations and provided data showing changes in achievement for the same students in both experimental and control groups. Studies with large pretest differences also pose threats to validity, even if statistical controls are used. For example, Ysseldyke et al (2003a,b) conducted two separate studies on the impact of educational technology programs on mathematics achievement. Both of the studies had large pretest differences ($ES > 0.50$). Large pretest differences cannot be adequately controlled for, as underlying distributions may be fundamentally different even with the use of ANCOVAs or other control procedures (Shadish, Cook, & Campbell, 2002), and in fact, the Ysseldyke studies reported larger effect sizes than did other studies of the same treatments.

4.4. Measures inherent to the experimental treatment

A frequent practice, especially in brief studies of mathematics interventions, is the use of outcome measures closely aligned with the content taught to the experimental group but not the control group. In an investigation of this problem, Slavin and Madden (2011) identified seven mathematics studies accepted by the What Works Clearinghouse that reported outcomes on both treatment-inherent and treatment-independent measures. The effect sizes were +0.45 and -0.03 , respectively. On 10 reading studies, the effect sizes were +0.51 and +0.06, respectively. A recent example of the use of treatment-inherent measures was a study by Roschelle et al. (2010) evaluating a program called SimCalc. In three large studies, two of them using random assignment, seventh and eighth graders were taught a 2½ week unit on proportionality. They were pre- and post-tested on two subtests. One, called M1, was designed to represent the content taught to all students in Texas. The other, M2, was specifically aligned to the content taught only in SimCalc. Not surprisingly, the effect sizes were far higher for the comparisons on M2 (effect sizes = +0.89,+0.69,+0.81, all $p < 0.0001$) than on M1 (effect sizes = +0.10 (ns),+0.13 ($p < 0.05$),+0.19 (ns)). On the whole test, combining M1 and M2, effects were very positive (effect sizes = +0.63,+0.50,+0.56, all $p < 0.0001$).

The Roschelle et al. (2010) study is unusual in facing the issue of inherent measures directly, but it raises a problem for the meta-analyst. Which measure “counts” in the meta-analysis? Only M1 is fair to the control group, as they never saw the content assessed on M2. Yet using only the M1 measure would greatly understate the impact of SimCalc.

In the present review, the Roschelle et al. (2010) study would not have qualified anyway, because the treatment duration of 2½ weeks was far less than the 12-week criterion. However, had this not been the case, only the M1 measure could have been included. Failure to exclude measures like M2 (or like the total score including M2) is a major factor in causing previous reviews to greatly overstate average effect sizes from studies using experimenter-made measures inherent to the experimental treatment.

4.5. Cherry-picking evidence

Cherry-picking is a strategy used by some developers or vendors to pick favorite findings to support their cause. When analyzing the effectiveness of Integrated Learning Systems (ILS), Becker (1992) included 11 Computer Curriculum Corporation (CCC) evaluation studies in his review. Four of the 11 studies were carried out by the vendor. Each of these studies was a one-year-long study involving sample sizes of a few hundred students. Effect sizes provided by the vendor were suspiciously large, ranging from +0.60 to +1.60. Upon closer examination, Becker (1992) found that the evaluators used an unusual procedure to exclude students in the experimental group, those who showed a sharp decline in scores at posttest, claiming that these scores were atypical portraits of their abilities. However, the evaluators did not exclude those who had a large gain, arguing that the large gain might have been caused by the program. In a study conducted in 11 Milwaukee Chapter 1 schools, the evaluators compared the impact of the CCC program on 600 students in grades 2–9 to the test-normed population. The evaluators excluded 8% of the negative outliers in math but did not exclude any positive outliers. The overall effect size reported was +0.80. However, after making reasonable adjustments, Becker estimated the average effect size to be around +0.35, not the reported +0.80. Another example was a WICAT study reported in Chicago (Becker, 1992). Only scores of a select sample of 56 students across grades 1–8 in two schools were reported. It raised the issue of why results for this particular group of students were reported but not results for other students. Becker (1992) suspected that achievement data might have been collected for all students by the schools, but the schools simply did not report disappointing results.

4.6. Discrepant findings from the Dynarski/Campuzano study

Recently, a major, large-scale randomized evaluation of modern technology applications was carried out by Dynarski et al. (2007), and a second study the following year was reported by Campuzano, Dynarski, Agodini, and Rall (2009). These studies, involving 51 schools in 16 districts, evaluated applications of CAI in sixth grade math and Algebra I, including such widely-used programs as *Cognitive Tutor*, *PLATO*, and *Larson Pre-Algebra*. The study found effect sizes near zero ($ES = +0.03$) at both grade levels (and also for first and fourth grade reading programs).

The Dynarski/Campuzano findings are disturbing in themselves, but they also call out for a re-examination of the evidence on technology applications in general. How is it possible that reviewers of prior research conclude that there are positive effects of computer applications in mathematics ranging from +0.10 to +0.62, when the largest randomized evaluation

of today's most widely used CAI approaches finds effect sizes near zero? Do the highest-quality studies of the past contradict those of Dynarski and Campuzano, or have rigorous evaluations of technology applications in math found effects resembling those of Dynarski/Campuzano? A major rationale for doing yet another review is to examine the literature on evaluations of technology applications in mathematics in light of this new evidence.

5. Rationale for present review

The present review hopes to overcome the major problems seen in previous meta-analyses by applying rigorous, consistent inclusion criteria to identify high-quality studies. In addition, we will examine how substantive and methodological features affect the overall outcome of educational technology on mathematics achievement, and to determine whether the findings of the Dynarski et al. (2007) and Campuzano et al. (2009) large-scale randomized evaluation are consistent or inconsistent with those of other high-quality studies. The present study seeks to answer three key research questions:

1. Do education technology applications improve mathematics achievement in K-12 classrooms as compared to traditional teaching methods without educational technology?
2. What study and research features moderate the effects of educational technology applications on student mathematics achievement?
3. Do the Dynarski/Campuzano findings conform with those of other high-quality evaluations?

6. Methods

The current review employed meta-analytic techniques proposed by Glass, McGaw, and Smith (1981) and Lipsey and Wilson (2001). Comprehensive Meta-analysis Software Version 2 (Borenstein, Hedges, Higgins, & Rothstein, 2009) was used to calculate effect sizes and to carry out various meta-analytical tests, such as Q statistics and sensitivity analyses. The meta-analytic procedures followed several key steps: (1) Locate all possible studies; (2) screen potential studies for inclusion using preset criteria; (3) code all qualified studies based on their methodological and substantive features; (4) calculate effect sizes for all qualified studies for further combined analyses; and (5) carry out comprehensive statistical analyses covering both average effects and the relationships between effects and study features.

