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A Synthesis of the Intervention Research

CENTER ON INSTRUCTION

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CONTENTS

EXECUTIVE SUMMARY

This meta-analysis synthesizes experimental and quasi-experimental research on instruction that enhances the mathematics performance of students in grades 1-12 with learning disabilities (LD). For the purpose of this study, we defined *mathematical interventions* as instructional practices and activities that attempt to enhance the mathematics achievement of students with LD. In our analysis, we included only randomized controlled trials (RCTs) and quasi-experimental designs (QEDs) with clear evidence of pretest comparability.

The major findings are presented below:

Approaches to Instruction and/or Curricular Design

1. ***Explicit instruction.*** Explicit math instruction that incorporates step-by-step, problem-specific instruction resulted in increased gains in math performance for LD students. Results imply that explicit instruction should play a key role in mathematics instruction for students with LD. Teachers should model each step in the process of reaching the solution to a problem and think aloud about the strategies they use during problem-solving. Students must also be given many opportunities to solve problems using the strategies being taught, and should receive corrective feedback from the teacher when they experience difficulty.
2. ***Student verbalization of their mathematical reasoning.*** Student verbalizations of the solutions to math problems resulted in increased gains in their math performance. Students can verbalize the steps in a solution format (e.g., "First add the numbers in the *units* column. Write down the answer. Then add numbers in the *tens* column…") or in a self-questioning/answer format (e.g., "What should I do first? I should…"). Students can verbalize during initial learning, while they solve the problem, and/or after they have reached the solution.
3. ***Visual representations.*** Visually representing math problems (e.g., graphics, diagrams) had positive benefits on students' mathematics performance. Students who completed a visual representation prescribed by the teacher, rather than a self-selected representation, achieved

relatively larger gains in math scores. Visuals also resulted in larger positive effects when they were part of a multi-component approach to instruction, such as explicitly teaching a strategy that requires students to use visuals. Visual representations appear to be more beneficial if both the teacher and the students use them.

4. ***Range and sequence of examples.*** Well-designed lessons with carefully selected examples that cover a range of possibilities or are presented in a particular sequence resulted in higher mathematical gains for LD students. We hypothesize that carefully sequenced examples are probably beneficial for *initial* learning of skills, while a range of examples helps students transfer their newly acquired skills to *new* performance situations. Example selection should highlight critical features of the problems (e.g., a word problem indicating separating a set into "three equal parts" as opposed to "three parts") and provide students with opportunities to make decisions between various strategies.
5. ***Multiple and heuristic strategies.*** Multiple and heuristic strategy instruction appears to be an important, contemporary enhancement to explicit instruction. Like explicit instruction, using heuristics and teaching multiple strategies resulted in the strongest effects. We defined a heuristic strategy as a generic problem-solving guide in which the strategy (list of steps) is not problem-specific. Students could be exposed to multiple strategies and then be guided towards selecting and using a strategy of their choice.
6. ***Giving teachers ongoing formative assessment data and feedback on students' mathematics performance.*** Providing teachers with information about students' math performance led to gains in proficiency. Even stronger impacts were observed when teachers also received instructional tips and suggestions that helped them decide what to teach, when to introduce the next skill, and how to group/pair students, as informed by performance data.
7. ***Providing data and feedback to LD students on their mathematics performance.*** Providing feedback to students with disabilities about their math performance, while not being detrimental, did *not* result in large impacts. Similarly, giving LD students opportunities to set and review goals periodically based on their math performance feedback did *not*

appear to have any added value over only providing them with feedback. On the other hand, providing students with feedback on effort expended appears to be beneficial for students with disabilities.

8. ***Peer-assisted mathematics instruction:*** Cross-age peer tutoring appears to be more beneficial than within-class peer-assisted learning for students with LD. The effects for within-class peer-assisted learning were among the smallest found in the meta-analysis. We hypothesize that in many cases, students with LD may be too far below grade level to benefit from feedback from a peer. Older students, however, could be taught how to explain concepts to a student with LD who is several years younger.

INTRODUCTION

This report describes instructional components that succeed with students who have learning difficulties (LD). Our intent was to synthesize experimental and quasi-experimental research on instructional approaches that enhance the mathematics performance of school-age students with learning disabilities. We endeavored to include only randomized controlled trials (RCTs) and quasi-experimental designs (QEDs) in which there was at least one treatment and one comparison group, no major confounds, evidence of pretest comparability for QEDs, and sufficient data with which to calculate effect sizes.

In a seminal chapter on the nature of learning disabilities in the area of mathematics, Geary (2003) noted "the complexity of the field of mathematics makes the study of any associated learning disability daunting" (p. 199). Although no two researchers define a learning disability in mathematics in precisely the same way, prevalence estimates typically range from 5% to 7% of the school age population (Fuchs, Compton, Fuchs, Paulsen, Bryant, & Hamlett, 2005; Geary, 2003; Gross-Tsur, Manor & Shalev, 1996; Ostad, 1998).

Despite the imprecision of the operational definitions, two important bodies of research have emerged and, in some ways, crystallized during the past five years. The first concerns the characteristics that appear to underlie learning disabilities in mathematics. With one exception (i.e., Hecht, Vagi, & Torgesen, 2007), this body of research focuses only on the nature of learning disabilities related to mathematical operations and concepts involving whole numbers. The second body of research—and the focus of this publication—examines instructional intervention for this population.

Before describing our meta-analysis of the instructional intervention research, we briefly review some recurrent findings from the descriptive research on underlying cognitive deficits of LD students. Because they display problems in so many areas of mathematics, pinpointing the exact nature of the cognitive difficulty has been an intricate process.

Characteristics underlying learning disabilities in mathematics: A brief overview

One consistent finding is that students with learning disabilities tend to have great difficulty with retrieval of very basic number combinations, such as 7–5 or 3x7 (e.g., Garnett & Fleischner, 1983; Geary, 1993; Jordan & Montani, 1997; Ostad, 1997). These students seem to have great difficulty in both storing these facts in memory and retrieving them when they solve a problem. In fact, Geary (1993) considered this difficulty a defining feature of LD in mathematics.

It appears that, in general, students with learning disabilities have a very limited working memory (Geary, Hoard, Byrd-Craven, Nugent, & Numtee, 2007; McLean & Hitch, 1999; Swanson & Sachse-Lee, 2001); that is, they are unable to keep abstract information in their minds for the purpose of solving specific problems. The working memory problems go above and beyond retrieval of basic facts.

Another common characteristic is delayed adoption of efficient counting strategies. Students with learning disabilities will tend to count on their fingers well after their peers have outgrown this approach, and, when forbidden by their teachers, they count by using the stripes on the ceiling or the radiator. For example, they are slow to grasp and use the "counting on strategy." Most young children discover (or are taught by peers, siblings, or parents) that a far more efficient method for finding the answer to 2+9 is to (a) understand that 2+9 is equivalent to 9+2, and (b) start counting-on from 9 until reaching the number 11. Students with LD, in contrast, would tend to start with 2 and count up.

Finally, students with learning disabilities seem to have problems in many aspects of basic number sense, such as comparing magnitudes of numbers by quickly visualizing a number line and transforming simple word problems into simple equations (Jordan, Hanich, & Kaplan, 2003; Fuchs et al., 2005). In addition, two studies (DiPerna, Lei, & Reid, 2007; Fuchs et al.) have found that teachers' ratings of a child's attention span and task persistence are good indicators of the student's subsequent problems in learning mathematics.

The nature of intervention research in the field of LD in mathematics

Until recently, mathematics instruction was often treated as an afterthought in LD research. A recent review of the ERIC literature (Gersten, Clarke, & Mazzocco, 2007) found that the ratio of studies on *reading* disabilities to *mathematics* disabilities for the decade 1996–2005 was 5:1. This was a dramatic improvement over the ratio of 16:1 in the prior decade. However, it is far from a large body of research. Despite the limited knowledge about the precise nature of learning disabilities in mathematics, especially in areas such as rational numbers, geometry, and pre-algebra, researchers have attempted to develop interventions for students with LD. In fact, the number of high-quality studies examining the effectiveness of various instructional practices for teaching mathematics to LD students far surpasses the number of experimental instructional research studies conducted with students without disabilities.

We speculate that there are several reasons for this phenomenon. One important factor has been the consistent support for research in special education; annual research budgets for special education often surpassed budgets for research on the education of students without disabilities in academic areas.

Several studies exist that use rigorous experimental or quasi-experimental design to investigate the effectiveness of various instructional approaches for teaching LD students. Perhaps unfortunately, much of the intervention research has not been directly tied into the research from cognitive and developmental psychology. This phenomenon is not atypical in most fields of education. Rather, an array of traditions of research and scholarship has influenced the body of research we review in this publication.

One tradition that influenced some early research was cognitive behavior modification, which focuses on identifying and modifying biased or distorted thought processes and problematic behaviors through techniques that actively involve an individual's participation, such as self-monitoring and cognitive restructuring (Meichenbaum, 1980). Another influence has been the research on problem solving and metacognition in general (e.g., Mayer, 1987; Woodward & Montague, 2002). None of these traditions is unique to mathematics; they could be used for problems in science or other related technical areas. For example, Mayer proposed that students be explicitly taught the four processes necessary to solve most types of problems—translation, integration, planning and self-monitoring, and execution of a solution. Another generic approach to instruction that influenced this body of research was the importance of visualization (Bos & Anders, 1990; Manalao, Bunnell, & Stillman, 2000; Wittrock, 1991).

In addition, many instructional interventions use methods that have strong traditions of use in mathematics instruction. These include explicit instruction (Stein, Kinder, Silbert, & Carnine, 2006), the concrete-representational-abstract model defined by Bruner (e.g., Butler, Miller, Crehan, Babbitt, & Pierce, 2003) and situated cognition (Bottge, Heinrichs, Mehta, & Hung, 2002; Bransford, Vye, Kinzer, & Risko, 1990). Still others were developed uniquely to address issues that arise in mathematics instruction and were influenced by research on meaningful categorization of arithmetic word problems (e.g., Jitendra, Griffin, McGoey, Gardill, Bhat, & Riley, 1998; Riley, Greeno, & Heller, 1983) and the research on number sense and number lines (e.g., Okamoto, & Case, 1996).

Rather than conduct a historical, narrative review of the various intellectual traditions and their role in mathematics instructional research, we chose to conduct a meta-analysis and sort studies by major types of instructional variables.

A brief review of prior relevant research syntheses

We believe there is relevant empirical support for a research synthesis that focuses on mathematical interventions conducted for students with learning disabilities. This line of reasoning was most strongly supported in a study by Fuchs, Fuchs, Mathes, & Lipsey. (2000), who conducted a meta-analysis in reading to explore whether students with LD could be reliably distinguished from students who were struggling in reading but were not identified as having a learning disability. Fuchs et al. found that low-achieving students with LD performed significantly lower than students without LD. The average effect size differentiating these groups was 0.61 standard deviation units (Cohen's *d*), indicating a substantial achievement gap in reading. Given this evidence of differentiated performance between students with LD and low-achieving students without LD, we included mathematical interventions conducted only with LD students in our meta-analysis.

FINDINGS

In Table 1, we summarize the random effects mean (Hedges' *g*) and measure of heterogeneity of outcomes (*Q*) for each of the coding categories. We also present a forest plot of the categories in Figure 1. Note that in this section as we discuss each category, we refer to random effects mean as "mean effect size."

Table 1 indicates that all categories except for *student feedback with goal setting* and *within class peer-assisted learning* resulted in significant effects, that is, mean effect sizes significantly greater than zero. In the following sections, we present the findings for each category and discuss these findings in the context of the studies themselves. For most of our coding categories, the *Q* statistic was significant, indicating that outcomes were not consistent. However, we do attempt to explain what may be likely sources for some of the heterogeneity. In some cases, extremely large effects seem to have been caused, in part, by a control condition with a minimal amount of relevant instruction. In other cases, we simply speculate on sources of variance. We also try to provide the reader with a sense of some of the interventions that are included in this meta-analysis.

Approaches to instruction and/or curriculum design

Explicit instruction

Eleven studies met the criteria for inclusion in this category. Table 2 presents salient features of the studies and effect sizes. These studies cover all three mathematics domains: computation, word problems, and concepts in rational numbers (e.g., fractions). The studies span all grade levels: elementary, middle, and high school.

The mean effect size for the explicit instruction category is 1.22 (range = 0.08 to 2.15), which was statistically significant. The *Q* statistic for this category was significant, indicating the outcomes were heterogeneous. This heterogeneity justifies looking for systematic differences among studies within this category that may account for observed differences in the effect sizes.

The smallest effect size (0.08) was for the study by Ross and Braden (1991). One possible reason for this small impact may be related to the focus of the study, which was cognitive behavior modification (Meichenbaum, 1985) rather than a unique approach designed specifically for teaching a mathematical topic. Students were taught a strategy that lacks mathematical sophistication. Here are excerpts from Ross and Braden:

1. "What is my assignment for today?"
2. "What kind of problem is this, addition or subtraction? What does the sign say?"
3. "OK. It's addition—that means I'll be adding numbers."
4. "First I add the first column of numbers—that's 4+9" (p. 251).

In contrast, the largest effect size (2.15) is for the study by Xin, Jitendra, and Deatline-Buchman (2005). The instructional intervention in this study incorporated explicit instruction, but in this case the strategy is derived from research on how experts solve mathematical problems (e.g., Fuson & Willis, 1989). In Xin et al., students were taught that there are several distinct problems types (e.g., proportion, multiplicative, compare) involving multiplication and division. Students first identify what type of problem they have been given, then use a corresponding diagram to represent its essential information and the mathematical procedure necessary to find the unknown. Then they translate the diagram into a math sentence and solve it. In addition, unlike that of Ross and Braden, this intervention incorporates other variables associated with effective instruction such as sequencing instructional examples by presenting each problem type consecutively before having students work on a mixture of problems. Further, even though both experimental (schema-based instruction) and control (general strategy instruction) conditions required the use of visuals, a higher degree of structure and specificity was associated with the visual representations in the experimental condition. It may be that a combination of factors associated with effective instruction caused the relatively large impact.

The common thread among the studies in this category is the use of systematic, explicit strategy instruction. Explicit instruction occurred using a variety of strategies and covered a vast array of topics. For example, in Jitendra et al. (1998) and Xin et al. (2005), students were taught explicitly how to use specific visual representations to display the solution for a given problem type. In Owen & Fuchs (2002), explicit instruction was provided by graphically representing a given quantity through circles, and dividing those circles (i.e., the members of the set) into two sets of circles for the purpose of finding half of a given quantity.

In contrast, explicit instruction in Marzola (1987), Ross and Braden (1991), Tournaki (1993), and Tournaki (2003) was based on a student verbalization strategy. Students had to systematically verbalize each step that led to their solution of the problem. The 11 studies also showed variation along another dimension: narrow versus broad skill focus. In six studies [Lee (1992), Marzola (1987), Owen and Fuchs (2002), Ross and Braden (1991), Tournaki (1993), and Tournaki (2003)], the skill focus was very narrow—e.g., teaching students to find half of a given quantity (Owen & Fuchs) or one-step addition and subtraction word problems (Lee). The remaining five studies had a much broader focus.

Clearly, differences in effect sizes might have been caused by the diversity of the independent variables in the studies. However, the strength of explicit instruction as an instructional tool is supported by the statistically significant mean.

Student verbalization of their mathematical reasoning

Eight studies examined the impact of student verbalizations on improving mathematics performance. The effect sizes and other salient features of these studies are presented in Table 3. These studies involved computation, word problems, and/or concepts and operations involving rational numbers. The mean effect size for this category is 1.04 (range of 0.07 to 2.01). The Q for this category was significant, again indicating heterogeneity of outcomes across the studies.

