Fadeout or Forgetting? A Brief Booster Lesson to Support the Maintenance of First-Grade Mathematics Intervention Effects

By

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CHAPTER 1

INTRODUCTION

Below average mathematical ability at the beginning of school is associated with an increased risk of poorly developed mathematical competencies at the end of school, above and beyond the influence of family background, social-emotional functioning, intelligence, and reading ability (Duncan et al., 2007; Every Child a Chance Trust, 2009; Geary, 2011). Poorly developed mathematical competencies are associated with lower rates of full-time employment, higher rates of unemployment, and limited financial security in adulthood (Geary, 2011; Ritchie & Bates, 2013).

Given the long-term consequences of low mathematical skills, a pressing need exists to identify children at risk of mathematics disability and provide early intensive interventions designed to ameliorate deficits before they become lifelong struggles. These intensive interventions often result in significant and substantial effects for intervention over control group students. Yet, follow-up studies reveal that effects diminish, or fade out, over time (e.g., Bailey et al., 2020; Clarke et al., 2016; Hallstedt et al., 2018; Smith et al., 2013).

In the present study, we assessed third-grade outcomes of students who had been randomly assigned to a control group or to participate in a first-grade mathematics intervention. Effects were strong at the end of first grade; however, in light of the fadeout literature on early mathematics intervention, we anticipated fadeout effects two years later. The purpose of the present study was to investigate whether a brief booster lesson, designed to reactivate student knowledge on one key component of the first-grade intervention, mitigates commonly observed fadeout effects. In other words, we explored whether intervention effects persist but reside in a deactivated state. The intervention component focused on calculation strategies for deriving answers to simple addition and subtraction problems.

In this introduction, we discuss potential explanations for fadeout and summarize prior research on the fadeout of mathematics intervention in the early elementary grades. We then discuss factors believed to support the persistence of intervention effects and outline how difficulty in transferring learned skills may lead to deactivation of those skills. Finally, we state the study's research question and outline the study's structure.

Causes of Fadeout

Too often, academic interventions demonstrating significant positive impacts at the end of the intervention fail to reveal significant effects at follow-up. The term *fadeout* is often used to refer to the pattern of diminishing academic effects following a generally effective intervention. The fadeout of intervention effects has not been a major focus in math intervention research, and the reasons for diminishing intervention effects are not well understood (Bailey et al., 2018, 2020).

One explanation for fadeout is catch-up. That is, children who received intervention do not experience a net skill loss; rather, the control group catches up in the posttreatment period (Clements et al., 2013). Such a phenomenon may occur when school instruction is repetitive for students who received intervention, but novel for students in the control condition.

A second explanation for fadeout involves the loss of skill within the intervention group. This may occur in the absence of sustained instructional support. That is, after intervention ends, students stop accessing and practicing the concepts and procedures they learned, and the strategies and knowledge they learned become deactivated (Bailey et al., 2018, 2020). This is of particular concern within the realm of mathematics due to the periodic shifts to novel topics within the mathematics curriculum (e.g., shifting from additive to multiplicative concepts over time). If intervention students are not provided support during these shifts, focused on applying the skills and strategies they mastered during intervention in the context of more complex skills, students will likely stop using the intervention strategies they previously learned. This may result in a performance decrease on the skills they learned during intervention.

A third explanation for fadeout is the cyclical nature with which schools select and implement intervention instruction for struggling students (Bailey et al., 2018). Typically, students are selected to receive intervention instruction based on poor mathematics performance. When students successfully complete an effective intervention, they are unlikely to receive continued school-based intervention, whereas control group students are identified for school-provided intervention. Thus, study control group students receive subsequent intervention while study intervention students do not. Control group receipt of an intervention proximal to the measurement of fadeout effects may explain fadeout.

Prior Work Examining Persistence of Mathematics Intervention Effects

In a systematic search of the literature, we identified four studies that examined the persistence of intervention effects in elementary students who had previously participated in a mathematics intervention (Bailey et al., 2020; Clarke et al., 2016; Hallstedt et al., 2018; Smith et al., 2013). Table 1 outlines each study, including the students' grade level and the intervention's instructional focus and duration. Table 2 presents the timeline for follow up testing, the measures used within each study, effect sizes between intervention and control groups immediately following intervention, and effect sizes at each follow-up occasion. Despite the follow-up timeframe for assessment, ranging from six months to two years across studies, diminishing intervention effects were found in each study.

Table 1

Prior Studies of Persistence of Mathematics Intervention Effects

Author	Grade	*N*	Intervention Focus	Weeks	Hours	Group Size	Adaptive Instruction
Bailey et al. (2020)	1st	639	Explicit instruction on the conceptual and procedural bases for first-grade arithmetic. Emphases were numeral identification, quantities, number relations, arithmetic principles, number families, and decomposition of sets.	16	24	1:1	No
Clarke et al. (2016)	KG	290	Focus on developing procedural fluency with and conceptual understanding of whole-number concepts and skills. Emphases on: (a) counting and cardinality; and (b) operations and algebraic thinking.	10	17	2:1 5:1	No
Hallstedt et al. (2018)	2nd	283	Tablet-based intervention with a focus on increasing fluency on basic arithmetic (addition and subtraction facts to 12), number knowledge, and word problems.	20	24	1:1	Yes
Smith et al. (2013)	1st	775	Building aspects of early number knowledge, including strategies for solving early number tasks, counting, backward counting, numeral identification, base ten arithmetic strategies, and number decomposition.	12	24-30	1:1	Yes

Note. KG = kindergarten.