6.1. Locating all possible studies and literature search procedures

All the qualifying studies from the present review come from four major sources. Previous reviews provided the first source, and references from the studies cited in the reviews were further investigated. A second group of studies was generated from a comprehensive literature search of articles written between 1960 and 2011. Electronic searches were made of educational databases (e.g., JSTOR, ERIC, EBSCO, Psych INFO, Dissertation Abstracts), web-based repositories (e.g., Google Scholar), and educational technology publishers' websites, using different combinations of key words (e.g., educational technology, instructional technology, computer-assisted instruction, interactive whiteboards, multimedia, mathematics interventions, etc.). In addition, we also conducted searches by program name. We attempted to contact producers and developers of educational technology programs to check whether they knew of studies that we had missed. Furthermore, we also conducted searches of recent tables of contents of key journals from 2010 to 2011: *Educational Technology and Society*, *Computers and Education*, *American Educational Research Journal*, *Journal of Educational Research*, *Journal of Research on Mathematics Education*, and *Journal of Educational Psychology*. We sought papers presented at AERA, SREE, and other conferences. Citations in the articles from these and other current sources were located. Over 700 potential studies were generated for preliminary review as a result of the literature search procedures.

6.2. Criteria for inclusion

To be included in this review, the following inclusion criteria were established.

1. The studies evaluated any type of educational technology applications, including computer assisted instruction, integrated learning systems, and other technology-based programs, used to improve mathematics achievement.
2. The studies involved students in grades K-12.
3. The studies compared students taught in classes using a given technology-assisted mathematics program to those in control classes using an alternative program or standard methods.
4. Studies could have taken place in any country, but the report had to be available in English.
5. Random assignment or matching with appropriate adjustments for any pretest differences (e.g., analyses of covariance) had to be used. Studies without control groups, such as pre-post comparisons and comparisons to "expected" scores, were excluded.

6. Pretest data had to be provided, unless studies used random assignment of at least 30 units (individuals, classes, or schools), and there were no indications of initial inequality. Studies with pretest differences of more than 50% of a standard deviation were excluded because, even with analyses of covariance, large pretest differences cannot be adequately controlled for as underlying distributions may be fundamentally different (Shadish et al., 2002).
7. The dependent measures included quantitative measures of mathematics performance, such as standardized mathematics measures. Experimenter-made measures were accepted if they were comprehensive measures of mathematics, which would be fair to the control groups, but measures of mathematics objectives inherent to the program (but unlikely to be emphasized in control groups) were excluded.
8. A minimum study duration of 12 weeks was required. This requirement is intended to focus the review on practical programs intended for use for the whole year, rather than brief investigations. Studies with brief treatment durations that measured outcomes over periods of more than 12 weeks were included, however, on the basis that if a brief treatment has lasting effects, it should be of interest to educators.
9. Studies had to have at least two teachers in each treatment group to avoid the confounding of treatment effects with teacher effects.
10. Programs had to be replicable in realistic school settings. Studies providing experimental classes with extraordinary amounts of assistance that could not be provided in ordinary applications were excluded.
11. Studies had to have taken place from 1980 to the present.

6.3. Study coding

To examine the relationship between effects and the studies' methodological and substantive features, studies needed to be coded. Methodological features included research design and sample size. Substantive features included grade levels, types of educational technology programs, program intensity, level of implementation, and socio-economic status. The study features were categorized in the following way:

1. Types of publication: Published or unpublished.
2. Year of publication: 1980s and before, 1990s, or 2000s and later.
3. Research design: Randomized, randomized quasi-experiment, matched control, or matched post hoc.
4. Sample size: Small ($N \leq 250$ students) or large ($N > 250$).
5. Grade level: Elementary (Grade 1–6), or secondary (Grade 7–12).
6. Program types: Computer-managed learning (CML), integrated, or supplemental.
7. Program intensity: Low (≤ 30 min per week), medium (between 30 and 75 min per week), or high (> 75 min per week).
8. Implementation: Low, medium, or high (as rated by study authors).
9. Socio-economic status: Low (free and reduced lunch $> 40\%$) or high (F/R lunch $< 40\%$).

Study coding was conducted by two researchers working independently. The inter-rater agreement was 95%. When disagreements arose, both researchers reexamined the studies in question together and came to a final agreement.

6.4. Effect size calculations and statistical analyses

In general, effect sizes were computed as the difference between experimental and control individual student posttests after adjustment for pretests and other covariates, divided by the unadjusted posttest pooled standard deviation. Procedures described by Lipsey and Wilson (2001) and Sedlmeier and Gigerenzer (1989) were used to estimate effect sizes when unadjusted standard deviations were not available, as when the only standard deviation presented was already adjusted for covariates or when only gain score standard deviations were available. If pretest and posttest means and standard deviations were presented but adjusted means were not, effect sizes for pretests were subtracted from effect sizes for posttests. Studies often reported more than one outcome measure. Since these outcome measures were not independent, we produced an overall average effect size for each study. After calculating individual effect sizes for all 75 qualifying studies, Comprehensive Meta-Analysis software was used to carry out all statistical analyses, such as Q statistics and overall effect sizes.

6.5. Limitations

Before presenting our findings and conclusion, it is important to mention several limitations in this review. First, due to the scope of this review, only studies with quantitative measures of mathematics were included. There is much to be learned from other non-experimental studies, such as qualitative and correlational research, that can add depth and insight to understanding the effects of these educational technology programs. Second, the review focuses on replicable programs used in realistic school settings over periods of at least 12 weeks, but it does not attend to shorter, more theoretically-driven studies that may also provide useful information, especially to researchers. Finally, the review focuses on traditional measures of math performance, primarily standardized tests. These are useful in assessing the practical outcomes of various programs

Table 2
Overall effect sizes.

	<i>k</i>	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Fixed	74	0.10	0.01	0.000	0.09	0.12	12.11	0.00	345.80	73	0.000
2. Random	74	0.16	0.02	0.000	0.11	0.20	7.14	0.00			

Table 3
By programs.

Types of program	Mixed effects analysis										
	<i>k</i>	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Supplemental	55	0.19	0.03	0.001	0.14	0.24	6.85	0.00			
2. CML	10	0.09	0.05	0.002	0.00	0.18	2.00	0.05			
3. Comprehensive	9	0.06	0.05	0.002	-0.04	0.15	1.12	0.26			
Total between (Q_B)									7.25	2	0.03

CML = Computer managed learning.

and are fair to control as well as experimental teachers, who are equally likely to be trying to help their students do well on these assessments. However, the review does not report on experimenter-made measures of content taught in the experimental group but not the control group, although results on such measures may also be of importance to researchers or educators.