As we examine the studies, we do see differences in the amount, specificity, and type of student verbalizations encouraged. Some studies gave students very specific questions to ask themselves. Others were based on cognitive behavior modification and were quite general. For example, Hutchinson (1993) taught students to ask themselves: "Have I written an equation?" or "Have I expanded the terms?" (p.39). Pavchinski's (1988) verbalizations had two purposes: self-instruction regarding attention to task (e.g., "What is my assignment for today? I need to start working now.") (p.33) and a more specific purpose related to solving the mathematics problems (e.g., "What does the sign say? OK, it's addition. That means…") (p.35). However, the data in Table 3 do not suggest that this factor was related to the magnitude of the effect.

Schunk and Cox (1986) instructed students to say what they were thinking out loud while solving problems. This approach resulted in the smallest effect size in this category (0.07). All of the other studies provided students with a set of questions or a template for thinking aloud. The largest effect size was 2.01 in the study by Marzola (1987), which is likely to be due to an artifact of the study: the control group received no instruction at all, only feedback on the accuracy of their independent work.

Using visual representations while solving problems

Twelve studies met the coding criteria for inclusion in this category. These studies were classified into two sets: those where both teachers and students used the visual representation (Table 4) and those where only the teachers used the visual representation (Table 5).

Teacher and student use of visual representations. In seven studies, initial teacher use of the visual representation was followed by mandatory student use of the same visual while solving problems. All of these studies, with the exception of Hutchinson (1993), focused on word problems. The mean effect size for this set of studies was 0.54 (range, 0.11 to 1.39) and was statistically significant. The Q for this category is not statistically significant, meaning the effect sizes represented relatively homogeneous outcomes. Teaching students to use some type or types of visual representation as a means for solving problems appears to be a consistently useful instructional technique in teaching mathematics to students with learning disabilities.

The largest effect size (1.39) in this category was for Owen and Fuchs (2002). This may be due to several factors. One is the specificity of the visual. The second is that the mathematical problems addressed in the study had a narrow focus: calculating half of the given numerical quantity. A third factor that may have contributed to the relatively large effect size is that students had to solve the problem using visuals and not just explain the problem using visuals. In fact, in all of the studies except Owen and Fuchs, graphic diagrams or pictures were used to explain or clarify the information presented in the problem. In Owen and Fuchs, students actually solved the problem in a graphic manner (e.g., draw circles for the numerical quantity for which half is to be found [eight circles for the number 8]; draw a rectangle and divide it into half to make two boxes; distribute circles evenly into the two boxes; determine the number of circles in one box to reach the answer).

Teacher use of visual representations. In five studies, the visual representations were used only by the teachers as they initially explained the target mathematics concepts and problems. In these studies, teachers used visuals to explain either computation problems or concepts relating to rational numbers (e.g., fractions). The studies used diverse, complex intervention approaches. With the exception of the study by Woodward (2006), which targeted the elementary school level, the studies were at the middle or high school level. The effect sizes of these studies and other salient features are presented in Table 5. The mean effect size was 0.41 and significant.

Range and sequence of examples

The nine studies included in this category appear in Table 6. The interventions in these studies were designed to include examples in a specified sequence or range that the authors hypothesized would facilitate learning. Five studies were conducted at the elementary school level, three at the middle school level, and one at the high school level. Effect sizes and other relevant aspects of the studies are described in Table 6. The mean effect size for this category is 0.82 (range = 0.12 to 2.15), which was statistically significant. The Q statistic was also significant, indicating heterogeneity of outcomes for this set of studies.

Sequences that highlight categories of problems that differ in superficial features but exemplify the same mathematical principle, as illustrated by the research of Xin and Jitendra and colleagues (Xin et al., 2005), appear to be quite effective. The largest effect size (2.15) in this category is associated with this study. Beirne-Smith (1991) examined the relative worth of presenting math facts in a sequential manner (2+4, 2+5, 2+6) that focused on drawing attention to the interrelationship among facts. This study had the smallest effect (0.12) when compared with other studies in this group. In Wilson and Sindelar (1991) and Woodward (2006), instructional examples progressed from simple/easy to more complex/difficult.

In the Butler et al. (2003) and Witzel, Mercer, and Miller (2003) studies, fractions/algebraic equations were taught first with concrete examples, then with pictorial representations, and finally in an abstract manner. The goal of the CRA (concrete-representational-abstract) instructional sequence was to ensure that students actually understood the visual representations before teachers used them to illustrate mathematical concepts. The authors of this study believe that, even in the secondary grades, students with LD need some brief period of time devoted to using concrete objects to help them understand the meaning of visual representations of fractions, proportions, and similar abstract concepts. Similarly, Kelly, Gersten, and Carnine (1990) taught fractions first with engaging graphical representations and then moved into abstract symbolic representations. Effect sizes for these studies were 0.29 for Butler et al., 0.50 for Witzel et al., and 0.88 for Kelly et al.

Fuchs, Fuchs, and Prentice (2004), Kelly et al. (1990), and Owen and Fuchs (2002) addressed the issue of range of examples in their instructional sequencing. Fuchs et al. exposed students to a range of problems that taught four superficial problem features (i.e., novel look, unfamiliar keyword, different question, larger problem-solving context) that could alter a problem without altering the structure or solution. The range was evident in the Kelly et al. study by the inclusion of a variety of fraction problems (e.g., proper, improper) and in the Owen and Fuchs study by the different ways in which the concept of half was addressed (e.g., symbol for half; word half, word one-half; etc).

The potential role of careful selection and sequencing of instructional examples to illustrate contrasts, build in-depth knowledge of mathematical processes, and highlight common features of seemingly disparate word problems seems to be quite important in helping students with LD learn mathematics. General effects were even larger for sequences of examples that helped teach students to apply the mathematics they know to contextualized word problems or in teaching concepts involving fractions and rational numbers. Sequences that highlight categories of problems that differ in their superficial features but exemplify the same mathematical principle (as illustrated by the research of Xin et al., 2005) appear to be quite effective.

Multiple/heuristic strategy instruction

All four studies included in this category were multifaceted, involving several of the other coded instructional variables. The mean effect size for this category was 1.56 (range, 0.54 to 2.45) and significant (see Table 7). The mathematical domain in Van Luit and Naglieri (1999) and in Woodward, Monroe, and Baxter (2001) was word problems; in Hutchinson (1993) it was fractions, and in Woodward (2006) it was a combination of fact retrieval and computational proficiency.

In Hutchinson (1993), a heuristic guide in the form of a self-questioning prompt card was used to teach fractions. The self-questioning prompts are not specific to a problem, as is the case with an explicit strategy. The prompts in the guide such as "Have I read and understood each sentence? Have I written out the steps of my solution on the worksheet?" (p.39) can be applied to any problem type. The three remaining studies taught students to apply multiple strategies and determine which situations were appropriate for each strategy.

In Van Luit and Naglieri (1999), the teacher first modeled several strategies for solving a computational problem. However, for most of the lesson, the teacher's task was to lead the discussion in the direction of using strategies and to facilitate the discussion of the solutions provided by the students. Each student was free to select a strategy, but the teacher assisted the children in discussion and reflection about their choices. The Van Luit and Naglieri study resulted in largest effect size in this category, 2.45.

In Woodward et al. (2001), multiple strategy instruction was part of the ad hoc tutoring that was provided to the students. The multiple strategy instruction is evident in these processes: "as different students suggested a strategy for solving the problem, the tutor probed the other students to see if they agreed and encouraged different individuals to work the next step in the problem… Explicit suggestions were made only after a substantial period of inactivity and after the tutor had determined that the students could not make any further progress without assistance" (p. 37).

Similarly, in Woodward (2006), students were taught multiple fact strategies. Daily lessons consisted of the introduction of new strategies or review of old strategies. Students were not required to memorize the strategies. They were, however, encouraged to discuss the strategy and contrast it with previously taught strategies. For example, students were shown that since 9x5 has the same value as 5x9, they were free to treat the problem as either 9 fives or 5 nines, and that this was true for all multiplication. They also were shown that this was equivalent to 10 fives minus one five, and that this could be a faster way to do this problem mentally. All of these options were discussed. The effect sizes in this set of studies support the potential for teaching students with LD more than one way to approach a problem, and indicate that there may be value in using this approach.

Other curriculum and instruction variables: complex "real world" problems

One study, Bottge et al. (2002), did not fit into any of our coding categories. This study explored the impact of *enhanced anchored instruction (EAI)*. The intent of EAI is to provide students with opportunities to apply mathematical principles and processes that are like real problems in a systematic, abstract fashion. Then students expand their use to the mathematics learned in complex problems which are designed to be engaging to adolescents. The underlying concept is that if students are asked to solve engaging problems using previously taught concepts, the enhanced motivation will result in dramatically increased engagement in the learning task. For example, students had to actually build a skateboard ramp using fractions and other computation skills.

Another unique feature of this intervention was that students were taught foundational knowledge using paper and pencil and texts, but application problems were typically presented via video or CD, rather than in traditional print. The effect size was 0.80, indicating some promise for this technique. Table 10 presents relevant information for this study.

Providing ongoing data and feedback to teachers on students' mathematics performance: the role of formative assessment

Seven studies met the criteria for inclusion in this category. All but two studies (Calhoon & Fuchs, 2003; Fuchs, Fuchs, Phillips, Hamlett, & Karns, 1995) included three experimental conditions and a control condition, enabling us to identify three orthogonal contrasts per study. By using orthogonal contrasts, the assumption of statistical independence was maintained, which is critical for a meta-analysis.

Overall, the seven studies resulted in a total of 10 contrasts. Consequently, the orthogonal contrasts were classified into two categories: teachers provided with formative assessment data, and teachers provided with feedback plus instructional enhancements (e.g., skills analysis, instructional recommendations, etc). Each is discussed briefly below.

Providing teachers with feedback on student progress

For seven contrasts, teachers were provided only with ongoing student performance data. Information on other key aspects of these studies (including effect sizes) is presented Table 10. Note that the major focus of these studies was on computation.

The mean effect size for this set of studies was 0.21 (range = 0.14 to 0.40) and significant. The *Q* for this category is not statistically significant, indicating that effects were relatively consistent. Note that the studies involved both special education and general education teachers, but only data from the special education students were included in the statistical analysis. Feedback was provided to the teachers periodically (in most cases twice a month). Treatments ranged from 15 weeks to two school years. The measures were deemed to be reliable and valid.

Providing teachers with feedback plus additional instructional enhancement

Three studies included an orthogonal contrast that allowed us to test for whether the enhancement provided significant benefit. The studies were mainly at the elementary level. See Table 10 for a description of the key features and the effect sizes for the three contrasts. The mean effect size for this set of studies is 0.34 (range = -0.06 to 0.48), which was statistically significant.

The instructional enhancements provided in these three studies helped teachers plan and fine tune their instruction. Allinder, Bolling, Oats, and Gagnon (2000) provided teachers with a set of written questions to help them use the formative assessment data to adapt instruction. These prompts included the following: "On what skill(s) has the student improved compared to the previous 2-week period?" "How will I attempt to improve student performance on the targeted skill(s)?" Teachers detailed their responses on a one-page form. They repeated the process two weeks later using both the new assessment data and the previous form to assist in decisions.

In Fuchs, Fuchs, Hamlett, Phillips, and Bentz (1994), each teacher received a set of specific recommendations as well as student performance data. Recommendations included: topics requiring additional instructional time for the entire class, students requiring additional help via some sort of small group instruction or tutoring, and topics to include in small group instruction, peer tutoring, and computer-assisted practice for each student experiencing difficulty. Fuchs, Fuchs, Hamlett, and Stecker (1991) also provided expert advice on instructional adjustments that could help address students' current difficulties based on their formative assessment data profiles.

Teacher feedback (all studies, all contrasts)

When we analyzed all the studies in this category, we noted that the set of studies is coherent and has a mean effect size of 0.23. Thus, providing feedback to teachers with or without additional guidance appears to be beneficial to students with LD.

Providing data and feedback to students with LD on their mathematics performance

Studies were sorted into two subcategories: studies that provided data and feedback to students on their performance or effort and studies that provided feedback that was also linked to some form of goal.

Feedback to students. There were seven studies in which feedback was provided to students on their performance, but no goal or expectation was attached. These seven studies covered all three mathematics domains and spanned all three grade levels (i.e., elementary, middle, and high school). Effect sizes and other key features are listed in Table 11.

The mean effect size for this category was 0.23 (range, -0.17 to 0.60), which was statistically significant. The *Q* statistic was not significant, indicating that the effects were relatively homogeneous. Consequently, we conclude that the studies in this category represent a consistent set of positive outcomes.

There were various sources for feedback in these studies—adults, peers, and software programs. In all studies except Schunk and Cox (1986), students were given feedback regarding their mathematical performance. This performance feedback ranged from a simple communication of the number correct to more extensive and in-depth communication systems that presented graphs of scores, skill profiles, and mastery status information (skills learned and not learned). In Schunk and Cox, feedback was given on effort expended (e.g., "You've been working hard."). Interestingly, this study had the largest impact (0.60) when compared with other studies in this category.

Feedback to students with goals. See Table 12 for the effect sizes and other salient features of the five studies included in this category. The mean effect size for this category was 0.13 (-0.34 to 1.14), which was not statistically significant. For Bahr and Rieth (1991), Fuchs et al. (1997), and Fuchs, Fuchs, Hamlett, and Whinnery (1991), the focus of the research question was the value that goal setting activities added to some other aspect of the intervention. In these three studies, feedback with goal setting was contrasted with feedback only. The lowest effect sizes in the "feedback to students with goals" category are associated with these three studies.

When the research question did not look into the value-added aspect of goal setting, as in Fuchs et al. (2004) and Reisz (1984), impacts appeared stronger. In fact, the largest effect size (1.14) in this category was in the study by Fuchs, Fuchs, and Prentice (2004), in which a combination of instructional variables, including feedback with goal setting, were contrasted with regular classroom instruction.

It appears that goal setting does not add value over providing feedback to students with LD about their mathematics performance. Given the problems many LD students have with self-regulation, feedback on progress by a teacher or peer may be far more effective than asking the students to take part in the goal setting process and then adjusting their learning based on their performance data.

Peer-assisted mathematics instruction

Eight studies met the criteria for inclusion in this category. Two studies used cross-age tutoring, and six studies focused on within class peer tutoring. These appear in Tables 13 and 14.

Cross-age tutoring

The two studies with cross-age tutoring are Bar-Eli and Raviv (1982) with an effect size of .75 and Beirne-Smith (1991) with an effect size of 1.15. In both studies, the tutors were upper elementary students who worked with students in the lower elementary grades in a one-on-one situation. Bar-Eli and Raviv focused on both computation and word problems and Beirne-Smith assessed computation only. The tutees in both studies were elementary students, while the tutors were well-trained upper elementary students. The main difference between the two interventions is that Bar-Eli and Raviv trained the tutors to actually teach lessons to the student with LD, whereas Beirne-Smith gave tutors a detailed protocol which specified the types of feedback to provide students when they experienced difficulty or made mistakes. Beirne-Smith also provided tutors with ideas on how to explain problem-solving strategies.

Within class peer-assisted learning

The six studies in this category covered all the domains of mathematics performance. Four of the six studies were at the elementary level, one was at the middle school level, and the other was at the high school level.