7. Findings

7.1. Overall effects

A total of 74 qualifying studies were included in our final analysis with a total sample size of 56,886 K-12 students: 45 elementary studies ($N = 31,555$) and 29 secondary studies ($N = 25,331$). As indicated in Table 2, the overall weighted effect size is +0.16. The large Q value indicated that the distribution of effect sizes in this collection of studies is highly heterogeneous ($Q = 345.80$, $df = 73$, $p < 0.00$). In other words, the variance of study effect sizes is larger than can be explained by simple sampling error. Thus, a random effects model was used¹ (Borenstein et al., 2009; Dersimonian & Laird, 1986; Schmidt et al., 2009). In order to explain this variance, key methodological features (e.g., research design, sample size) and substantive features (e.g., type of intervention, grade level, SES) were used to model some of the variation.

7.2. Substantive features of the studies

Five key substantive features were identified and examined in this review: Types of intervention, year of publication, grade levels, program intensity, level of implementation, and socio-economic status.

7.2.1. Types of intervention

With regards to intervention types, the studies were divided into three major categories: Computer-Managed Learning (CML) ($N = 7$), Comprehensive Models ($N = 8$), and Supplemental CAI Technology ($N = 37$). Over 70% of all studies fell into the supplemental CAI program category.

Table 3 presents the summary results of the analyses by program types. A marginally significant between-group effect ($Q_B = 7.25$, $df = 2$, $p < 0.03$) was found, indicating some variation among the three programs. The 55 supplemental CAI programs produced the largest effect size, +0.19, and the 10 computer-managed learning programs and the nine comprehensive models produced similar small effect sizes of +0.09 and +0.06, respectively. The results of the analyses of CML and the comprehensive models must be interpreted with caution due to the small number of studies in these two categories, however.

¹ Note: A random-effects model was used for three reasons. First, the test of heterogeneity in effect sizes was statistically significant. Second, the studies for this review were drawn from populations that are quite different from each other, e.g., age of the participants, types of intervention, research design, etc. Third, the random-effects model has been widely used in meta-analysis because the model does not discount a small study by giving it a very small weight, as is the case in the fixed-effects model (Borenstein et al., 2009; Dersimonian & Laird, 1986; Schmidt, Oh, & Hayes, 2009). The average effect size using a fixed-effects procedure was only +0.11 (see Table 2).

Table 4
By year of publication.

Research design	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. 1980s	21	0.23	0.05	0.002	0.14	0.32	4.86	0.000			
2. 1990s	18	0.15	0.04	0.000	0.07	0.23	3.69	0.000			
3. 2000s and 2001s	35	0.12	0.03	0.001	0.06	0.18	4.10	0.000			
Total between (Q _B)									3.78	2	0.15

Table 5
By grade levels.

Grade	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Elementary	45	0.17	0.03	0.001	0.11	0.22	6.00	0.00			
2. Secondary	29	0.14	0.04	0.001	0.07	0.21	3.92	0.00			
Total between (Q _B)									0.43	1	0.51

Table 6
By intensity.

Intensity	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Low (<30 min/wk)	10	0.06	0.04	0.004	-0.03	0.15	1.31	0.19			
2. Medium(30–75 min/wk)	32	0.20	0.04	0.001	0.12	0.27	5.21	0.00			
3. High (>75 min a week)	29	0.14	0.03	0.001	0.08	0.20	4.27	0.00			
Total between (Q _B)									5.85	2	0.05

Table 7
By implementation.

Reported quality	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Low	5	0.12	0.04	0.001	0.05	0.19	3.18	0.00			
2. Medium	32	0.12	0.03	0.001	0.06	0.18	3.71	0.00			
3. High	6	0.26	0.05	0.002	0.17	0.35	5.52	0.00			
4. NA	31	0.19	0.04	0.001	0.12	0.25	5.32	0.00			
Total between (Q _B)									7.72	3	0.05

NA: no information about implementation.

7.2.2. Year of publication

One might expect that the overall effectiveness of educational technology applications would be improving over time as technology becomes more advanced and sophisticated. However, the evidence does not support this expectation. Fletcher-Finn and Gravatt (1995) and Liao (1998) did not find a consistent upward pattern. Christmann and Badgett (2003) found a negative trend over a 14 year time span, with effect sizes dropping from +0.73 in 1969 to +0.36 in 1998. Our present review found no trend toward more positive results in recent years (see Table 4). The mean effect sizes for studies in the 80s, 90s, and after 2000 were +0.23, +0.15, and +0.12, respectively.

Table 8
By SES.

SES	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Low SES	41	0.12	0.02	0.001	0.08	0.17	5.35	0.00			
2. High SES	10	0.25	0.11	0.013	0.03	0.47	2.23	0.03			
3. Diverse SES	13	0.19	0.06	0.003	0.08	0.30	3.33	0.01			
4. No information	10	0.15	0.04	0.001	0.07	0.22	3.85	0.00			
Total between (Q_B)									2.20	3	0.53

7.2.3. Grade levels

The results by grade levels are shown in Table 5. The effect size for elementary studies ($ES = +0.17$) was higher than that for secondary studies ($ES = +0.14$), but the difference was not statistically significant ($p < 0.51$). The direction of the difference is consistent with previous reviews (Bangert-Drowns et al., 1985; Kulik et al., 1985), suggesting that educational technology had a more positive effect on elementary students than secondary students.

7.2.4. Program intensity

Program intensity (frequency of intended use) was divided into three major categories: low intensity (the use of technology less than 30 min a week), medium intensity (between 30 and 75 min a week), and high intensity (over 75 min a week). Analyzing the use of technology as a moderator variable, a statistically significant difference was found between the three intensity categories ($Q_B = 5.85$, $df = 2$, $p = 0.05$). The effect sizes for low, medium, and high intensity were $+0.06$, $+0.20$, and $+0.14$, respectively (see Table 6). In general, programs that were used more than 30 min a week had a bigger effect than those that were used less than 30 min a week.