In sharp contrast to cross-age tutoring, the mean effect size for this category was 0.12 (range = -0.27 to 0.25), which was not statistically significant. The Q statistic was not significant, thus indicating that the outcomes were relatively homogeneous. Based on the evidence to date, within class peer-assisted learning does not result in beneficial impacts for students with LD.

A critical feature in the studies we reviewed is the amount and extent of training provided to students who assumed the role of tutor. Among these six studies, it generally appears that the training provided to tutors was sufficient for them to perform their tasks. There was extensive variation, however, in the roles and responsibilities of the members of the team or group.

The earlier studies gave the peer tutor quite constricted roles. For example, Slavin, Madden, and Leavey (1984a, 1984b) limited the role of the partner or tutor to providing feedback on the accuracy of a student's responses. The remaining studies provided increasingly more complex roles for the tutor. One other interesting factor to consider in the evolution of this body of research is that the early research of Fuchs et al. (1995) used the conventional tutor-tutee model where the tutor was the relative "expert" and the tutee the relative novice. In the latter studies by Fuchs and colleagues, tutoring is reciprocal in nature. In other words, students alternate between assuming the role of tutor and tutee.

This is one of the few areas where the mean effect size is not significantly different than zero, and effect sizes are consistently more modest than for most of the other areas. For students with LD, this practice seems to show some promise but more research needs to be done to examine whether it is, in fact, an effective practice. Findings do seem stronger for tutoring by a trained, older and/or more proficient student than by a peer.

DISCUSSION: IMPLICATIONS FOR PRACTICE

Many students experience problems learning mathematics, and quantitative abilities are critical for many jobs. Yet mathematics instruction for students with disabilities and those with learning difficulties has never received the attention provided to reading instruction. As can be seen by the dates of many of these publications, this situation has slowly but forcefully been changing.

We begin by offering several general observations about this body of research and its historical evolution. We then discuss implications for designing interventions for students who are struggling.

As we think back on these sets of studies, important historical trends become clear. First, we found more sophistication in recent studies. Often the newer interventions entail instructional models and principles derived from cognitive psychology and research on mathematics. Some of the more recent studies artfully integrate these ideas with the key principles of effective instruction for students with learning disabilities such as explicitness, careful sequencing to highlight key discriminations, and the provision of adequate practice with feedback. Others capitalize on the use of peer-assisted learning, often in the form of structured tutoring from a more proficient peer. Another major theme in this set of studies was the range of uses the researchers made of visual representations as they attempted to help students learn and apply these visual representations to solve increasingly complex word problems.

Authors of the studies struggled to find the precise language that describes what they attempted to do in the instructional intervention. By coding studies according to these major themes, we attempted to unpack the nature of effective instruction for students with learning disabilities in mathematics. Certainly, there is a need for much more unpacking of the nature of the independent variables. As instructional researchers work more closely with mathematicians and cognitive psychologists, this process will likely continue.

Each coding category in the meta-analysis was essentially an instructional variable and can be viewed as a potential aspect of effective mathematics instruction for students with LD. This is especially true because most coding categories led to significant effects.

Explicit instruction

Explicit instruction, a mainstay feature in many special education programs, was a key feature of many studies included in this meta-analysis. Teasing out the effect of explicit instruction is somewhat difficult, however, because it is often implemented in conjunction with other instructional principles.

Many studies referred to their independent variables as including explicit instruction. However, some of these studies were not included in the meta-analysis because they lacked some essential components characteristic of explicit instruction. To create a common basis for comparisons, we defined explicit instruction as instruction in which the teacher demonstrated a step-by-step plan (strategy) for solving the problem; the plan was problem-specific and not a generic, heuristic guide for solving problems; and students used the same procedure/steps shown by the teacher to solve problems.

Overall, the studies that used explicit instruction as an instructional delivery tool resulted in significant effects and produced some of the largest effects. This echoes findings documented in meta-analyses on instruction for students with learning disabilities in other domains (Gersten & Baker, 2001; Gersten, Fuchs, Williams, & Baker, 2001; Swanson & Hoskyn, 1998). Moreover, in the present meta-analysis, the large effects for explicit instruction were calculated in studies in which students were taught single or multiple skills.

Explicit instruction in a single skill. Studies in which the objective was to teach a single mathematical proficiency (e.g., single digit addition or finding half of a given quantity) resulted in large effects. These studies targeted developmentally appropriate proficiencies for younger learners. Because of the small number of studies, we must exercise caution in generalizing these findings to instruction of all single mathematical skills.

Explicit instruction in multiple skills. Explicit instruction as we have defined it was used in several studies in which students were taught to solve a wide variety of problem types (e.g., multi-step problem solving, fraction concepts, and fraction and whole number operations). Students in these studies tended to be older than those in studies of explicit instruction in a single skill. After factoring out studies with no-treatment controls (i.e., those studies that contained controls with no instruction) to minimize effect size inflation, the remaining studies still resulted in very large effects. In some cases, explicit instruction was used to teach rules for classifying problems into particular types as a bridge to selecting a process for solving each problem (Jitendra et al., 1998; Xin et al., 2005). This is what Lewis (J. Lewis, personal communication, October 23, 2007) calls "teaching students to apply the mathematics they know to novel situations."

Overall, these findings confirm that explicit instruction is an important tool for teaching mathematics to students with learning disabilities. It is important to note, however, that we did not conclude that explicit instruction is the only mode of instruction that can or should be used with students with learning disabilities.

Instructional planning: careful selection of examples

As with the explicitness of instructional delivery, thoughtfully planning instruction in mathematics appears to result in benefits to the learner who has learning disabilities. In particular, studies in which the instructional sequence and range of examples were carefully planned resulted in larger effects than when these variables were not considered. We believe that the sequence of examples may be most important during early acquisition of new skills when scaffolding may be most critical for student success.

The range of examples taught is probably most critical to support transfer of learning to new novel problem situations. In other words, if the teacher teaches a wide range of examples, it will result in the learner's being able to apply a skill to a wider range of problem types. Both of these planning options should be considered carefully when teaching students with LD. Given the nature of students' concerns about their ability to be successful, early success with new concepts and problems can be supported by sequencing examples and problems with increasing complexity, and ensuring that students have an opportunity to apply their knowledge to the widest range of problems to promote the transfer of their knowledge to unfamiliar examples.

Multiple/heuristic strategies

Another contemporary approach to instruction is the teaching of multiple strategies in a unit and providing students with opportunities to choose a relevant strategy and discuss the pros and cons of that strategy. Exemplified in only two studies (e.g., Van Luit & Naglieri, 1999; Woodward, 2006), the multiple/heuristic approach appears to offer students an opportunity to talk themselves through problems and to reflect on their attempts to solve problems. The notion is that this process may help students reach a higher level of understanding.

What remains unclear about the multiple strategy approach is whether it involves teaching a multi-step strategy or teaching multiple skills that can be employed to derive the solution to a problem. The practical implication may be that learning 7x8 as a memorized fact may be less cognitively demanding than learning to decompose 7x8 as (7x7) + 7. Additionally, what is unclear in the studies that have used this approach is whether students gain an understanding of number and number properties that is transferable to algebraic reasoning. For example, 7x8 could more appropriately be decomposed as (7x7) + (7x1) based on the distributive property. If the purpose is to improve student understanding as a result of the more strategic approach to learning, it would be best if it resulted in outcomes consistent with later mathematical expectations. Neither of the two studies provides definitive answers to these questions.

Visuals and graphics

Using visual or graphic depictions of problems to illustrate solution strategies for mathematical problems has been used intuitively by teachers for many years. Indeed, they are important. The results of this meta-analysis confirm what teachers have sensed: using graphic representations and teaching students how to understand them resulted in moderate effects. Results also suggest that the specificity of the visuals plays a major role in how the visual affects learning. For example, in Xin et al. (2005) both conditions involved visuals; however, the experimental group had a more specific visual that was developed based on research on how experts solve mathematical problems. Moreover, in D. Baker (1992), students were given multiple visuals but were not told which ones to use. This less specific approach resulted in less impact. In the studies in which visual diagrams resulted in positive effects, the visuals were used as part of a multi-component approach to instruction. Therefore, it is difficult to attribute these effects to the visuals alone. Nevertheless, it seems that visuals, when sufficiently specific, enhance outcomes.

Student verbalizations

Many students with learning disabilities are behaviorally impulsive. When faced with multi-step problems, they frequently attempt to solve the problems by randomly combining numbers rather than implementing a solution strategy step-by-step. One very promising finding is the positive impact on learning when students with LD are encouraged to verbalize their thinking, their strategies, or even the explicit strategies modeled by the teacher. This includes both the use of strategies derived from cognitive psychology to develop generic problem-solving strategies and more "classic" direct/explicit instruction in which students are taught one specific way to solve a problem, followed by extensive practice.

Verbalization may help to anchor skills and strategies both behaviorally and mathematically. The consistently positive effects suggest that verbalizing steps in problem-solving may be addressing students' impulsivity directly; suggesting that verbalization may serve to facilitate students' self-regulation during problem-solving. Unfortunately, it is not common to see teachers encouraging verbalization in special education. Our findings would suggest that it is important to teach students to use language to guide their learning.

Peer-assisted mathematics instruction

We hypothesize that the use of mathematical language may explain why, in some cases, peer tutoring activities can be successful. For students with LD, within class, peer-assisted learning has not been as successful as it has been with other students. Our interpretation of this finding is related to other findings regarding the degree of explicitness and scaffolding that appears to support the mathematics development of students with LD. It seems likely that peer tutoring efforts may fall short of the level of explicitness necessary to effectively help students with LD improve their performance. This interpretation is supported by the comparatively more positive effects of peer tutoring when the tutor is an older student and has received extensive training in tutoring. Although there are relatively few studies in this area, cross-age tutoring appears to work more effectively than tutoring when the tutor and the student are the same age or close in age. The ability of an older tutor to engage the learner in meaningful mathematical discourse may explain these differences. Practically speaking, however, cross-age tutoring can present logistical difficulties in some cases.

Providing ongoing feedback

One clear finding is that providing regular classroom teachers with specific information about each student's performance enhances math achievement. However, giving specific information to special educators consistently indicates even stronger effects. It may be the case that special educators are better prepared to use detailed student performance data to set individual goals for students because they are familiar with creating individual education plans.

While highly specific information is particularly useful for special educators, it is apparently less so for general education teachers—this strategy had an extremely small impact on the performance of students with LD. One possible reason is that much of the content of math curricula may be too difficult for LD students. A series of observational studies of mathematics instruction with students in the intermediate grades (Williams & Baxter, 1996; Woodward & Baxter, 1997) suggests this can be the case. Another possible factor is that the few studies on this topic were large-scale field experiments and the variation in implementation may have dampened effects.

Results also suggest that giving special educators specific suggestions or problem packets linked to students' identified needs can dramatically increase the effectiveness of the feedback provided to special educators. Although this is based on only one study (Fuchs, Fuchs, Hamlett & Stecker, 1991), it is a critical piece of information. As schools or districts develop and implement progress monitoring systems in mathematics, graphs of student performance can be augmented with specific instructional guidelines and curricular materials for teachers, special educators who may co-teach or provide support services, peer tutors, cross-age tutors, and adults who provide extra support.

In summary, findings converge regarding the practice of providing teachers with precise information on student progress and specific areas of students' strengths and weaknesses in mathematics for enhancing achievement for this population. This is more likely to occur if the information on topics or concepts that require additional practice or re-teaching is precise. It appears that teachers and students also benefit if teachers are given specific software lessons or curricula so that they can immediately provide relevant instructional material to their students.

Even though feedback about student performance on specific problems or problem types was beneficial when given to teachers, providing students with LD with similar feedback was less productive. In fact, the largest effects related to feedback were documented for providing students with non-specific feedback on effort, rather than specific performance feedback. There seemed to be no benefit to providing LD students with specific feedback.

NEXT STEPS

An impressive feature of this body of research is the increasing concern with the actual mathematics being taught (e.g. Fuchs et al., 2004; Woodward, 2006). A goal of future research and professional development efforts would be to examine the impact of curricula that have been reviewed by research mathematicians and promote teachers' and students' use of precise, mathematically accurate language (National Mathematics Advisory Panel, 2008). For example, students or teachers should not say that 7x6 is the same as 6x7. The first term refers to 7 groups (sets) of 6, and the latter to 6 groups of 7. The answer is the same, or more formally, the value is the same. Knowing precisely what 7x6 means is, in the view of many research mathematicians (e.g., Wu, 2001) and mathematics education researchers (e.g., J. Hiebert, personal communication, October 23, 2007), a key to future success or failure in more abstract courses such as algebra.

METHOD

Selection of studies: literature review

For the purpose of this study, we defined *mathematical interventions* as instructional practices and activities that attempt to enhance the mathematics achievement of students with LD. We operationally defined LD as students with an Individual Educational Plan (IEP) in mathematics and a designation of LD by the school district. Thus we remind the reader that the conclusions reached are limited in most cases to students identified as possessing learning disabilities in mathematics. It is unclear how well these principles apply to students who do not have learning disabilities in mathematics. However, the National Mathematics Advisory Panel (2008), Baker, Gersten, and Lee (2002), and Kroesbergen and Van Luit (2003) noted that many of these same principles are effective for students considered "at risk" or "low achieving." It is, of course, possible, even probable that many of these techniques will also succeed with non-diagnosed students who are experiencing difficulties in learning mathematics.

We reviewed all studies published from January 1971 to August 2007 that focused on *mathematics interventions* to improve the mathematics proficiency of school-age students with LD. Two searches for relevant studies were undertaken. The first search encompassed the time period from 1971 to 1999, and included peer-reviewed studies as well as doctoral dissertations. The second search extended to August 2007, but limited the search to peer-reviewed studies. Except in that one regard, the methodologies were virtually identical.

In each case, the first phase began with a literature search using the ERIC and PSYCHINFO databases. The following combinations of descriptors were used in the first search: *mathematics achievement, mathematics education, mathematics research, elementary education, secondary education, learning disabilities, and learning problems.* We conducted a systematic search of *Dissertation Abstracts International* to review dissertations for possible inclusion. We also examined the bibliographies of research reviews on various aspects of instructional intervention research for students with learning disabilities (i.e., Kroesbergen & Van Luit, 2003; Maccini & Hughes, 1997; Mastropieri, Scruggs, & Shiah, 1991; Miller, Butler, & Lee, 1998; Swanson & Hoskyn, 1998; Swanson, Hoskyn, & Lee, 1999) for studies that may not have been retrieved from the computerized search. Finally, we conducted a manual search of major journals in special, remedial, and elementary education to locate relevant studies.

These search procedures for the period between 1971 and 1999 resulted in the identification of 579 studies. Of this total, 194 studies were selected for further review based on an analysis of the title, key words, and abstracts. Of these 194 studies located in the first search, only 30 (15%) met the criteria for inclusion in the meta-analysis.

Research studies from 1999 to August 2007 were located using a similar, but streamlined search procedure. For this literature search, we used the terms *mathematics and LD or arithmetic and LD*. The second search resulted in a pool of an additional 494 potential studies. We also narrowed this set of studies to 38 by reviewing the title, keywords, and abstracts. Finally, 14 of the 38 studies (37%) were selected as meeting the criteria for inclusion in the meta-analysis. Thus, the two searches resulted in a total of 44 research studies.

Determining if a study met the pre-established criteria for inclusion was done by two of the authors; any disagreements were reconciled. To ensure that our inclusion criteria were reliable, two of the authors independently examined 13 of the studies in the second round. Inter-rater reliability was 84.6%. The authors initially disagreed on the inclusion of two of the 13 studies; however, after discussion they reached consensus.