7.2.5. Level of implementation

We also found significant differences among low, medium, and high levels of implementation. It is important to note that almost half of the studies (41%) did not provide sufficient information about implementation, and levels of program implementation were estimated by the authors. The average effect size of studies with a high level of implementation ($ES = +0.26$) was significantly greater than those of low and medium levels of implementation ($ES = +0.12$) (see Table 7). However, the implementation ratings must be considered cautiously because researchers who knew that there were no experimental-control differences may have described poor implementation as the reason, while those with positive effects might be less likely to describe implementation as poor.

7.2.6. Socio-economic status (SES)

Effect sizes were similar in schools serving children of low and high SES. Low SES refers to studies in which 40% or more students received free and reduced-price lunches, and high SES refers to studies in which fewer than 40% of students received free and reduced-price lunches. The 13 studies that involved a diverse population, including both low and high SES students, and the 10 studies that had no SES information, were excluded in this analysis. The p -value (0.53) of the test of heterogeneity in effect sizes suggests that the variance in the sample of effect sizes was within the range that could be expected based on sampling error alone. The effect sizes for low and high SES were $+0.12$ and $+0.25$, respectively (see Table 8).

7.3. Methodological features of the studies

As indicated in Table 2, the large Q -value ($Q = 345.80$, $df = 73$, $p < 0.00$) in the test of heterogeneity in effect sizes suggests that there are some underlying systematic differences in this collection of studies. Three key potential methodological features were examined: Year of publication, research design, and sample size.

Table 9
By design.

Research design	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Randomized	26	0.08	0.03	0.001	0.03	0.14	2.88	0.00			
2. RQE	8	0.24	0.11	0.010	0.04	0.45	2.33	0.02			
3. Matched	20	0.20	0.04	0.001	0.12	0.29	4.96	0.00			
4. MPH	20	0.15	0.04	0.001	0.07	0.22	3.85	0.00			
Total between (Q_B)									7.13	3	0.07

Table 10
By design.

Research design	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Randomized	26	0.08	0.03	0.001	0.04	0.16	3.24	0.001			
2. Quasi-experiments	48	0.20	0.03	0.001	0.14	0.25	6.55	0.000			
Total between (Q _B)									7.20	1	0.01

Table 11
By sample size.

Sample size	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Large (N > 250)	44	0.12	0.02	0.001	0.08	0.17	5.15	0.000			
2. Small (N < 250)	30	0.26	0.05	0.003	0.16	0.36	5.19	0.000			
Total between (Q _B)									6.13	1	0.01

Table 12
By design and size.

Research design/size	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Large randomized	15	0.06	0.03	0.001	0.00	0.13	1.81	0.07			
2. Small randomized	11	0.17	0.05	0.003	0.06	0.28	3.14	0.00			
3. Large matched control	29	0.16	0.03	0.001	0.09	0.22	4.88	0.00			
4. Small matched control	19	0.31	0.07	0.005	0.17	0.45	4.33	0.00			
Total between (Q _B)									11.97	3	0.01

Table 13
Classic fail-safe N.

Z-value for observed studies	13.63
P-value for observed studies	0.00
Alpha	0.05
Tails	2.00
Z for alpha	1.96
Number of observed studies	74
Number of missing studies that would bring p-value to > alpha	3506

7.3.1. Research design

One potential source of variation may lie in the research design of the different studies (e.g., Abrami & Bernard, 2006). There were four main types of research designs in this review: randomized experiments, randomized quasi-experiments, matched control studies, and post hoc studies. Randomized experiments ($N = 26$) were those in which students, classes, or schools were randomly assigned to conditions and the unit of analysis was at the level of the random assignment. Randomized quasi-experiments (RQE) ($N = 8$) also used random assignment at the school or class level but due to a limited sample of schools or classes, the analysis had to be done at the student level. Matched control studies ($N = 20$) were ones in which experimental and control groups were matched on key variables at pretest, before posttests were known. Matched post hoc studies (MPH) ($N = 20$) were ones in which groups were matched retrospectively, after posttests were known. Table 9 summarizes the outcomes by research design. The average effect sizes for randomized experimental studies, randomized quasi-experiments, matched control studies, and matched post hoc studies were +0.08, +0.24, +0.20, and +0.15, respectively. Since there were only eight RQE studies, and the effect sizes of the matched and MPH studies were similar, we decided to combine these three quasi-experimental categories into one category and compare it to the randomized experiments. Results are found in Table 10. The mean effect size for quasi-experimental studies was +0.20, more than twice that for randomized studies (+0.08).

Table 14

Orwin's fail-safe N.

Standardized difference in means in observed studies	0.10
Criterion for a 'trivial' standardized difference means	0.01
Mean standardized difference in means in missing studies	0.00
Number of missing studies needed to bring standardized difference in means under 0.01	701

Table 15

By publication.

Publication	Mixed effects analysis										
	k	ES	SE	Variance	95% confidence interval		Test of mean		Test of heterogeneity in effect sizes		
					Lower	Upper	Z-value	P-value	Q-value	df (Q)	P-value
1. Published	18	0.15	0.04	0.001	0.08	0.22	4.18	0.00			
2. Unpublished	56	0.15	0.03	0.001	0.10	0.21	5.86	0.00			
Total between (Q_B)									0.01	1	0.94

7.3.2. Sample size

Another potential source of variation may be study sample size (Slavin & Smith, 2009). Previous studies suggest that studies with small sample sizes are likely to produce much larger effect sizes than do large studies (Cheung & Slavin, 2011; Liao, 1999). In this collection of studies, there were a total of 45 large studies with sample sizes greater than 250 and 30 small studies with fewer than 250 students. As indicated in Table 11, we found a statistically significant difference between large studies and small studies. The mean effect size for the 30 small studies (ES = +0.26) was about twice that of large studies (ES = +0.12, $p < 0.01$).

7.3.3. Design/Size

Within each research design, the effect sizes of the small studies were about twice as large as those of the large studies. Large matched control studies produced an effect size of ES = +0.16, as compared to +0.31 for small matched control studies. A similar pattern was also found within the randomized group. Large randomized studies had an effect size of +0.06, whereas small randomized studies had an effect size that was twice as large (ES = +0.17) (see Table 12). The findings for the large, randomized studies, as a group, resembled those of the Dynarski/Campuzzano studies, with very small effect sizes.

7.4. Sensitivity analysis

To avoid the impact of potential outliers that might skew the overall results, a sensitivity analysis was conducted to check for extreme positive as well as negative effect sizes for the overall effect size estimate of +0.16. Using a "one-study removal" analysis (Borenstein et al., 2009), the range of effect sizes still falls within the 95% confidence interval (0.11 to 0.20). In other words, the removal of any one effect size does not substantially affect the overall effect sizes.

7.5. Publication bias

To check whether there was a significant number of studies with null or negative results that have not been uncovered in the literature search which might nullify the effects found in the meta-analysis, classic fail-safe N and Orwin's fail-safe N analyses were performed. As suggested in Table 13, the classic fail-safe N test determined that a total of 3,629 studies with null results would be needed in order to nullify the overall effect size estimate of +0.15. The Orwin's test (Table 14) estimates the number of missing null studies that would be required to bring the mean effect size to a trivial level. We set 0.01 as the trivial value. The result indicated that the number of missing null studies to bring the existing overall mean effect size to 0.01 was 702. Both tests suggest that publication bias could not account for the significant positive effects observed across all studies.

We also used a mixed-effects model to test whether there was a significant difference between published journal articles and unpublished publications, such as conference papers, technical reports, and dissertations. As indicated in Table 15, published articles and unpublished reports produced the same effect size of +0.15. Thus, no publication bias was found ($p < 0.94$).

8. Discussion

The findings of this review indicate that educational technology applications produce a positive but modest effect (ES = +0.16) on mathematics achievement. Our findings are consistent with the more recent reviews conducted by Slavin and Lake (2008), Slavin et al. (2009) and Rakes et al. (2010). Our overall effect size falls somewhere between that of the two recent large-scale randomized studies by Campuzzano and Dynarski (ES = +0.03) and that of previous reviews

(ES = +0.31). There are at least two possible factors that may explain the difference between our review and previous reviews. As mentioned earlier, many of the previous reviews included studies of marginal quality, which often inflate effect size estimates. In particular, studies using measures inherent to the experimental treatment, very small studies, and matched (rather than randomized) studies all tend to report larger effect sizes. In this review, we applied strict inclusion criteria to select our studies. As a result, many studies included in other reviews were not included in the present review. We also weighted means by sample sizes. Second, none of the previous reviews included the six effect sizes from the two most recent large-scale third party evaluation reports by Campuzano et al. (2009) and Dynarski et al. (2007), which found minimal effects of educational technology in middle and high schools on math achievement. Since these two reports contained studies that had large sample sizes, including them has a negative effect on the overall effect size. For example, the overall effect size would have changed from +0.15 to +0.18 had we excluded the six effect sizes from these two large-scale evaluation reports. The change was more obvious at the secondary level where the six effect sizes from these two reports changed the overall effect size from +0.13 to +0.19. The effect size of all large randomized studies (ES = +0.08) was similar to those reported in the Dynarski and Campuzano studies.

Among the three types of educational technology applications, supplemental CAI had the largest effect on mathematics achievement, with an effect size of +0.19. The other two interventions, computer-management learning (CML) and comprehensive programs, had a much smaller effect size, +0.09 and +0.06, respectively. The effect size of CML is similar to that reported in reviews by Kulik et al. (1985) and Niemiec et al. (1987), who also found CML to have a minimal effect on student mathematics achievement. In a recent meta-analysis conducted by Cheung and Slavin (2011) that examined the effectiveness of educational technology programs on reading achievement, it was found that integrated approaches such as *Read 180* and *Voyager Passport*, which integrated computer and non-computer instruction in the classroom, produced a larger effect (ES = +0.28) than supplemental programs (ES = +0.11). However, integrated approaches such as *Cognitive Tutor* (ES = -0.04) and *I Can Learn* (ES = +0.16) in mathematics did not produce the same kind of effects as in reading (ES = +0.28). These findings provide some suggestive evidence that a more integrated approach may be more effective in reading than in mathematics.

In addition to these overall findings, this review also looked at the differential impact of educational technology on mathematics by various study and methodological features. It is worth mentioning some of the key findings generated from these variables and how they might impact student math outcomes.

First, 64% of the studies in this review were quasi-experimental, including matched control, randomized quasi-experiments, and matched post hoc experiments, and only one-third (36%) were randomized experiments. Six out of the 26 randomized studies were conducted by Campuzano et al. (2009) and Dynarski et al. (2007). We also found that the effect sizes of the quasi-experimental studies (+0.20) were more than twice the size of the randomized studies (+0.08). Our finding is consistent with findings reported by Cheung and Slavin (2011), who found very similar differences between randomized and non-randomized studies of technology in reading. In their review, Niemiec et al. (1987) found that “methodologically weaker studies produced different results than strong studies. . . [and] the results of quasi-experimental studies have larger variances.” Unequal variances may produce results that could be potentially unreliable and misleading (Hedges, 1984). The present findings point to an urgent need for more practical randomized studies in the area of educational technology applications for mathematics.

Second, our findings indicate that studies with small sample sizes produce, on average, twice the effect sizes of those with large sample sizes. Similar results were also found within each research design. The results support the findings of other research studies that made similar comparisons (Pearson, Ferdig, Blomeyer, & Moran, 2005; Slavin & Madden, 2011; Slavin & Smith, 2009). This should come as no surprise for three reasons. First, small-scale studies are often more tightly controlled than large-scale studies and, therefore, are more likely to produce positive results. In addition, standardized tests are more likely to be used in large scale studies, and these are usually less sensitive to treatments. For example, Li and Ma (2011) found that studies that used non-standardized tests had larger effect sizes than those that used standardized tests. Finally, the file-drawer effect is more likely to apply to small-scale studies with null effects than to large-scale studies.

Third, previous reviews suggested that the use of educational technology had a bigger effect on elementary students than secondary students (Li & Ma, 2011; Niemiec et al., 1987; Slavin & Lake, 2008; Slavin et al., 2009). We found a similar result, but the difference between elementary studies (ES = +0.17) and secondary studies (ES = +0.14) was not statistically different. As Kulik et al. (1985) argued, “High school. . . students apparently have less need for highly structured, highly reactive instruction provided in computer drills and tutorials. They may be able to acquire basic textbook information with the cues and feedback that CAI systems provide.”

Fourth, a statistically significant difference was found among the three categories of program intensity. Applications that required computer use of more than 30 min or more had a larger effect than those that required less than 30 min a week. Some researchers argued that the small effect produced by these supplemental programs could be due to low implementation. For instance, in their study of Integrated Learning Systems (ILS), Van Dusen and Worthen (1995) found that few teachers followed the actual ILS usage guidelines. Thus, students typically only ended up spending between 15% and 30% of the recommended time on the computer. Some used less than 10 min per week. Teachers, who often saw ILS as supplemental technology, rarely integrated ILS instruction into regular classroom instruction. Reviewers and researchers often treat the limited time devoted to technology as an implementation problem, but perhaps it speaks to a fundamental problem that separate CAI programs are not well accepted or seen as central to instruction by teachers, so teachers may not make sure

that students get the full amount of time on technology recommended by vendors. Future studies should investigate more closely the impact of the time and integration factors for various grade levels.