Criteria for inclusion

We used the following three criteria to determine whether to include a study in this meta-analysis:

Purpose of the study. The study had to focus on an evaluation of the effectiveness of a well-defined method (or methods) for improving mathematics proficiency. This could be done in the following ways: (a) use of specific curricula or teaching approaches to improve mathematics instruction (e.g., teacher use of 'think-aloud' learning strategies; use of real world examples); (b) use of various classroom organizational or activity structures (e.g., peer-assisted learning); or (c) use of formative student assessment data to enhance instruction (e.g., curriculum-based measurement data; goal setting with students using formative data). Studies that only examined the effect of test-taking strategies on math test scores, that taught students computer-programming logic, or focused on computer-assisted instruction (i.e., technology) were not included. We felt that computer-assisted instruction would be more appropriate for a meta-analysis in the area of technology. Studies that assessed the achievement impact of changes in structural or organizational elements in schools, such as co-teaching or inclusion—but did not address a specific instructional approach—were excluded, even though they may have included a mathematics achievement measure (in this decision, we differed from Swanson and Hoskyn, 1998).

Design of the study. We limited the search to studies that could lead to strong claims of causal inference, that is, randomized controlled trials or quasi-experimental designs. No single subject or multiple baseline studies were included, since they cannot be integrated into a meta-analysis. Quasi-experiments were included as long as students were pretested on a relevant mathematics measure and one of the following three conditions were met: (a) researchers in the original study adjusted posttest performance using appropriate analysis of covariance (ANCOVA) techniques; (b) authors provided pretest data so that effect sizes could be calculated using the Wortman and Bryant (1985) procedure or (c) if posttest scores could not be adjusted statistically for pretest differences in performance, there was documentation showing that no significant differences existed between groups at pretest on relevant measures of mathematics achievement.

Participants in the study. The participants were students with identified learning disabilities. A study with students without learning disabilities was included only if one of the following criteria was met: (a) separate outcome data were presented for the different participant groups so that effect sizes could be computed separately for students with LD; or (b) if separate outcome data were not presented for students with LD, then over 50% of the study participants needed to be students with LD.

All studies provided operational definitions of LD. These definitions pertained either to state regulations regarding LD (e.g., Fuchs et al., 1994) or district regulations (e.g., Marzola, 1987). In some cases (e.g., Bar-Eli & Raviv, 1982), the authors ensured that there was a 15-point discrepancy between IQ and mathematics achievement on a normed measure. In many cases, researchers used performance data from standardized tests to confirm that students' mathematics performance was well below expected normative levels. Thus, our sample would be considered a sample of school-identified LD students.

Coding of studies

Phase I coding: quality of research design

We coded the studies that met the final eligibility criteria in three phases. In Phase I, two of the authors examined the design of each study to ensure that it was methodologically acceptable. The design features of each study are listed in Appendix A. In our analysis, we noted if the study was a randomized controlled trial (RCT) or quasi-experimental design (QED) based on whether or not students were randomly assigned to intervention conditions[1]. For quasi-experiments, we only considered them acceptable if students were pretested on a mathematics performance measure and sample means were within .25 SD units of each other on the pretest. Finally, we report on the unit of analysis (class or student) and whether the unit of analysis and unit of assignment were the same. This information is important for statistical analyses; a mismatch can lead to spurious inferences since it fails to account for clustering at the classroom level (Bryk & Raudenbush, 1992; Donner & Klar, 2000; Gersten & Hitchcock, in press).

Phase II coding: describing the studies

In Phase II, all studies were coded on the following variables: (a) mathematical domain, (b) sample size, (c) grade level, (d) length of the intervention, and (e) dependent measures. We also determined who implemented the intervention (i.e., classroom teacher, other school personnel, or researchers), if fidelity of treatment was assessed, whether posttests should be categorized as immediate posttests, transfer measures or maintenance tests, and whether scoring procedures for relevant mathematics performance scores included inter-rater agreement procedures.

Operational definition of mathematical domain. We used the work of the National Research Council (2001), Fuchs and colleagues (e.g., Fuchs, Fuchs, Hamlett, & Appleton, 2002; Owen & Fuchs, 2002), and the National Mathematics Advisory Panel (2008) to identify and operationalize four mathematical domains of math achievement. These domains were (a) computation, (b) word problems, (c) concepts and operations involving rational numbers, and (d) algebra.

We defined *computation* as basic operations involving whole numbers (i.e., addition, subtraction, multiplication, and/or division). The domain of *word problems* includes all types of word problems—those requiring only a single

[1] There were three studies (D. Baker, 1992; Fuchs, Roberts, Fuchs, & Bowers, 1996; Slavin, Madden, & Leavey, 1984b); where randomization was imperfect. In one case, random assignment was conducted in all but two of the five school sites. In another, all but two teachers were assigned randomly. In the third case, a replacement student was chosen to match a student who could not participate. We considered these studies as RCTs for the purpose of this meta-analysis.

step, those requiring multiple steps, those with irrelevant information, and those considered by the authors to be "real world" problems. Problems in the domain *concepts and operations involving rational numbers* require solutions that are based on an understanding of fractions, decimals, and equivalents. Note that some of these problems could be computational in nature: for e.g., 1/3 +1/7, .07+ .4, locating 8/5 on a number line. *Algebra* was defined as simple algebraic procedures.

Dependent measures. We determined if a measure was researcher-developed or a commercially available norm-referenced test. We also categorized the measures in terms of the skills and knowledge involved in solving the problems. For example, did a measure test a range of skills as did the Wide Range Achievement Test (WRAT), focus on a narrow skill area such as the Math Operations Test-Revised, or address general mathematics such as the Test of Mathematical Abilities? Finally, we determined the alignment between the focus of the intervention and the skills and knowledge being assessed by each measure.

Authors varied in terms of how they defined immediate posttest versus maintenance test, and what they considered a transfer test. Some considered a test given two days after a unit was complete a maintenance test. Some authors were extremely liberal in what they considered a transfer item (e.g., a word problem with a similar structure to what had been taught, but using slightly different words from those in the curriculum). We needed uniform operational definitions so that we could synthesize findings across disparate studies.

Consequently, we defined posttests, maintenance tests, and transfer tests in the following manner:

Posttest. A posttest has to measure skills covered by the instructional intervention. If the posttest measured new skills not covered during instruction, we made a note of it for subsequent use in interpreting the findings. In addition, most posttests were given within three weeks of the end of the instructional intervention. If a posttest administration extended past the three-week period we made a note of it.

Maintenance test. A maintenance test is a parallel form of the post test given three weeks after the end of the instructional intervention to assess maintenance of effects (i.e., retention of learning). If a maintenance test was given earlier than three weeks, we designated it as a posttest, and used it in our outcome calculations.

Transfer test. A transfer test measures students' ability to solve problems that they were not exposed to during instruction. We used the definition of *far transfer* used in the work of Fuchs et al. (2002), and Van Luit and Naglieri (1999). Far transfer tests include tasks that are different (sometimes substantially) from the tasks students were exposed to during the instructional intervention. For example, if the instruction covered single-digit addition problems, the far transfer test could include two-digit addition or three-digit addition problems. Similarly, if the instruction was on mathematical operations (addition, subtraction, division, and multiplication), the far transfer test could include problems requiring application of these skills (e.g., money, measurement, word problems, interpretation of charts or graphs, etc). If the word problems included additional steps or asked the student to discern which information was irrelevant, these too were considered far transfer problems. To solve these problems, students would have to transfer their knowledge to new situations. Only far transfer measures were used in calculating transfer effect sizes in this study. Thus, only a small number of studies (nine) were identified as having transfer measures.

Many of the measures some authors considered *near transfer* items were considered as posttests since near transfer measures require students to solve problems that closely resemble the ones used during instruction. Thus, the problems on the near transfer measure differ from the posttest tasks in minor ways—new numbers/quantities (change 23 + 45 to 13 + 34; six candies to 4 candies), different cover stories (buy pencils instead of erasers), and different key-words (how many boxes versus how many sacks). We included all near transfer tests in our outcome (posttest) calculations.

Finally, measures that were parallel forms of posttests (and so stated in the manuscripts) were not considered as transfer tests, but were coded as either second posttests or maintenance tests (depending on when they were administered). See Appendix B for information on the manner in which posttests, maintenance tests, and transfer tests were classified for each study.

Exclusion of studies during phase II coding. During Phase II coding, we excluded three studies from the meta-analysis. Friedman (1992) was excluded because the dependent measure *Wide Range Achievement Test* (WRAT) was poorly aligned with the intervention; the WRAT only assesses computation and the intervention focused on word problems. Greene (1999) was excluded from the meta-analysis because of a confounded design. Jenkins (1992) was excluded because the differential attrition in this study exceeded 30% and there was no attempt to conduct an intent-to-treat analysis.

Table 15 lists the 41 studies included in the meta-analysis and their characteristics. (Note: total number of experiments/quasi-experiments was 42; one of the articles included two different experiments.)

Phase III coding: determining the nature of the independent variable(s)

The primary purpose of Phase III coding was to determine a set of research issues that could be explored in this set of studies. Two of the authors developed a coding scheme for the selected set of studies through an iterative process that spanned several months. During the first reading of the article, we coded according to a broad category (e.g., curriculum design, providing ongoing feedback to teachers and students). We then reviewed these initial codes, reviewed our notes, and reread relevant sections of each article to pinpoint the precise research questions addressed. This involved rereading all of the studies by at least two of the authors. The authors completed all the coding at this level, although we often involved research assistants in discussions.

In our final analysis, we settled on four major categories for the studies. These categories include (a) approaches to instruction and/or curriculum design, (b) providing ongoing formative assessment data and feedback to teachers on students' mathematics performance, (c) providing data and feedback to students with LD on their mathematics performance, and (d) peer-assisted mathematics instruction.

The coding categories for each of the 42 interventions included in the meta-analysis also appear in Table 15. The four broad categories were broken down further into specific subcategories. See Figure 2 for a description of the categories and subcategories. The process of identifying these specific subcategories was iterative and involved two authors and spanned several months.

Note that several studies included three or more instructional conditions, and thus addressed several research questions. These studies were therefore coded into more than one category whenever applicable. We always used orthogonal contrasts to capture the unique research questions posed.

Some research studies had complex instructional interventions that were based on fusing instructional variables (e.g., use of visuals and explicit instruction). These studies were also coded whenever applicable into more than one category. However, at no point during the meta-analysis were two categories with the same complex intervention ever compared with each other.

In the next section, we describe and present the operational definitions of the four major categories.

Coding Categories

1. Approaches to instruction and/or curriculum design

Under this category we list six elements of instruction: explicit instruction, student verbalizations of their mathematical reasoning, visual representations, range and sequence of examples, multiple/heuristic strategies, and other instructional and curricular variables.

Explicit instruction. A good deal of the special education literature in mathematics has called for instruction to be explicit and systematic (e.g., Fuchs & Fuchs, 2003; Gersten, Baker, Pugach, Scanlon, & Chard, 2001; Swanson & Hoskyn, 1998). However, the term is used to describe a wide array of instructional approaches. We found a reasonable amount of variance in the way explicit instruction was defined in the studies reviewed. In order to operationalize the construct, we only coded examples of systematic, explicit instruction if they possessed the following three specific components: (a) the teacher demonstrated a step-by-step plan (strategy) for solving the problem, (b) this step-by-step plan needed to be specific for a set of problems (as opposed to a general problem-solving heuristic strategy), and (c) students were asked to use the same procedure/steps demonstrated by the teacher to solve the problem.

Student verbalizations of their mathematical reasoning. Student verbalization or encouragement of students' thinking aloud about their approach to solving a problem is often a critical component in scaffolded instruction (e.g., Palincsar, 1986). Approaches that encourage and prompt this type of verbalization have been found to be effective for students with LD in a wide array of curricula areas, including content-area subjects such as history and science, as well as foundational areas such as reading and math (Baker, Gersten, & Scanlon, 2002).

Most discussions of mathematics teaching note that a key component of effectiveness is "manag(ing) the discourse around the mathematical tasks in which teachers and students engage … (Teachers) must make judgments about when to tell, when to question, and when to correct. They must decide when to guide with prompting and when to let students grapple with a mathematical issue" (NRC, 2001; pp. 345). The process of verbalizing how to solve problems should encourage students to select an appropriate representation and, in discussion with peers and/or their teacher, evaluate its relevance. It also can lead to discussions of which strategies apply to which particular situations (Van Luit & Naglieri, 1999).

Several researchers (e.g., Van Luit & Naglieri, 1999; Woodward et al., 2001) have attempted to provide guidance to teachers on how to make these judgments and provide appropriate prompts for students with learning disabilities. They argue that all students, including those with disabilities, should participate in discussions of alternate ways to solve a problem and be encouraged to express their thoughts on the approaches that make sense to them mathematically. To be included in this category, the instructional intervention had to include some aspect of student verbalizations (e.g., verbalizing solution steps, self-instruction, etc.).

Visual representations. Visual representations of mathematical relationships are consistently recommended in the literature on mathematics instruction (e.g., Griffin, Case, & Siegler, 1994; NRC, 2001; Witzel et al., 2003). The National Research Council Report notes that "mathematical ideas are essentially metaphorical (pp. 95) ... Mathematics requires representations… Representations serve as tools for mathematical communication, thought, and calculation, allowing personal mathematical ideas to be externalized, shared and preserved. They help clarify ideas in ways that support reasoning and building understanding" (pp. 94).

In order for a study to be included in this category, the following had to be evident: (a) either the students had to use the visual representation while solving the problem, or (b) the teacher had to use the visual representation during initial teaching and or demonstration of how to solve the target problem. If the study focused on student use of the visual, we required that use be mandatory and not an optional step for students working to solve the problems.

Range and sequence of examples. The literature on effective mathematics instruction stresses the importance of example selection in teaching concepts to students (e.g., Ma, 1999; Silbert, Carnine, & Stein, 1989; Witzel et al., 2003). To be included in this category, studies needed to assess the effectiveness of either (a) a specified sequence/pattern of examples (concrete to abstract, easy to hard, etc), or (b) represent systematic variation in the range of examples (e.g., teaching only proper fractions vs. initially teaching proper and improper fractions).

Multiple/heuristic strategies. To be included in this category, the intervention had to include either a heuristic strategy guide or instruction in multiple strategies. A heuristic strategy guide is a generic guide that is not problem-specific and can be used in organizing information and solving a range of math problems. For example, it can include steps such as "Read the problem. Highlight the key words. Solve the problems. Check your work." It is conceptually the opposite of explicit strategy instruction, wherein the steps of a given strategy are specific to solving a particular type of math problem. Multiple strategy instruction involves exposing students to different ways of solving a problem. Students are taught to evaluate strategies (sometimes through discourse and reflection) and finally select a strategy for solving the given problem.

Other instructional and curricular variables. These included studies with other instructional and curricular variables that did not fall into the above-mentioned categories.

2. Providing ongoing formative assessment data and feedback to teachers on students' mathematics performance: the role of formative assessment

Ongoing assessment and evaluation of students' progress in mathematics can help teachers measure the pulse and rhythm of their students' growth in mathematics, and also help them fine-tune their instruction to meet the needs of their students. We were interested in determining the effects of teacher monitoring of student performance on students' growth in mathematics, that is, the indirect impact of the use of assessments. To be included in this category, the teachers had to be provided with information on student progress. This information could be (a) only feedback on student progress or (b) feedback plus additional instructional enhancement (e.g., skill analysis, instructional recommendations, etc.).

3. Providing data and feedback to students with LD on their mathematics performance

Providing students with information on their performance or effort is considered by many to be a key aspect of effective instruction. Information about performance or effort may serve to reinforce student effort positively, it may serve as a way to keep students accountable for staying engaged in working as expected on the business of learning mathematics, and it may provide useful information for students in understanding where they have been successful and unsuccessful in their learning. For studies too be included in this category, students had to receive some sort of feedback on their performance or effort. The students could have received (a) only feedback (e.g., positive and/or corrective feedback; recommendations about additional problems to work on or areas to study) or (b) feedback tied to a specific performance goal. The feedback could also be from a variety of sources including teachers (e.g., Schunk and Cox, 1986), other peers (e.g., Slavin et al., 1984a), and computer software programs (e.g., Bahr & Rieth, 1991).

4. Peer-assisted math instruction

Students with LD are often provided some type of assistance or one-on-one tutoring in areas for which they need help. Sometimes, students' peers provide this assistance or one-on-one tutoring. There are two types of peer tutoring. The more traditional is cross-age, wherein a student in a higher grade functions primarily as the tutor for a student in a lower grade. In the newer within-classroom approach, two students in the same grade essentially tutor each other. In many cases, a higher performing student is strategically placed with a lower performing student but typically both students work in the role of the tutor (provides the tutoring) and the tutee (receives the tutoring). Role reciprocity is very important in this approach. Typically, the higher performing child is paired with a lower performing child and in the experience, the higher performing child is able to provide models of strong mathematics skills for the lower performing child. For example, in working on a set of problems, the higher performing child will work on the problems first and the lower performing child will provide feedback. Then roles will be reversed and the lower performing child will work on problems for which he or she just had a model for how to solve them. Or, in explaining a math solution, the higher performing child will provide the explanation first and the lower performing child will have had a model for a strong explanation.

In studies on peer tutoring, students typically work in dyads and training is quite clear and explicit regarding roles and responsibilities students are to assume while working with their partners. Generally, students use their time in peer-assisted instruction practicing math problems for which they have received previous instruction from their teacher. To be included in this category, the studies had to incorporate some form of peer-assisted instruction as their independent variable.

DATA ANALYSIS

Effect size computation

Effect sizes for each contrast were calculated as Hedges' *g*, the difference between the experimental and comparison condition means divided by the pooled standard deviation (Cooper & Hedges, 1994). All estimates were then corrected for small sample bias using procedures outlined by Hedges (as cited in What Works Clearinghouse, 2007; http://ies.ed.gov/ncee/wwc/pdf/conducted_computations.pdf).

For studies that reported both pretest and posttest scores, we calculated posttest effect sizes adjusting for pretest performance (i.e., $g_{adjusted} = g_{posttest} - g_{pretest}$; see Wortman & Bryant, 1985). This technique is especially useful for quasi-experimental studies or any study reporting initial non-equivalence of groups on a pretest measure. Our prior work has indicated that this adjustment provides more accurate gauges of effect size than simple unadjusted posttest effects (Baker et al., 2002; Gersten & Baker, 2001). These values were also corrected for small sample bias using the Hedges (1981) correction.

In this meta-analysis we encountered several unusual issues while computing effect sizes. They are as follows:

Effect size computation for studies with three or four experimental conditions. Many of the studies in our sample reported outcomes from two or three experimental conditions, each involving different combinations of instructional components. We could not compare each condition to the control group because the set of contrasts would not be orthogonal. We therefore developed either two or three orthogonal contrasts based on the research questions posed by the study's authors. We thus were able to compute two or three *g*s that were orthogonal and also addressed a specific research question (Hedges, personal communication, 2003).

Effect size computation for studies with the classroom as the unit of analysis. Four research studies assigned classes to treatment conditions and assessed all of the students with LD in the class on pretest and outcome measures, but then entered the mean score from one to four selected LD students into the analysis of variance. While appropriately analyzing treatment effects at the level of assignment for the *F*-ratios and *p* values present in the study, the variance reported in the studies is problematic for meta-analysis. That is because effect sizes at the classroom level will tend to be somewhat inflated. Had the authors reported the ratio of between-class to within-class variance (ICC) we could have adjusted the Level-2 variance reported to the total variance (Level-2 + Level-1) required. Without the ICC report, an alternative for estimation was found in unmoderated ICC values reported by Hedges and Hedberg (2007, p. 72). These ICCs were further adjusted based on the differential ratios of Level-2 to Level-1 units in data sets from which they were drawn to sample sizes in studies analyzed here. Adjustment of *g* from these studies was then calculated:

$$g_{ICC} = g\sqrt{ICC_{adj}} \quad \text{Where:} \quad ICC_{adj} = ICC\left((n_{level2} / n_{level1})_{study} \Big/ (n_{level2} / n_{level1})_{dataset}\right)$$

Aggregation and comparison across factors. Typical studies reported effects for multiple factors other than treatment group (e.g., gender, grade-level, measurement-type, or measurement-time-point). In addition, treatments themselves range in complexity from single component (e.g., same-grade peer tutoring) to multiple component interventions (e.g., peer tutoring + student feedback + goal setting). Considered separately, these factors divide into *instructional components* (e.g., use of peer-assisted learning, example sequencing, use of think-aloud procedures) *participant factors* (e.g., gender or grade-level), and *end-point factors* (e.g., measures or time-points), each of which was aggregated differently depending on their importance to the study and their estimability (Seely & Birkes, 1980).

For the present analysis, stratified analyses of treatment components allowed consideration of complex intervention effects in multiple treatment categories. Participant and endpoint factors, however, were aggregated to one effect size estimate per study. For participant factors (e.g., gender, grade-level) where each participant may be considered an independent unit for analysis, summary statistics (i.e., *mean, sd and N*) were aggregated to single values using a procedure attributed to Nouri and Greenberg (see Cortina & Nouri, 2000). For studies that reported multiple endpoints (i.e., multiple posttest measures or parallel versions of a test administered within one to 15 days after intervention concluded) different procedures were employed. Both of these endpoint off-factors may be expected to have correlated error that required different approaches.

In cases of parallel forms of a posttest being administered at multiple time points within 15 days of intervention, we treated these as a larger measure at a single time-point (i.e., Total score = $N_{items} \times k_{time\text{-}points}$). To aggregate multiple time-points a modification of the Nouri and Greenberg formula was used (see formula in Figure 3). For studies reporting outcomes with multiple measures a different approach was used: An effect size for each measure was first computed and effects so-computed were then combined into a simple average effect (i.e., $g_{average} = g1 + g2 \ldots + gk / k$).

Although simple averaging implies the risk of overestimating the aggregated effect by underestimating the variance among measures, and remedies for this problem do exist (e.g., Gleser & Olkin, 1994; Rosenthal & Rubin, 1986), these remedies require correlational information which may be neither reported nor directly estimable for meta-analysis (excepting cases where raw data are available). Also, while statistically meaningful, the difference of averaging effects and computing a latent effect from multiple measures may be small (see Appendix C for a worked example). For this study we judged such averaging to permit a reasonable approximation of the true score effect, capitalizing on the unit-free nature of the standardized mean difference statistic (i.e., *g*).

In some studies a combination of factors were presented for aggregation (e.g., multiple groups, time-points, and measures), which required systematic application of the aggregation strategies described. Once computed, a sensitivity analysis of aggregated effects was conducted by regressing effect size onto the number of groups, time-points, and measures aggregated in a fixed-weighted analysis. This analysis revealed no systematic biasing (r_{max} = .10). Thus, having applied these selection and estimation procedures systematically, we calculated a pool of independent effect sizes (N = 51) for meta-analysis.

Q statistic. For each instructional component (e.g., explicit instruction, feedback to students) we determined if the *g*s were consistent across the studies (i.e., shared a common effect size) by calculating a homogeneity statistic Q (Hedges & Olkin, 1985). The Q-statistic is distributed as chi-square with $k - 1$ degrees of freedom, where k is the number of effect sizes (Lipsey & Wilson, 2001) and is: $Q = \sum_{j=1}^{N} w_j \cdot \left(g_j - \bar{g}_{..}\right)^2$ or $Q = \sum_{j=1}^{N} wght_j \times \left(ES_j - \overline{ES}_{..}\right)^2$. A significant chi-square indicates that the moderator variables significantly influenced the magnitude of effect sizes.

Having established that variance exceeded more than what was predicted by sampling error alone (i.e., $Q > df$; $p < .05$), a mixed-weight regression analyses was conducted to estimate the moderating influence of participant and intervention variables, as well as method characteristics on comprehension outcomes (Raudenbush & Bryk, 2002).

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APPENDICES

APPENDIX A

Design Variables of Studies Included in Math Meta-analysis[a]

#	Study	Randomized Controlled Trials (RCT) or Quasi-Experimental Design (QED)	Unit of Assignment	Unit of Analysis	Misalignment between of Unit of Analysis and Unit of Assignment[b]
1	Allinder, R. M., Bolling, R.M., Oats, R.G., & Gagnon, W. A. (2000).	RCT[c]	Teachers	Students	Yes
2	Bahr, C. M. & Rieth, H. J. (1991).	RCT	Students	Students	---
3	Baker, D. E. (1992).	RCT	Students[d]	Students	
4	Bar-Eli, N. & Raviv, A. (1982).	RCT	Students	Students	---
5	Beirne-Smith, M. (1991).	RCT	Students & Teachers	Students & Teachers	---
6	Bottge, B. A., Heinrichs, M., Mehta, Z. D., & Hung, Y. (2002).	RCT	Students	Students	---
7	Butler, F. M., Miller, S. P., Crehan, K., Babbitt, B., & Pierce T. (2003).	RCT	Classes	Students	Yes
8	Calhoon, M. B. & Fuchs, L. S. (2003).	RCT	Classes	Students	Yes
9	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Appleton, A. C. (2002).	RCT	Students	Students	---
10	Fuchs, L. S., Fuchs, D., Hamlett, C. L., Phillips, N. B., & Bentz, J. (1994).	RCT	Teachers	Students yoked to Teachers	---
11	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1990).	RCT	Teachers	Teachers	---
12	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1991).	RCT	Teachers	Teachers	---
13	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Whinnery, K. (1991).	RCT	Students	Students	---
14	Fuchs, L. S., Fuchs, D., Karns, K., Hamlett, C. L., Katzaroff, M., & Dutka, S. (1997).	RCT	Classes	Classes	---
15	Fuchs, L. S., Fuchs, D., Phillips, N. B., Hamlett, C. L., & Karns, K. (1995).	RCT	Teachers	Classes	---

#	Study	Randomized Controlled Trials (RCT) or Quasi-Experimental Design (QED)	Unit of Assignment	Unit of Analysis	Misalignment between of Unit of Analysis and Unit of Assignment[b]
16	Fuchs, L. S., Fuchs, D., & Prentice, K. (2004).	RCT	Teachers	Students	Yes
17	Fuchs, D., Roberts, P. H., Fuchs, L. S., & Bowers, J. (1996).	RCT[e]	Teachers & Students	Students	---
18	Hutchinson, N. L. (1993).	RCT	Students	Students	---
19	Jitendra, A. K., Griffin, C. C., McGoey, K., Gardill, M. G., Bhat, P., & Riley, T. (1998).	RCT	Students	Students	---
20	Kelly, B., Gersten, R., & Carnine, D. (1990).	RCT	Students	Students	---
21	Lambert, M. A. (1996).	QED	Classes	Students	Yes
22	Lee, J. W. (1992)	RCT	Classes	Students	Yes
23 & 24	Manalo, E. Bunnell, J., & Stillman, J. (2000)[f].	RCT	Students	Students	---
25	Marzola, E. (1987).	RCT	Schools	Students	Yes
26	Omizo, M. M., Cubberly, W. E., & Cubberly, R. D. (1985).	RCT	Students & Teachers	Students	---
27	Owen, R. L. & Fuchs, L. S. (2002).	RCT	Classes	Students	Yes
28	Pavchinski, P. (1988).	RCT	Teachers	Students	Yes
29	Reisz, J. D. (1984).	RCT	Students	Students	---
30	Ross, P. A. & Braden, J. P. (1991).	RCT	Teachers	Students	Yes
31	Schunk, D. H. & Cox, P. D. (1986).	RCT	Students	Students	---
32	Slavin, R. E., Madden, N. A., & Leavey, M. (1984a).	QED	Schools	Classes & Students	---
33	Slavin, R. E., Madden, N. A., & Leavy, M. (1984b).	RCT[g]	Schools	Students	Yes
34	Tournaki, H. (1993).	QED	Students	Students	---
35	Tournaki, N. (2003).	RCT	Students	Students	---
36	Van Luit, J. E. H. & Naglieri, J. A. (1999).	RCT	Students	Students	---

#	Study	Randomized Controlled Trials (RCT) or Quasi-Experimental Design (QED)	Unit of Assignment	Unit of Analysis	Misalignment between of Unit of Analysis and Unit of Assignment[b]
37	Walker, D. W. & Poteet, J. A. (1989/1990).	RCT	Teachers	Classes & Students	---
38	Wilson, C. L. & Sindelair, P. T. (1991).	RCT	Schools	Students	Yes
39	Witzel, B. S., Mercer, C. D., & Miller, M. D. (2003).	RCT	Teachers	Students	Yes
40	Woodward, J. (2006).	RCT	Students	Students	---
41	Woodward, J., Monroe, K., & Baxter, J. (2001).	QED	Schools	Students	Yes
42	Xin, Y. P., Jitendra, A. K., & Deatline-Buchman, A. (2005).	RCT	Students	Students	---

[a]Total number of research papers = 41; total number of mathematical interventions = 42.

[b]If two units were used either for assignment or for analysis, either could match the unit of assignment/analysis to be a "match."

[c]Random assignment was assumed because (a) participants volunteered and (b) random assignment was used to assign two interventions to treatment group.

[d]For approximately 15% of the students, assignment was at the school level. Most students were randomly assigned individually, but two schools were randomly assigned as a whole.

[e]Teachers were randomly assigned with two exceptions. Two teachers were assigned based on their previous experience with the interventions.

[f]Two mathematical interventions were reported in this research paper.

[g]Randomly assigned; attrition of one school; replacement school chosen purposefully.

APPENDIX B

Posttests, Maintenance Tests and Transfer Tests of Studies Included in the Meta-analysis[1].