Fifth, in terms of the relationship between study recency and effectiveness, recent reviews are consistent in failing to find improvements over time in effects of technology on learning. It has long been assumed that, with technological advancement, student achievement effects of technology would be improved. On the other hand, Liao (1998) and Christmann and Badgett (2003) found no positive trend in outcomes for recent studies. We found no such positive trend in recent studies in our review, and Cheung and Slavin (2011) also found that effects of technology in reading were not improving over time.

Sixth, in contrast to some earlier reviews (Niemiec et al., 1987; Smith, 1980; Sterling, 1959), we found no statistically significant difference between published articles and unpublished reports. Published articles and unpublished reports, such as dissertations and technical reports, produced the same effect size of +0.15. There were more unpublished reports ($N = 56$) than published articles ($N = 18$) in this review. However, our selection criteria screen out studies of poor quality, so only the higher-quality unpublished studies were included.

Finally, more recent educational technology applications such as *Geometer's Sketchpad* (Choi-Koh, 1999; Hollebrands, 2003; Hollebrands, 2007), *Mathematica* (Cooley, 1997; Roddick, 2001; Thompson, 2009), *SimCalc* (Roschelle et al., 2010), or applets like those available on the National Library of Virtual Manipulatives (Reimer & Moyer, 2005; Steen, Brooks, & Lyon, 2006; Suh, Moyer, & Heo, 2005) have become increasingly popular in mathematics classrooms today. Though several studies have been conducted to examine the effectiveness of these applications, none of them met our inclusion criteria. High quality evaluations are needed for these increasingly popular applications in today's classrooms.

9. Conclusion

Technology has infiltrated every aspect of modern life. Classrooms are no exception. School districts across the country have been investing a substantial amount of their annual budgets on educational technology in an effort to boost academic performance in the past two decades. In addition, compared to the situation a couple of decades ago, schools are in a much better position to implement educational technology in their classrooms. Many teachers now are more experienced and willing to use educational technology in their classroom instruction, and educational technology is more affordable compared to a decade ago. Undoubtedly, educational technology will continue to play an increasingly important role in the years to come. So the question is no longer whether teachers should use educational technology or not, but rather how best to incorporate various educational technology applications into classroom settings. The present review indicates that incorporating supplemental programs into regular classroom curriculum may be beneficial (Eisenberg & Johnson, 1996; Kulik & Kulik, 1991), and adhering to program usage guidelines suggested by technology providers may be helpful in improving student achievement.

Educational technology is making a modest difference in learning of mathematics. It is a help, but not a breakthrough. However, the evidence to date does not support complacency. New and better tools are needed to harness the power of technology to enhance mathematics achievement for all children.

Appendix A

Elementary

Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size
<i>Computer-managed learning systems</i>							
<i>Accelerated math</i>							
Ysseldyke and Bolt (2007)	Randomized quasi-experiment (L)	1 year	5 schools	2–5	Schools in Texas, Alabama, South Carolina, and Florida	Terra Nova	+0.03
Ysseldyke et al. (2003)	Matched (L)	1 year	1310 students (397E, 913C)	3–5	Schools in large urban district in the Midwest	NALT	+0.11
Nunnery and Ross (2007)	Matched (L)	2 years	915 students (416E, 499C)	5	Schools in a suburban school district in Texas	TAAS	+0.20
Spicuzza et al. (2001)	Matched (L)	5 months	495 students (137E, 358C)	4–5	Large urban district in the Midwest	NALT	+0.17

Appendix A (continued)

	Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size
Ross and Nunnery (2005)	Matched Post Hoc (L)	1 year	4191 students (2350E, 1841C)	3–5	Schools in southern Mississippi	MCT	+0.04	
	Johnson-Scott (2006)	Matched Post Hoc (S)	1 year	3 schools 7 classes 82 students	5	Schools in rural Mississippi	MCT	+0.23
<i>Supplemental CAI</i>								
<i>Jostens/compass learning</i>								
Becker (1994)	Randomized (L)	1 year	1 school 400 students (200E, 200C)	2–5	Inner city east-coast school	CAT	+0.04	
	Alifrangis (1991)	Randomized (S)	1 school 250 students (125E, 125C)	4–6	School at an army base near Washington, DC 37% minority	CTBS		-0.08
Hunter (1994)	Matched (S)	1 year	6 schools 120 students (60E, 60C)	2–5	Chapter 1 schools in Jefferson County, Georgia.	ITBS	+0.40	
Estep, McInerney, Vockell, and Kosmoski (2000)	Matched Post Hoc (L)	1–5 years	106 schools 3180 students (1590E, 1590C)	3	Schools across Indiana	ISTEP	+0.02	
Spencer (1999)	Matched Post Hoc (S)	5 years	92 students (52E, 40C)	2–3	Urban school district in southeastern Michigan	CAT	+0.40	
<i>CCC/successmaker</i>								
Ragosta (1983)	Randomized (L)	3 years	4 schools 1440 students (720E, 720C)	1–6	Schools in the Los Angeles Unified School District	CTBS	+0.36	
Hotard & Cortez (1983)	Randomized (S)	6 months	2 schools 190 students (94E, 96C)	3–6	Schools in Lafayette Parish, Louisiana	CTBS	+0.19	
	Manuel (1987)	Randomized (S)	12 weeks 165 students (99E, 79C)	3 schools	3–6	Schools in Omaha, Nebraska	CTBS	+0.07
Gatti and Petrochenkov (2010)	Randomized quasi-experiment (L)	1 year	10 schools 63 classes 812 students (506E, 306C)	3,5	Schools from 8 urban and suburban school districts in 7 states.	GMAD	+0.077	

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Appendix A (continued)

	Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size
Mintz (2000)	Matched Post Hoc (L)	1 year	8 schools	4–5 489 students (201E, 288C)	Schools in Etowah County, Alabama	SAT-9	–0.06	
Laub (1995)	Matched Post Hoc (L)	5 months	2 schools	4–5 314 students (157E, 157C)	Schools in Lancaster County, Pennsylvania	SAT	+0.27	
<i>Other ILS</i> Schmidt (1991), (Wasatch ILS)	Matched (L)	1 year	4 schools	2–6 1,224 students (683E, 541C)	Schools in Southern California	CTBS	0.05	
	Miller (1997) (Waterford integrated learning system)	Matched Post Hoc (L)	1 to 3 years	30 schools (10E, 20C) 3600 students (1200E, 2400C)	3–5 New York City Public Schools	MAT		+0.17
Brehmer-Evans (1994) (ILS)	Matched Post Hoc (S)	1 year	2 schools	2–3 140 students (62E, 78C)	Magnet schools in the school district of the city of River Rouge, Michigan. 68% White & 32% minority.	CAT	–0.01	
<i>The math machine</i> Abram (1984) (The math machine)	Randomized (S)	12 weeks	1 school	1 5 classes 103 students (50E, 53C)	Suburban school district in Southwest	ITBS	–0.18	
Watkins (1986) (The math machine)	Randomized (S)	6 months	1 school	1 82 students (41E, 41C)	Suburban southwestern school	CAT	+0.41	
Whitaker (2005)	<i>Classworks</i> Matched Post Hoc (S)	1 year	2 schools	4–5 220 students (123E, 97C)	Schools in rural Tennessee	TCAP	+0.21	
Birch (2002)	<i>Lightspan</i> Matched (S)	2 years	2 schools	2,3 101 students (51E, 50C)	Schools in the Caesar Rodney School District in Delaware	SAT	+0.28	
<i>Compass Learning Odyssey Math</i> Wijekumar, Hitchcock,	Randomized (L)	1 year	32 schools	4	Schools in Mid-	CTBS	+0.02	

Appendix A (continued)

Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size
Turner, Lei, and Peck (2009)			122 teachers (60E, 62C) 2456 students (1223E, 1233C)		Atlantic region with diverse SES		
<i>EnVision math</i>							
Resendez, Azrin, and Strobel (2009)	Randomized (L)	2 years	6 schools 50 classes 708 students (330E, 378C)	2–5	Schools from 6 states, predominantly White, diverse SES	MAT	+0.35
<i>SRA drill & practice</i>							
Easterling (1982)	Randomized (S)	4 months	3 schools 42 students (21E, 21C)	5	Schools in a large urban school district	CAT	+0.02
Leap Frog (2004)	<i>Leap track</i> Matched (S)	5 months	11 classes 158 students (100E, 58C)	1,3,4	Schools in an urban high poverty district in Oakland, California. 84% FRL, 60% ELL, & 82% Latino.	CTBS	+0.08
<i>Other Supplemental CAI</i>							
Becker (1994) (CNS)	Randomized (L)	1 year	1 school 9 classes 360 students (180E, 180C)	2–5	Inner city east-coast school	CAT	+0.15
Carrier, Post, and Heck (1985) (various CAI)	Randomized (S)	14 weeks	6 classes 144 students (71E, 73C)	4	Metropolitan school district in Minnesota	Experimenter-designed Test, Algorithms, Math facts	+0.21
Fletcher, Hawley, and Piele (1990) (Milliken math sequences)	Randomized (S)	4 months	1 school 4 classes 79 students (39E, 40C)	3,5	School in rural Saskatchewan, Canada	CTBS	+0.40
Van Dusen and Worthen (1995) (unspecified program)	Randomized quasi-experiment (L)	1 year	6 schools 141 classes 4,612 students	K–6	Schools selected from diverse geographic areas across the U.S.	Norm-Referenced Tests	+0.01
Shanoski (1986) (Mathematics Courseware)	Randomized quasi-experiment (L)	20 weeks	32 classes (18E, 14C) 832 students (392E, 435C)	2–6	4 schools in rural Pennsylvania	CAT	–0.02
Turner (1985) (Milliken Math)	Randomized quasi-	15 weeks	1 school 275	3–4	School in suburb of	CTBS	+0.37

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Appendix A (continued)

Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size
Sequencing and Pet Professor)	experiment (L)		students (185E, 90C)		Phoenix, Arizona		
Metrics Associates (1981) (Coursewares by CCC)	Matched (L)	1 year	352 students (151E, 201C)	2–6	Title I schools in six communities in MA.	MAT	+0.10
Rutherford et al. (2010) (Spatial temporal mathematics)	Matched (L)	1 year	34 schools (18E, 16C)	2–5	Low SES schools in Orange County, CA with Hispanic majority.	Schools in CST	+0.37
Morgan, Sangston, and Pokras (1980) (Unspecified CAI)	Matched (L)	1 year	13 schools (9E, 4C) 2080 students (1440E, 640C)	3–6	Schools in Montgomery County, MD.	Experimenter-designed Test	+0.16
Hess and McGarvey (1987) (Memory, number farm)	Matched (S)	5 months	186 students (88E, 98C)	K	Schools drew wide range socio-economic backgrounds	Criterion-Referenced Test	+0.14
Bass, Ries, and Sharpe (1986) (CICERO software)	Matched (S)	1 year	1 school 178 students (91E, 87C)	5–6	Chapter 1 school in rural Virginia	SRA Achievement Series	–0.12
Webster (1990) (Courses by computers math)	Matched (S)	14 weeks	5 schools 120 students (64E, 56C)	5	Schools in rural Mississippi Delta school district	SAT	+0.13
Pike (1991) (Unspecified CAI)	Matched (L)	1 year	6 schools 293 students (161E, 132C)	4–5	Chapter I schools in central Georgia. 90% FRL; 90% AA	ITBS	+0.15
Meyer (1986) (Unspecified CAI)	Matched (S)	18 weeks	1 school 62 students	1–5	School with underachieving students	SAT	+0.48
Levy (1985) (Mathematics strands, problem solving - ISI)	Matched Post Hoc (L)	1 year	4 schools 576 students (291E, 285C)	5	Suburban New York School District	SAT	+0.21
Karvelis (1988) (Unspecified CAI)	Matched Post Hoc (S)	1 year	4 schools 223 students (106E, 117C)	3	Low performing schools in San Francisco, CA.	CTBS	+0.08
Borton (1988) (The San Diego basic skills in mathematics program)	Matched Post Hoc (S)	1 year	1 school 92 students (36E, 56C)	5	Suburban school near San Diego	CTBS	+0.68
Secondary Comprehensive Cognitive tutor							
Campuzzano, Dynarski, Agodini, and Rall (2009)	Randomized (L)	1 year	11 schools 18 classes (9E, 9C)	8–9	Schools across US. 50% FRL, 46% W, 41% AA, 13% H	ETS Algebra I	–0.06

Appendix A (continued)

Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size	
			276 students					
	Pane, McCaffrey, Slaughter, Steele, and Ikemoto (2010)	Randomized (L)	1 year	8 schools 699 students (348E, 351C)	9–12	Schools in Baltimore County, MD. 46% minority, 26% FRL, & high SES.	District math final exam	–0.19
Cabalo and Vu (2007)	Randomized quasi-experiment (L)	1 year	22 classes (11E, 11C) 541 students (281E, 260C)	8–13	Schools in Maui, HI. 55% Asian-American.	NWEA Math Goals Survey 6+		+0.03
Shneyderman (2001)	Matched (L)	1 year	6 schools 777 students (325E, 452C)	9–10	High schools in Miami, FL	ETS Algebra I, FCAT-NRT		+0.12
Smith (2001)	Matched (L)	3 semesters	445 students (229 E, 216 C)	9–12	High schools in a large, urban district in Virginia	Virginia Standards of Learning (SOL) Algebra I test		–0.07
Barrow, Markman, and Rouse (2009)	<i>I Can Learn</i> Randomized (L)	1 year	17 schools 146 classes 1605 students (795E, 810C)	6–12	Schools in 3 urban districts; 83%AA, 13%H	NWEA Algebra/ State Tests		+0.13
	Kirby (2004)	Randomized (S)	1 year	1 school 204 students (91E, 113C)	8	School in Alameda County, CA	California Standards Tests (CST)	+0.04
	Kirby (2006a)	Matched Post Hoc (L)	1 semester	13 schools 57 teachers 1360 students (680E, 680C)	8	New Orleans public schools	LEAP	+0.19
Kirby (2006b)	Matched Post Hoc (L)	1 semester	1144 students (166E, 978C)	10	High-poverty high schools in New Orleans	LEAP		+0.23
	<i>Computer-Managed Learning Systems</i>							
	<i>Accelerated Math</i>							
	Ysseldyke and Bolt (2007)	Randomized quasi-experimental (L)	1 year	3 schools 1000 students	6–8	Middle schools in MS, MI, NC. 37% AA, 34% W, 26% H, Low SES	Terra Nova	+0.07
	Nunnery and Ross (2007)	Matched (L)	2 years	992 students (482E, 510C)	8	Schools in a suburban TX school district.	TAAS	+0.17
Gaeddert (2001)	Matched (S)	1 semester (3 1/2 months)	100 students in 6 classes	9–12	High school in Kansas	SAT 9		+0.35

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Appendix A (continued)

Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size
Atkins (2005)	Matched Post Hoc (L)	3 years	542 students (354E, 188C)	6–8	Rural schools in eastern Tennesse. 53% FRL, 99%W, Low SES	Terra Nova	–0.26
<i>Supplemental CAI Jostens/Compass Learning</i> Hunter (1994)	Matched (L)	28 weeks	6 schools 6–8 (3E, 3C) 90 students (45E, 45C)	6–8	Schools in rural ITBS Jefferson County, Georgia 83% AA, 17% W, Low SES	+0.22	
Howell (1996)	Matched (S)	1 year	10 classes (5E, 5C) 131 students (66E, 65C)	6–8	Schools in Dodge Co., Georgia	ITBS, Computations, Concepts, and Problem Solving	–0.06
<i>Larson Pre-Algebra</i> Campuzzano et al. (2009)	Randomized (L)	1 year	8 schools in 3 districts 2588 students in 2 cohorts	6	Schools across the US. 66% FRL, 42% H, 30%AA, and 28% W	SAT-10	+0.11
<i>Larson Algebra I</i> Campuzzano et al. (2009)	Randomized (L)	1 year	12 schools in 5 districts 1204 students in 2 cohorts	8–9	Schools across the US. 50% FL, 48%W, 41%AA, and 13% H	ETS Algebra	0.00
<i>PLATO Achieve Now</i> Campuzzano et al. (2009)	Randomized (L)	1 year	8 schools 1037 students	6	Schools across the US. 66% FRL, 42% H, 30%AA, and 28% W	SAT-10	–0.03
<i>PLATO Web Learning Network</i> Thayer (1992)	Matched (L)	18 weeks	2 schools 22 classes 467 students (234E, 233C)	9–12	Remedial math students in an inner-city high schools in Miami	SSAT	+0.21
<i>New Century</i> Boster, Yun, Strom, and Boster (2005)	Matched (L)	1 year	306 students (139E, 167C)	7	Low achieving students in suburb of	CST	+0.28

Appendix A (continued)

Study	Design	Duration	N	Grade	Sample characteristics	Effect sizes by posttest and subgroup	Overall effect size
			167C)		Sacramento, California. 39% FRL, 18% ELL		
<i>SRA Drill & Practice</i>							
Dellario (1987)	Matched Post Hoc (S)	1 year	9 schools 202 students (97E, 43 C)	9	Low-performing students in southwestern Michigan	SDMT, (MAT, CAT)	+0.36
<i>Other Supplemental CAI</i>							
Dynarski et al. (2007) (a variety of CAI applications)	Randomized (L)	1 year	23 schools 69 teachers (39E, 32C) 1404 students (774E, 630C)	8–10	Schools in 10 districts throughout the US. 51% FRL, 43% W, 42% AA, 15% H	ETS End-of-course Algebra Exam	–0.06
Dynarski et al. (2007) (a variety of CAI applications)	Randomized (L)	1 year	28 schools 81 teachers (47E, 34C) 3136 students (1878E, 1258C)	6	Schools in districts throughout the US. 65% FRL, 35% H, 34% W, 31% AA	Stanford 10	+0.07
Becker (1990) (a variety of CAI applications)	Randomized (L)	1 year	Paired classes at 50 schools (24 schools randomized by student)	5–8	Schools throughout the US	Stanford Achievement Test	+0.07
Bailey (1991) (The High school math competency series, MECC conquering math series, and quarter mile)	Randomized (S)	1 year	4 classes (2E, 2C) 46 students (21E, 25C)	9	High school in Hampton, Virginia	ITBS scores <30th percentile	+0.70
Moore (1988) (Milliken math sequence)	Randomized (S)	9 months	8 classes 117 students (59E, 58C)	7–8	Remedial math students, half in special education	District math placement test	+0.24
Todd (1985) (Diascriptive Reading)	Matched (S)	1 year	2 schools 4 classes 302 students (161E, 141C)	6–8	Predominantly White; middle-class; Garland, Texas	ITBS	+0.91
McCart (1996)(WICAT ILS)	Matched Post Hoc (S)	6 months	2 schools 52 students	8	Semi-rural suburban school district in New Jersey	NJ-EWT	+1.20

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