#	Authors	Posttests up to 3 weeks	Maintenance after 3 weeks (*N*=6 out of 42 studies)	Transfer Tests (*N*=9 out of 42 studies)
1	Allinder, R. M., Bolling, R., Oats, R., & Gagnon, W. A. (2000).	+		
2	Bahr, C. M. & Rieth, H. J. (1991).	+		
3	Baker, D. E. (1992).	+		
4	Bar-Eli, N. & Raviv, A. (1982).	+		
5	Beirne-Smith, M. (1991).	+		
6	Bottge, B. A., Heinrichs, M., Mehta, Z. D., & Hung, Y. (2002).	+		+
7	Butler, F. M., Miller, S. P., Crehan, K., Babbitt, B., & Pierce T. (2003).	+		
8	Calhoon, M. B. & Fuchs, L. S. (2003).	+		
9	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Appleton, A. C. (2002).	+[3]		+
10	Fuchs, L. S., Fuchs, D., Hamlett, C. L., Phillips, N. B., & Bentz, J. (1994).	+		
11	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1990).	+		
12	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1991).	+		
13	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Whinnery, K. (1991).	+		
14	Fuchs, L. S., Fuchs, D., Karns, K., Hamlett, C. L., Katzaroff, M., & Dutka, S. (1997).	+		
15	Fuchs, L. S., Fuchs, D., Phillips, N. B., Hamlett, C. L., & Karns, K. (1995).	+		+
16	Fuchs, L. S., Fuchs, D., & Prentice, K. (2004).	+		

#	Authors	Posttests up to 3 weeks	Maintenance after 3 weeks (*N*=6 out of 42 studies)	Transfer Tests (*N*=9 out of 42 studies)
17	Fuchs, D., Roberts, P. H., Fuchs, L. S., & Bowers, J. (1996).	+	+	
18	Hutchinson, N. L. (1993).	+		
19	Jitendra, A. K., Griffin, C. C., McGoey, K., Gardill, M. G., Bhat, P., & Riley, T. (1998).	+[4]		
20	Kelly, B., Gersten, R., & Carnine, D. (1990).	+		
21	Lambert, M. A. (1996).	+		
22	Lee, J. W. (1992).	+		
23 & 24	Manalo, E., Bunnell, J., & Stillman, J. (2000)[2]. Study 1	+[5]	+	
	Study 2	+[5]	+	
25	Marzola, E. (1987).	+		
26	Omizo, M. M., Cubberly, W. E., & Cubberly, R. D. (1985).	+		
27	Owen, R. L. & Fuchs, L. S. (2002).	+		
28	Pavchinski, P. (1988).	+[5]		
29	Reisz, J. D. (1984).	+	+	
30	Ross, P. A. & Braden, J. P. (1991).	+5		
31	Schunk, D. H. & Cox, P. D. (1986).	+		
32	Slavin, R. E., Madden, N. A., & Leavey, M. (1984[a]).	+		
33	Slavin, R. E., Madden, N. A., & Leavy, M. (1984[b]).	+		
34	Tournaki, H. (1993).	+		+
35	Tournaki, H. (2003).	+		+
36	Van Luit, J. E. H. & Naglieri, J. A. (1999).	+	+	+
37	Walker, D. W. & Poteet, J. A. (1989/1990).	+		+
38	Wilson, C. L. & Sindelar, P. T. (1991).	+[5]		
39	Witzel, B., Mercer, C. D., & Miller, M. D. (2003).	+[5]		
40	Woodward, J. (2006).	+[5]		+
41	Woodward, J., Monroe, K., & Baxter, J. (2001).	+		

#	Authors	Posttests up to 3 weeks	Maintenance after 3 weeks (*N*=6 out of 42 studies)	Transfer Tests (*N*=9 out of 42 studies)
42	Xin, Y. P., Jitendra, A. K., & Deatline-Buchman, A. (2005).	+5	+	+

[1]Total number of research papers = 41; total number of mathematical interventions = 42.

[2]Two mathematical interventions were reported in this research paper.

[3]Took a mean of posttest and near transfer test.

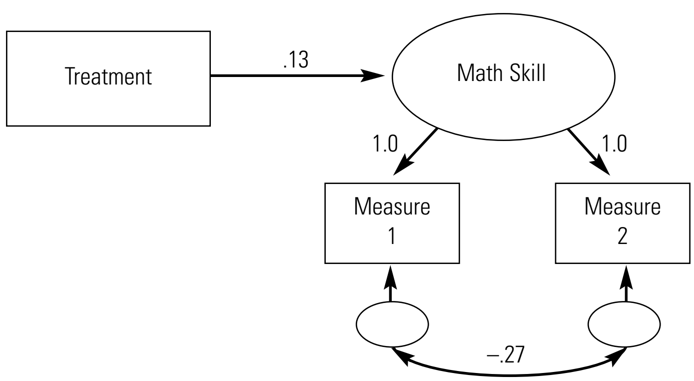
[4]Took a mean of the posttest, maintenance test (which was given within 3 weeks of the end of the intervention), and near transfer test.

[5]Took a mean of posttest and maintenance test (which was given within 3 weeks of the end of the intervention).

APPENDIX C

Worked Example

A worked example is possible in cases where the summary statistics and correlations of treatments with measures and measures with each other are known. In the following example raw data were used to estimate summary statistics and correlations of posttest data in a simple treatment-control comparison using two measures of computation. For correlational analysis four assumptions were made for analysis. First, it was assumed that the treatment group was measured exactly. Second, it was assumed that the correlations of each measure of computation with latent computation ability were equivalent. Third, similarly, it was assumed that the residuals of each measure were equivalent and correlated. Fourth, it was assumed that the variance in comprehension and group were explained without reference to other outside factors (e.g., setting). Given these assumptions a latent model was fitted using LISREL 8 software (Joreskog & Sorbom, 2001). This conceptually appropriate model provided good fit to the data in this ($x^2_{2df} = 2.714$; $p = .257$; *RMSEA* = .058) and an estimated path correlation from treatment group to comprehension outcome ($r_{xy} = .133$) which converts to an effect size estimate ($g = .27$) which is comparable to an estimate based solely averaging the effect sizes from the two measures computed separately ($g_{average} = (.1449 + .377)/2 = .261$).



This model illustrates the potential near approximation of the effects estimates when assuming error-free measurement and correlated residuals to what we have done for this analysis in aggregating estimates.

TABLES AND FIGURES

Table 1

Simple Comparisons of All Effects (*g*) and Heterogeneity (*Q*) for Orthogonal Effects and Effects Stratified by Method

Instructional Component	Random Effects (g)						Hetergeneity		
	Random Effects Mean	Median	SE	p(g)	95% CI		Q	Critical Q	p
					Lower	Upper			
Explicit instruction (n=11)	1.22***	1.39	0.23	0	0.78	1.67	41.68***	18.31	0.00
Multiple heuristics (n=4)	1.56***	1.62	0.46	0.00	0.65	2.47	9.10*	7.82	0.03
Verbalizations (n=8)	1.04***	1.42	0.32	0.00	0.42	1.66	53.39***	14.07	0.00
Visuals for teacher and student (n=7)	0.54***	0.67	0.16	0.00	0.23	0.85	9.88	12.59	0.13
Visuals for teacher only (n=5)	0.41*	0.50	0.18	0.02	0.06	0.77	4.33	9.49	0.36
Visuals combined (n=12)	0.47***	0.52	0.12	0.00	0.25	0.70	14.13	19.68	0.23
Range and sequence (n=9)	0.82***	0.54	0.20	0.00	0.42	1.21	19.78**	15.51	0.01
Teacher feedback (n=7)	0.21*	0.19	0.10	0.04	0.01	0.41	0.32	12.59	1.00
Teacher feedback with recommendations (n=3)	0.34~	0.40	0.21	0.10	-0.07	0.74	1.01	5.99	0.60
Teacher feedback *combined* (n=10)	0.23**	0.21	0.09	0.01	0.05	0.41	1.63	16.92	1.00
Student feedback (n=7)	0.23**	0.17	0.09	0.01	0.05	0.40	3.60	12.59	0.73
Student feedback with goals (n=5)	0.13	-0.17	0.30	0.29	-0.15	0.49	12.67**	9.49	0.01
Student feedback *combined* (n=12)	0.21*	0.14	0.10	0.04	0.01	0.40	16.37	19.68	0.13
Cross-age tutoring[a] (n=2)	1.02***	0.95	0.23	0.00	0.57	1.47	0.68	3.48	0.41
Within class peer-assisted learning (n=6)	0.12	0.17	0.11	0.27	-0.09	0.32	2.66	11.07	0.75

Note. SE = standard error; CI = confidence interval; *n* refers to number of effects.
[a]Fewer observed effects (*n*=2) reduces confidence in cross-grade estimates
~*p*< .10 * *p*<.05 ** <.01 *** *p*<.001

Table 2

Explicit Instruction

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Appleton, A. C. (2002).	Problem-solving tutoring vs. basal instruction only	1.78	Word Problems	38	4	RCT	24 sessions of 33 minutes	Researcher	No	Transfer	0.92, 0.95	96-99%	Researcher Developed
Jitendra, A. K., Griffin, C. C., McGoey, K., Gardill, M. G., Bhat, P., & Riley, T. (1998).	Explicit instruction in diagrammatic representations vs. control (basal curriculum)	0.67 [c]	Word Problems	34 [a]	2–5	RCT	19 sessions of 43 minutes	Researcher	Yes	--	0.77, 0.88	Yes	Researcher Developed
Kelly, B., Gersten, R., & Carnine, D. (1990).	Instruction incorporating principles of curriculum design vs. control (basal curriculum)	0.88	Rational Numbers (Fractions)	34 [a]	9–11	RCT	10 sessions of 30 minutes	Teacher; Researcher	Yes	--	0.98	--	Researcher Developed
Lee, J. W. (1992).	Explicit instruction on using a visual cue vs. control (textbook curriculum)	0.86 [b]	Word Problems	33	4-6	RCT	9 sessions of 45 minutes	Researcher	No	--	0.59-0.91	95%	Researcher Developed
Marzola, E. (1987).	Explicit problem-solving instruction with verbalizations vs. feedback only (no systematic instruction)	2.01[b]	Word Problems	60	5-6	RCT	12 sessions of 30 minutes	Teacher	No	--	--	--	Researcher Developed

Table 2 Continued

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Owen, R. L. & Fuchs, L. S. (2002).	Explicit visual strategy instruction vs. control (basal instruction)	1.39	Word Problems	24 [a]	3	RCT	6 sessions of 30 minutes	Researcher	94.9%	--	0.89	99.5%	Researcher Developed
Ross, P. A. & Braden, J. P. (1991).	Explicit strategy instruction with verbalizations vs. control (typical classroom instruction plus token reinforcement)	0.08 [b c]	Computation	94	1–5	RCT	19 sessions of 60 minutes	Teacher	Yes	--	--	--	Researcher Developed & Norm-referenced
Tournaki, H. (1993).	Explicit self-instruction strategy vs. drill and practice	1.74	Computation	42	3–5	QED	8 sessions of 15 minutes	Researcher	No	Transfer	0.93	98%	Researcher Developed
Tournaki, H. (2003).	Explicit minimum addend strategy with verbalizations vs. drill and practice	1.61	Computation	42	3–5	RCT	8 sessions of 15 minutes	Researcher	No	Transfer	0.93	98%	Researcher Developed
Wilson, C. L. & Sindelar, P. T. (1991).	Explicit strategy instructions vs. sequential instruction (simple to more complex problems)	0.91 [c]	Word Problems	62	2–5	RCT	14 sessions of 30 minutes	Researcher	Yes	--	0.85-0.88	--	Researcher Developed
Xin, Y. P., Jitendra, A. K., & Deatline-Buchman, A. (2005).	Explicit schema-based strategy instruction vs. general strategy instruction	2.15 [c]	Word Problems	22 [a]	6–8	RCT	12 sessions of 60 minutes	Researcher	Yes	Maintenance, Transfer	0.88	100%	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate the data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD.

[b] Effect size is based on multiple measures.

[c] Effect size is based on posttests and short-term retention measures given within a 3-week period.

Table 3

Student Verbalization of Their Mathematical Reasoning

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Hutchinson, N. L. (1993).	Cognitive strategy instruction vs. control (regular instruction)	1.24 [a]	Rational Numbers (Fractions)	20	8–10	RCT	60 sessions of 40 minutes	Researcher	No	--	--	--	Researcher Developed & Norm-referenced
Marzola, E. (1987).	Explicit problem-solving instruction with verbalizations vs. feedback only (no systematic instruction)	2.01 [a]	Word Problems	60	5–6	RCT	12 sessions of 30 minutes	Teacher	No	--	--	--	Researcher Developed
Omizo, M. M., Cubberly, W. E., & Cubberly, R. D. (1985).	Modeling by teacher plus student verbalizations vs. modeling by teacher only	1.75	Computation	60	1–3	RCT	3 sessions of 30 minutes	Teacher, Researcher	No	--	--	--	Researcher Developed
Pavchinski, P. (1988).	Self-instruction vs. traditional teacher instruction	0.22 [a b]	Computation	94	1–5	RCT	19 sessions of 60 minutes	Teacher	Yes	Maintenance	--	--	Researcher Developed & Norm-referenced
Ross, P. A. & Braden, J. P. (1991).	Explicit strategy instruction with verbalizations vs. control (typical classroom instruction plus token reinforcement)	0.08 [a c]	Computation	94	1–5	RCT	19 sessions of 60 minutes	Teacher	Yes	--	--	--	Researcher Developed & Norm-referenced

Table 3 Continued

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Schunk, D. H. & Cox, P. D. (1986).	Continuous student verbalizations vs. no student verbalizations	0.07	Computation	90	6–8	RCT	6 sessions of 45 minutes	Researcher	Yes	--	0.87	--	Researcher Developed
Tournaki, H. (1993).	Self-instruction strategy vs. drill and practice	1.74	Computation	42	3–5	QED	8 sessions of 15 minutes	Researcher	Yes	Transfer	0.93	98%	Researcher Developed
Tournaki, H. (2003).	Explicit minimum addend strategy with verbalizations vs drill and practice	1.61	Computation	42	3–5	RCT	8 sessions of 15 minutes	Researcher	No	Transfer	0.93	98%	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate the data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Effect size is based on multiple measures.

[b] Effect size is based on posttests and short-term retention measures given within a 3-week period.

Table 4

Visual Representations: Use by Both Teachers and Students

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Baker, D. E. (1992).	Strategy with drawing vs. strategy without drawing	0.31	Word Problems	46	3–5	RCT	2 sessions of 45 minutes	Researcher	No	--	0.82	--	Researcher Developed
Hutchinson, N. L. (1993).	Cognitive strategy instruction vs. control (regular instruction)	1.24 [b]	Rational Numbers (Fractions)	20	8–10	RCT	60 sessions of 40 minutes	Researcher	No	--	--	--	Researcher Developed & Norm-referenced
Jitendra, A. K., Griffin, C. C., McGoey, K., Gardill, M. G., Bhat, P., & Riley, T. (1998).	Explicit instruction in diagrammatic representations vs. control (basal instruction)	0.67 [c]	Word Problems	34 [a]	2–5	RCT	19 sessions of 43 minutes	Researcher	Yes	--	0.77, 0.88	Yes	Researcher Developed
Lambert, M. A. (1996).	Complex strategy involving visualization vs. control (textbook curriculum strategy)	0.11 [b]	Word Problems	76	9–12	QED	8 sessions of 55 minutes	Teacher	No	--	0.73, 0.83	--	Researcher Developed
Lee, J. W. (1992).	Explicit instruction on using a visual cue vs. control (textbook curriculum)	0.86 [b]	Word Problems	33	4–6	RCT	9 sessions of 45 minutes	Researcher	No	--	0.59-0.91	95%	Researcher Developed

Table 4 Continued

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Owen, R. L. & Fuchs, L. S. (2002).	Explicit visual strategy instruction vs. control (basal instruction)	1.39	Word Problems	24 [a]	3	RCT	6 sessions of 30 minutes	Researcher	Yes	--	0.89	99.5%	Researcher Developed
Walker, D. W. & Poteet, J. A. (1989/1990).	Diagrammatic representations of problems vs. control (basal keyword strategy)	0.31	Word Problems	70	6–8	RCT	17 sessions of 30 minutes	Teacher	Yes	Transfer	0.76-0.91	--	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD.

[b] Effect size is based on multiple measures.

[c] Effect size is based on posttests and short-term retention measures given within a 3-week period.

Table 5

Visual Representations: Use by Teachers Only

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Kelly, B., Gersten, R., & Carnine, D. (1990).	Instruction incorporating principles of curriculum design vs. control (basal curriculum)	0.88	Rational Numbers (Fractions)	34 [a]	9–11	RCT	10 sessions of 30 minutes	Teacher, Researcher	Yes	--	0.98	--	Researcher Developed
Manalo, E., Bunnell, J., & Stillman, J. (2000). Experiment 1	Strategy instruction plus mnemonics vs. strategy instruction only	-0.01 [b c]	Computation	29	8	RCT	10 sessions of 25 minutes	Researcher	No	Maintenance	0.71	--	Researcher Developed
Manalo, E., Bunnell, J., & Stillman, J. (2000). Experiment 2	Strategy instruction plus mnemonics vs. strategy instruction only	-0.29 [b c]	Computation	28	8	RCT	10 sessions of 25 minutes	Researcher	No	Maintenance	0.71	--	Researcher Developed
Witzel, B. S., Mercer, C. D., & Miller, M. D. (2003).	Concrete- representational -abstract sequence of instruction vs. abstract only instruction	0.50 [c]	Algebra	68 [a]	6 –7	RCT	19 sessions of 50 minutes	Teacher	Yes	--	--	--	Researcher Developed
Woodward, J. (2006).	Strategy instruction plus timed practice drills vs timed practice drills	0.54 [b c]	Computation	15	4	RCT	20 sessions of 25 minutes	Teacher	Yes	Transfer	0.90	--	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD.

[b] Effect size is based on multiple measures.

[c] Effect size is based on posttests and short-term retention measures given within a 3-week period.

Table 6

Range and Sequence of Examples

Study	Research Question	Hedge's g (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Beirne-Smith, M. (1991).	Sequential presentation of sets of related math facts vs. random presentation of math facts (both within the context of a peer tutoring study)	0.12	Computation	20	1–5	RCT	4 sessions of 30 minutes	Peer tutors	Yes	--	--	--	Researcher Developed
Butler, F. M., Miller, S. P., Crehan, K., Babbitt, B., & Pierce T. (2003).	Concrete-representational-abstract instructional sequence vs. representational-abstract instructional sequence	0.29 [b]	Computation; Word Problems; Rational Numbers (Fractions)	42 [a]	6–8	RCT	10 sessions of 45 minutes	Researcher	Yes	--	Yes	97%	Researcher Developed & Norm-referenced
Fuchs, L. S., Fuchs, D., & Prentice, K. (2004).	Transfer training + self-regulation vs. control (regular classroom instruction)	1.14 [b]	Word Problems	45	3	RCT	32 sessions of 33 minutes	Teacher, Researcher	Yes	--	0.88-0.97	98%	Researcher Developed
Kelly, B., Gersten, R., & Carnine, D. (1990).	Instruction incorporating principles of curriculum design vs. Control (instruction using basal curriculum)	0.88	Rational Numbers (Fractions)	34 [a]	9–11	RCT	10 sessions of 30 minutes	Teacher, Researcher	Yes	--	0.98	--	Researcher Developed
Owen, R. L. & Fuchs, L. S. (2002).	Explicit visual strategy instruction vs. control (basal instruction)	0.26	Word Problems	24 [a]	3	RCT	6 sessions of 30 minutes	Researcher	Yes	--	0.89	99.5%	Researcher Developed

Table 6 Continued

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Wilson, C. L. & Sindelar, P. T. (1991).	Explicit strategy instruction + sequential instruction (simple to more complex problems) vs. explicit strategy instruction only	1.55 [c]	Word Problems	62	2–5	RCT	14 sessions of 30 minutes	Researcher	Yes	--	0.85-0.88	--	Researcher Developed
Witzel, B.S., Mercer, C. D., & Miller, M. D. (2003).	Concrete-to- representational-to-abstract sequence of instruction vs. abstract only instruction	0.50 [c]	Algebra	68 [a]	6 –7	RCT	19 sessions of 50 minutes	Teacher	Yes	--	--	--	Researcher Developed
Woodward, J. (2006).	Strategy instruction plus timed practice drills vs. timed practice drills	0.54 [b c]	Computation	15	4	RCT	20 sessions of 25 minutes	Teacher	Yes	Transfer	0.90	--	Researcher Developed
Xin, Y. P., Jitendra, A. K., & Deatline-Buchman, A. (2005).	Explicit schema-based strategy instruction vs. general strategy	2.15 [c]	Word Problems	22 [a]	6–8	RCT	12 sessions of 60 minutes	Researcher	Yes	Maintenance, Transfer	0.88	100%	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD.

[b] Effect size is based on multiple measures.

[c] Effect size is based on posttests and short-term retention measures given within a 3-week period.

Table 7

Multiple/Heuristic Strategies

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Hutchinson, N. L. (1993).	Cognitive strategy instruction vs. control (regular instruction)	1.24 [a]	Rational Numbers (Fractions)	20	8–10	RCT	60 sessions of 40 minutes	Researcher	No	--	--	--	Researcher Developed & Norm-referenced
Van Luit, J. E. H. & Naglieri, J. A. (1999).	MASTER program vs. general instruction program	2.45	Computation	42	3–5	RCT	51 sessions of 45 minutes	Teacher	No	Transfer	--	--	Researcher Developed
Woodward, J. (2006).	Strategy instruction plus timed practice drills vs. timed practice drills	0.54 [a b]	Computation	15	4	RCT	20 sessions of 25 minutes	Teacher	Yes	Transfer	0.90	--	Researcher Developed
Woodward, J., Monroe, K., & Baxter, J. (2001).	Classwide instruction in performance tasks + problem solving instruction in as hoc tutoring vs. regular instruction.	2.00	Word Problems	11	4	QED	69 sessions of 30 minutes	Teacher, Staff	No	--	0.85-0.92	93%	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Effect size is based on multiple measures.

[b] Effect size is based on posttests and short-term retention measures given within a 3-week period.

Table 8

Other Instructional and Curricular Variables

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Bottge, B. A., Heinrichs, M., Mehta, Z. D., & Hung, Y. (2002).	Anchored instruction vs. problem solving instruction	.80 0a	Word Problems	8	7	RCT	12 sessions	Teacher	Yes	Maintenance, Transfer	0.73-0.92	98-99%	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD.

Table 9

Providing Teachers with Student Performance Data

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Allinder, R. M., Bolling, R., Oats, R., & Gagnon, W. A. (2000).	CBM vs. Control (No CBM; basal instruction)	0.27	Computation	54	3-5	RCT	36 weeks	Teacher	Yes	_	0.85	99%	Researcher Developed
Calhoon, M. B. & Fuchs, L. S. (2003).	CBM vs. Control (No CBM; basal instruction)	0.17 [b]	Computation; Word Problems	92	9–12	RCT	30 sessions of 30 minutes	Teacher	Yes	--	0.87, 0.92	97.2%, 96.4%	Researcher Developed & Norm-referenced
Fuchs, L. S., Fuchs, D., Hamlett, C. L., Phillips, N. B., & Bentz, J. (1994).	CBM vs. Control (No CBM; regular classroom instruction)	0.19	Computation	40	2-5	RCT	25 weeks	Teacher	Yes	--	0.85	99%	Researcher Developed
Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1990).	CBM vs. Control (No CBM; no systematic performance monitoring)	0.14 [b]	Computation	91 [a]	3-9	RCT	15 weeks	Teacher	Yes	--	0.85	99%	Researcher Developed
Fuchs, L .S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1991).	CBM vs. Control (No CBM-standard monitoring and adjusting teaching)	0.40 [b]	Computation	63	2-8	RCT	20 weeks	Teacher	Yes	--	0.85	99%	Researcher Developed

Table 9 Continued

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Fuchs, L. S., Fuchs, D., Phillips, N. B., Hamlett, C. L., & Karns, K. (1995).	CBM vs. Control (No CBM)	0.17	Computation; Rational Numbers (Fractions)	40	2-4	RCT	25 weeks	Teacher	Yes	Transfer	0.86	99%	Researcher Developed
Fuchs, D., Roberts, P. H., Fuchs, L. S., & Bowers, J. (1996).	CBM vs. Control (No CBM)	0.32	Computation	47	3-7	RCT	36 weeks	Teacher	No	--	0.85	99%	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD. b Effect size is based on multiple measures.

Table 10

Providing Teachers with Student Performance Data Plus Instructional Enhancements (e.g., instructional recommendations)

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Allinder, R. M., Bolling, R., Oats, R.G., & Gagnon, W. A. (2000).	CBM with self-monitor-ing vs. CBM only	0.48	Computation	54	3–5	RCT	36 weeks	Teacher	Yes	--	0.85	99%	Researcher Developed
Fuchs, L. S., Fuchs, D., Hamlett, C. L., Phillips, N. B., & Bentz, J. (1994).	CBM with computer-generated instructional recommendations vs. CBM only	-0.06	Computation	40	2–5	RCT	25 weeks	Teacher	Yes	--	0.85	99%	Researcher Developed
Fuchs, L .S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1991).	Computerized instruc-tional advice vs. CBM only	0.24 [b]	Computation	63	2–8	RCT	20 weeks	Teacher	Yes	--	0.85	99%	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[b] Effect size is based on multiple measures.

Table 11

Providing Students with Mathematics Performance Feedback

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Calhoon, M. B. & Fuchs, L. S. (2003).	CBM + PALS vs. control (basal instruction)	0.17 [b]	Computation; Word Problems	92	9–12	RCT	30 sessions of 30 minutes	Teacher	Yes	--	0.87, 0.92	97.2%, 96.4%	Researcher Developed & Norm-referenced
Fuchs, L. S., Fuchs, D., Hamlett, C. L., Phillips, N. B., & Bentz, J. (1994).	CBM vs. Control (No CBM; regular classroom instruction)	0.19	Computation	40	2–5	RCT	25 weeks	Teacher	Yes	--	0.85	99%	Researcher Developed
Fuchs, L. S., Fuchs, D., Karns, K., Hamlett, C. L., Katzaroff, M., & Dutka, S. (1997).	CBM vs. control (basal instruction)	-0.17	Computation; Word Problems; Rational Numbers (Fractions)	40	2–4	RCT	46 sessions	Teacher	Yes	--	0.88	99%	Researcher Developed
Fuchs, L. S., Fuchs, D., Phillips, N. B., Hamlett, C. L., & Karns, K. (1995).	CBM + PALS vs. control (no systematic student performance monitoring)	0.17	Computation; Rational Numbers (Fractions)	40	2–4	RCT	25 weeks	Teacher	Yes	Transfer	0.86	99%	Researcher Developed

Table 11 Continued

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Schunk, D. H. & Cox, P. D. (1986).	Feedback on effort expended vs. no feedback on effort	0.60	Computation	90	6–8	RCT	6 sessions of 45 minutes	Researcher	Yes	--	0.87	--	Researcher Developed
Slavin, R. E., Madden, N. A., & Leavey, M. (1984a).	Working in a cooperative learning group vs. control (regular instruction)	0.24 [a]	Computation	113	3–5	QED	24 weeks	Teacher	Yes	--	--	--	Norm-referenced
Slavin, R. E., Madden, N. A., & Leavey, M. (1984b).	Working in a cooperative learning group vs. control (regular instruction)	0.07	Computation	117	3–5	RCT	10 weeks	Teacher	No	--	--	--	Norm-referenced

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Effect size is based on multiple measures.

[b] Effect size is based on multiple measures.

Table 12

Providing Students with Mathematics Performance Feedback and Goal Setting Opportunities

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Bahr, C. M. & Rieth, H. J. (1991).	Feedback with goal vs. feedback with no goal	-0.34 [b]	Computation	46 [a]	7–8	RCT	12 sessions of 10 minutes	Computer	No	--	--	--	Researcher Developed & Norm-referenced
Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Whinnery, K. (1991).	Feedback with goal lines superimposed on graphs vs. feedback with graphs without goal lines	-0.19	Computation	36	2–8	RCT	20 weeks	Teacher	Yes	--	--	--	Researcher Developed
Fuchs, L. S., Fuchs, D., Karns, K., Hamlett, C. L., Katzaroff, M., & Dutka, S. (1997).	Feedback with goal setting vs. feedback only	0.07	Computation; Word Problems; Rational	40	2–4	RCT	46 sessions	Teacher	Yes	--	0.88	99%	Researcher Developed
Fuchs, L. S., Fuchs, D., & Prentice, K. (2004).	Transfer training +self-regulation (goal setting) vs. control (regular classroom instruction)	1.14 [b]	Numbers (Fractions) Word Problems	45	3	RCT	32 sessions of 33 minutes	Teacher, Researcher	Yes	--	0.88-0.97	98%	Researcher Developed
Reisz, J. D. (1984).	Feedback with goal setting discussion vs. control (no description)	0.11	Computation; Word Problems	29	7–8	RCT	16 sessions	Researcher	No	Maintenance	0.88, 0.94	--	Norm-referenced

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD.

[b] Effect size is based on multiple measures.

Table 13

Cross-age Tutoring

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Bar-Eli, N. & Raviv, A. (1982).	Cross age peer tutoring vs. no peer tutoring	1.15	Computation; Word Problems	60	2–6	RCT	33 sessions	Peer- tutors	Yes	--	--	--	Norm-referenced
Beirne-Smith, M. (1991).	Cross age peer tutoring vs. no peer tutoring	0.75	Computation	20	1–5	RCT	4 sessions of 30 minutes	Peer tutors	Yes	--	--	--	Researcher Developed

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

Table 14

Within-class Peer-assisted Learning

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Bahr, C. M. & Rieth, H. J. (1991).	Working in pairs with cooperative goals vs. working individually	0.25 [b]	Computation	46 [a]	7–8	RCT	12 sessions of 10 minutes	Computer	No	--	--	--	Researcher Developed & Norm-referenced
Calhoon, M. B. & Fuchs, L. S. (2003).	PALS vs. control (basal instruction)	0.17 [b]	Computation; Word Problems	92	9–12	RCT	30 sessions of 30 minutes	Teacher	Yes	--	0.87, 0.92	97.2%, 96.4%	Researcher Developed & Norm-referenced
Fuchs, L. S., Fuchs, D., Karns, K., Hamlett, C. L., Katzaroff, M., & Dutka, S. (1997).	Peer tutoring vs. control (basal instruction)	-0.17	Computation; Word Problems; Rational Numbers (Fractions)	40	2–4	RCT	44 sessions of 35 minutes	Teacher	Yes	--	0.88	99%	Researcher Developed
Fuchs, L. S., Fuchs, D., Phillips, N. B., Hamlett, C. L., & Karns, K. (1995).	PALS vs. control (standard procedures)	0.17	Computation; Rational Numbers (Fractions)	40	2–4	RCT	46 sessions of 27 minutes	Teacher	Yes	Transfer	0.86	99%	Researcher Developed

Table 14 Continued

Study	Research Question	Hedge's *g* (Random-Weight Effect)	Intervention										
			Math Domain	Student *N*	Grade	Design	Length	Instruction Provided by	Fidelity	Maintenance or Transfer Assessed	Reliability Pre-Post Measures	Interscorer Agreement	Type of Dependent Measures
Slavin, R. E., Madden, N. A., & Leavey, M. (1984[a]).	Working in a cooperative learning group vs. control(regular instruction)	0.24 [b]	Computation	113	3–5	QED	24 weeks	Teacher	Yes	--	--	--	Norm-referenced
Slavin, R. E., Madden, N. A., & Leavy, M. (1984[b]).	Working in a cooperative learning group vs. working individually	-0.27	Computation	117	3–5	RCT	10 weeks	Teacher	No	--	--	--	Norm-referenced

Note. Dashes for Maintenance or Transfer Assessed indicate that data were not obtained or did not meet our criteria for maintenance and transfer measure. Dashes for Reliability Pre-Post Measures and Interscorer Agreement indicate data were not reported. RCT = randomized controlled trial; QED = quasi-experimental design.

[a] Sample in this study was primarily LD, but not 100% LD.

[b] Effect size is based on multiple measures.

Table 15

List of Mathematical Interventions Used in the Meta-analysis[1]

#	Authors	Approaches to Instruction and/or Curriculum Design *N*=28 studies	Feedback to Teachers *N*=7 studies	Feedback to Students *N*=11 studies	Peer–assisted Mathe-matics Instruction *N*=8 studies
1	Allinder, R. M., Bolling, R., Oats, R., & Gagnon, W. A. (2000). Effects of teacher self-monitoring on implementation of curriculum-based measurement and mathematics computation achievement of students with disabilities. *Remedial and Special Education, 21,* 219-226.		+		
2	Bahr, C. M. & Rieth, H. J. (1991). Effects of cooperative, competitive, and individualistic goals on students achievement using computer-based drill-and-practice. *Journal of Special Education Technology, 11,* 33-48.			+	+
3	Baker, D. E. (1992). The effect of self-generated drawings on the ability of students with learning disabilities to solve mathematical word problems. Unpublished doctoral dissertation, Texas Tech University.	+			
4	Bar-Eli, N. & Raviv, A. (1982). Underachievers as tutors. *Journal of Educational Research, 75,* 139-143.				+
5	Beirne-Smith, M. (1991). Peer tutoring in arithmetic for children with learning disabilities. *Exceptional Children, 57,* 330-337.	+			+
6	Bottge, B. A., Heinrichs, M., Mehta, Z. D., & Hung, Y. (2002). Weighing the benefits of anchored math instruction for students with disabilities in general education classes. *The Journal of Special Education, 35,* 186-200.	+			
7	Butler, F. M., Miller, S. P., Crehan, K., Babbitt, B., & Pierce, T. (2003). Fraction instruction for students with mathematics disabilities: Comparing two teaching sequences. *Learning Disabilities Research and Practice, 18,* 99-111.	+			

Table 15 Continued

#	Authors	Approaches to Instruction and/or Curriculum Design *N*=28 studies	Feedback to Teachers *N*=7 studies	Feedback to Students *N*=11 studies	Peer–assisted Mathe-matics Instruction *N*=8 studies
8	Calhoon, M. B. & Fuchs, L. S. (2003). The effects of peer-assisted learning strategies and curriculum-based measurement on the mathematics performance of secondary students with disabilities. *Remedial and Special Education, 24*, 235-245.		+	+	+
9	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Appleton, A. C. (2002). Explicitly teaching for transfer: Effects on the mathematical problem-solving performance of students with mathematics disabilities. *Learning Disabilities Research and Practice, 17,* 90-106.	+			
10	Fuchs, L. S., Fuchs, D., Hamlett, C. L., Phillips, N. B., & Bentz, J. (1994). Classwide curriculum-based measurement: Helping general educators meet the challenge of student diversity. *Exceptional Children, 60,* 518-537.		+	+	
11	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1990). The role of skills analysis in curriculum-based measurement in math. *School Psychology Review, 19,* 6-22.		+		
12	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Stecker, P. M. (1991). Effects of curriculum-based measurement and consultation on teacher planning and student achievement in mathematics operations. *American Educational Research Journal, 28,* 617-641.		+		
13	Fuchs, L. S., Fuchs, D., Hamlett, C. L., & Whinnery, K. (1991). Effects of goal line feedback on level, slope, and stability of performance within curriculum-based measurement. *Learning Disabilities Research and Practice, 6,* 66-74.			+	
14	Fuchs, L. S., Fuchs, D., Karns, K., Hamlett, C. L., Katzaroff, M., & Dutka, S. (1997). Effects of task-focused goals on low-achieving students with and without learning disabilities. *American Educational Research Journal, 34,* 513-543.			+	+

Table 15 Continued

#	Authors	Approaches to Instruction and/or Curriculum Design *N*=28 studies	Feedback to Teachers *N*=7 studies	Feedback to Students *N*=11 studies	Peer–assisted Mathe-matics Instruction *N*=8 studies
15	Fuchs, L. S., Fuchs, D., Phillips, N. B., Hamlett, C. L., & Karns, K. (1995). Acquisition and transfer effects of classwide peer-assisted learning strategies in mathematics for students with varying learning histories. *School Psychology Review, 24,* 604-620.		+	+	+
16	Fuchs, L. S., Fuchs, D., & Prentice, K. (2004). Responsiveness to mathematical problem-solving instruction: Comparing students at risk of mathematics disability with and without risk of reading disability. *Journal of Learning Disabilities, 37*, 293-306.	+		+	
17	Fuchs, D., Roberts, P. H., Fuchs, L. S., & Bowers, J. (1996). Reintegrating students with learning disabilities into the mainstream: A two-year study. *Learning Disabilities Research and Practice, 11,* 214-229.		+		
18	Hutchinson, N. L. (1993). Effects of cognitive strategy instruction on algebra problem solving of adolescents with learning disabilities. *Learning Disability Quarterly, 16,* 34-63.	+			
19	Jitendra, A. K., Griffin, C. C., McGoey, K., Gardill, M. G., Bhat, P., & Riley, T. (1998). Effects of mathematical word problem solving by students at risk or with mild disabilities. *The Journal of Educational Research, 91,* 345-355.	+			
20	Kelly, B., Gersten, R., & Carnine, D. (1990). Student error patterns as a function of curriculum design: Teaching fractions to remedial high school students with learning disabilities. *Journal of Learning Disabilities, 23,* 23-29.	+			
21	Lambert, M. A. (1996). *Teaching students with learning disabilities to solve word-problems: A comparison of a cognitive strategy and a traditional textbook method*. Unpublished doctoral dissertation, Florida Atlantic University.	+			

Table 15 Continued

#	Authors	Approaches to Instruction and/or Curriculum Design *N*=28 studies	Feedback to Teachers *N*=7 studies	Feedback to Students *N*=11 studies	Peer–assisted Mathe-matics Instruction *N*=8 studies
22	Lee, J. W. (1992). *The effectiveness of a novel direct instructional approach on math word problem solving skills of elementary students with learning disabilities.* Unpublished doctoral dissertation, Ohio State University.	+			
23 & 24	Manalo, E., Bunnell, J., & Stillman, J. (2000)[2]. The use of process mnemonics in teaching students with mathematics learning disabilities. *Learning Disability Quarterly, 23,* 137-156.	+ +			
25	Marzola, E. (1987). *An arithmetic problem solving model based on a plan for steps to solution, mastery learning, and calculator use in a resource room setting for learning disabled students.* New York: Teachers College Press.	+			
26	Omizo, M. M., Cubberly, W. E., & Cubberly, R. D. (1985). Modeling techniques, perceptions of self-efficacy, and arithmetic achievement among learning disabled children. *The Exceptional Child, 32,* 99-105.	+			
27	Owen, R. L. & Fuchs, L. S. (2002). Mathematical problem-solving strategy instruction for third-grade students with learning disabilities. *Remedial and Special Education, 23,* 268-278.	+			
28	Pavchinski, P. (1988). *The effects of operant procedures and cognitive behavior modification on learning disabled students' math skills.* Unpublished doctoral dissertation, University of Florida.	+			
29	Reisz, J. D. (1984). *The effect of goal setting activities on the locus of control and achievement of learning disabled middle-school students.* Unpublished doctoral dissertation, University of Alabama.			+	
30	Ross, P. A. & Braden, J. P. (1991). The effects of token reinforcement versus cognitive behavior modification on learning-disabled students' math skills. *Psychology in the Schools, 28,* 247-256.	+			

Table 15 Continued

#	Authors	Approaches to Instruction and/or Curriculum Design *N*=28 studies	Feedback to Teachers *N*=7 studies	Feedback to Students *N*=11 studies	Peer–assisted Mathe-matics Instruction *N*=8 studies
31	Schunk, D. H. & Cox, P. D. (1986). Strategy training and attributional feedback with learning disabled students. *Journal of Educational Psychology, 78,* 201-209.	+		+	
32	Slavin, R. E., Madden, N. A., & Leavey, M. (1984a). Effects of team assisted individualization on the mathematics achievement of academically handicapped and non-handicapped students. *Journal of Educational Psychology, 76,* 813-819.			+	+
33	Slavin, R. E., Madden, N. A., & Leavey, M. (1984b). Effects of cooperative learning and individualized instruction on mainstreamed students. *Exceptional Children, 50,* 434-443.			+	+
34	Tournaki, H. (1993). *Comparison of two methods of teaching addition to learning disabled and regular education students.* Unpublished doctoral dissertation, New York University.	+			
35	Tournaki, N. (2003). The differential effects of teaching addition through strategy instruction versus drill and practice to students with and without disabilities. *Journal of Learning Disabilities, 36* , 449-458.	+			
36	Van Luit, J. E. H. & Naglieri, J. A. (1999). Effectiveness of the MASTER program for teaching special children multiplication and division. *Journal of Learning Disabilities, 32,* 98-107.	+			
37	Walker, D. W. & Poteet, J. A. (1989/1990). A comparison of two methods of teaching mathematics story problem-solving with learning disabled students. *National Forum of Special Education Journal, 1,* 44-51.	+			
38	Wilson, C. L. & Sindelar, P. T. (1991). Direct instruction in math word problems: Students with learning disabilities. *Exceptional Children, 57* , 512-518.	+			

Table 15 Continued

#	Authors	Approaches to Instruction and/or Curriculum Design *N*=28 studies	Feedback to Teachers *N*=7 studies	Feedback to Students *N*=11 studies	Peer–assisted Mathe-matics Instruction *N*=8 studies
39	Witzel, B., Mercer, C. D., & Miller, M. D. (2003). Teaching algebra to students with learning difficulties: An investigation of an explicit instruction model. *Learning Disabilities Research and Practice, 18,* 121-131.	+			
40	Woodward, J. (2006). Developing automaticity in multiplication facts: Integrating strategy instruction with timed practice drills. *Learning Disability Quarterly, 29,* 269-289.	+			
41	Woodward, J., Monroe, K., & Baxter, J. (2001). Enhancing student achievement on performance assessments in mathematics. *Learning Disability Quarterly, 24,* 33-46.	+			
42	Xin, Y. P., Jitendra, A. K., & Deatline-Buchman, A. (2005). Effects of mathematical word problem-solving instruction on middle school students with learning problems. *The Journal of Special Education, 39*, 181-192.	+			

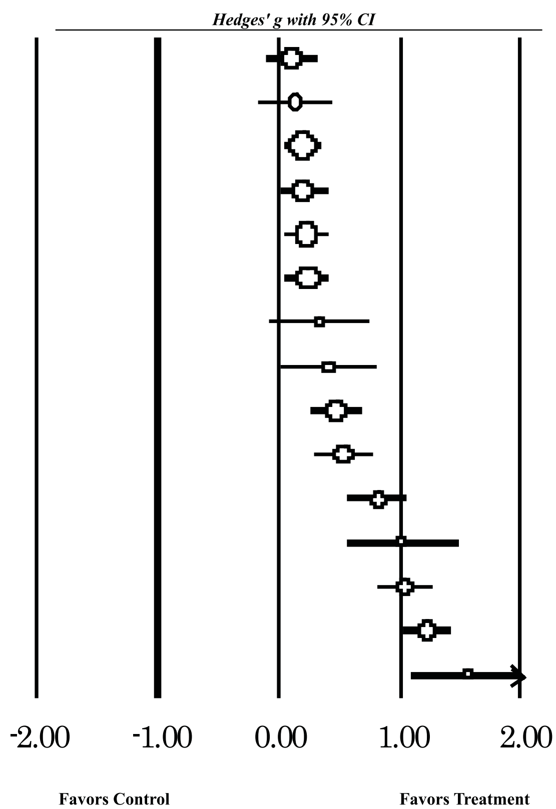
[1]Total number of research papers = 41; total number of mathematical interventions = 42.

[2]Two mathematical interventions were reported in this research paper.

Figure 1

Forest Plot of Random-weighted Treatment Effects (*g*) Collapsed Within Each Treatment Category

Treatment	*N* Treatment	*N* Control
Within class peer-assisted learning (n=6)	181	183
Student feedback with goals (n=5)	85	79
Student feedback combined (n=12)	370	306
Teacher feedback (n=7)	258	169
Student feedback (n=7)	285	227
Teacher feedback combined (n=10)	307	217
Teacher feedback with recommendations (n=3)	49	48
Visuals for teacher only (n=5)	49	48
Visuals combined (n=12)	201	192
Visuals for teacher and student (n=7)	152	144
Range and sequence (n=9)	147	152
Cross-age tutoring (n=2)	50	40
Verbalizing (n=8)	184	205
Explicit Instruction (n=11)	219	217
Multiple heuristics (n=4)	47	41



Note: Effects for individual studies were represented once within any treatment category, but may be represented in more than one treatment category. Symbols are sized uniformly with standard error proportionate to student sample size across treatment categories.

Figure 2

Meta-analysis Coding Categories

Meta-analysis Coding Categories

I. Approaches to instruction and/or curriculum design (*N*=45)

1.Explicit Instruction (*N*=11)

2.Student Verbalization of their Mathematical Reasoning (*N*=8)

3.Visual Representations (*N*=12)

4.Range and Sequence of Examples (*N*=9)

5.Multiple/Heuristic Strategies (*N*=4)

6.Other Instructional and Curricular Variables (*N*=1)

II. Providing ongoing data and feedback to teachers on students' mathematics performance: The role of formative assessment (*N*=10)

1.Providing feedback (on student progress) only (*N*=7)

2.Providing feedback plus instructional enhancements (skills analysis, instructional recommendations) (*N*=3)

III. Providing data and feedback to students with LD on their mathematics performance (*N*=12)

1.Providing feedback only (*N*=7)

2.Providing feedback tied to a performance goal (*N*=5)

IV. Peer Tutoring (*N*=8)

1.Cross-age Tutoring (*N*=2)

2.Within Class Peer-Assisted Learning (*N*=6)

Note: Numbers in parenthesis indicate number of comparisons coded under that category.

Figure 3

Nouri and Greenberg Formula (In Cortina & Nouri, 2000)

Given: Group1: $Mean_1$ sd_1 N_1 Group2: $Mean_2$ sd_2 N_2

Compute Grand Mean (this becomes the mean of the combined off-factor groups):

$$\bar{X}.. = \frac{\left(\overline{X}_1 \times N_1 + \overline{X}_2 \times N_2\right)}{\left(N_1 + N_2\right)}$$

Compute between groups sums of squares:

$$SS_{between} = \sum_{j=1}^{j}\left(\overline{X}.. - \overline{X}._j\right)^2$$

$$SS_{within} = s_1^2 \times \left(N_1 - 1\right) + s_2^2 \times \left(N_2 - 1\right) ... + s_j^2 \times \left(N_j - 1\right)$$

N is combined in the typical case:

$$N_{combined} = N_1 + N_2 ... + N_j$$

Finally, compute new estimated standard deviation for combined off-factor groups:

$$s_{combined} = \sqrt{\frac{(SS_{between} + SS_{within})}{N_{combined} - 1}}$$

In the current study certain off-factors (i.e., subtests and brief-delay posttests) were aggregated with modification to the above procedure without consideration of probable correlations as recommended by some (e.g., Rosenthal & Rubin, 1986). We were unwilling to estimate the intraclass correlations needed and similarly unwilling to ignore the variance by computing effect estimates and simply averaging them. As a compromise, these off-factors were computed with a slight modification:

$$s_{combined} = \sqrt{\frac{(SS_{between} + SS_{within})}{N_{combined} - 2}}$$

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