Table 2

Prior Studies of Persistence on Mathematics Intervention Summary of Outcomes

Author	Follow Up Timeline Post-Intervention	Measures	Immediate Effects	Follow Up Time 1	Follow Up Time 2
Bailey et al. (2020)	12 months	(P) Facts Correctly Retrieved	*d* = 0.24* – 0.42*	*d* = 0.09 – 0.16	*d* = –0.01 – 0.04
	24 months	(D) Number Sets	*d* = 0.20* – 0.33*	*d* = 0.03 – 0.16	*d* = 0.12 – 0.12
		(D) WRAT Arithmetic	*d* = 0.29* – 0.30*	*d* = 0.01 – 0.08	*d* = 0.03 – 0.08
		(D) Number Line Estimation	*d* = 0.06 – 0.14	*d* = 0.11 – 0.12	*d* = –0.02 – 0.02
		(D) KeyMath – Numeration	*d* = 0.06 – 0.14*	*d* = 0.00 – 0.00	*d* = 0.08 – 0.08
Clarke et al. (2016)	6 months	(D) SESAT / SAT-10	*d* = 0.18	*d* = –0.02	N/A
Hallstedt et al. (2018)	6 months	(P) Addition 0-12	*d* = 0.67*	*d* = 0.18	*d* = 0.03
	12 months	(P) Subtraction 0-12	*d* = 0.53*	*d* = 0.28*	*d* = 0.13
		(P) Addition 0-18	*d* = 0.13	*d* = –0.11	*d* = 0.02
		(P) Subtraction 0-18	*d* = 0.50*	*d* = 0.04	*d* = 0.07
Smith et al. (2013)	12 months	(P) Math Recovery Proximal	*d* = 0.29*	*d* = -0.02	N/A
		(D) WJIII Math Fluency	*d* = 0.15*	*d* = 0.09	
		(D) WJIII Applied Problems	*d* = 0.28*	*d* = 0.00	
		(D) WJIII Quantitative Concepts	*d* = 0.24	*d* = 0.06	

Note. All effects are reported as Cohen's *d*. * = significant effect. (P) = proximal; (D) = distal; WRAT = Wide Range Achievement Test; SESAT = Stanford Early Achievement Test; SAT-10 = Stanford Achievement Test – Tenth Edition; WJIII = Woodcock Johnson III Achievement Test.

Clarke et al. (2016) assessed the longitudinal effects of ROOTS, an intervention designed to develop procedural fluency and conceptual understanding of whole number concepts. After 10 weeks of small-group intervention, significant effects favored students who received intervention over control on proximal measures of early numeracy skills (ES = 0.16 to 0.75) as well as distal, standardized measures of problem solving and procedures (ES = 0.18). By January of first grade, however, there were no significant differences between conditions on the Stanford Achievement Test (SAT-10; Harcourt Educational Measurement, 2002), a distal measure of mathematics ability.

Hallstedt and colleagues (2018) examined the immediate effects of a second-grade mathematics intervention as well as the follow-up effects at six- and 12-months post-intervention. The adaptive intervention program, which was administered entirely on a tablet, focused primarily on addition and subtraction fact fluency with a secondary focus on problem solving. After 20 weeks of intervention, significant effect sizes (0.50 to 0.67) favored intervention over control on proximal measures of addition and subtraction fact fluency, also administered using a tablet. Six months post-intervention, effect sizes on these same measures had diminished (–0.11 to 0.28). At 12 months post-intervention, effect sizes were further reduced (0.02 to 0.13) and were no longer significant.

Smith et al. (2013) used both proximal and distal measures when evaluating Math Recovery, a one-to-one intervention designed to improve the outcomes of first-grade students who are struggling in mathematics with a 12-week intervention. Math Recovery divides instructional time among six aspects of early number knowledge that are believed to support arithmetic: strategies for solving early number tasks, forward number word sense, backward number word sense, numeral identification, base ten arithmetic strategies, and combining and decomposition strategies. Immediately following intervention, significant effect sizes (0.15 to 0.30) favored the Math Recovery over control on arithmetic, concepts and applications, quantitative concepts, and math reasoning measured using various subtests from the Woodcock-Johnson III Achievement Test (Woodcock et al., 2001). However, by the end of second grade, differences between conditions were no longer significant, with effect sizes ranging from –0.02 to 0.09 on these same measures.

In the most recent and comprehensive study, Bailey et al. (2020) used proximal and distal measures to examine the persistence of intervention effects one-year post-intervention and again two-years post-intervention. The original first-grade intervention targeted the conceptual and procedural bases that support early arithmetic (i.e., numeral identification, quantities, number families, and decomposition of sets). After 16 weeks of intervention, significant effects favored intervention over control, with effect sizes ranging from 0.20 to 0.90 on measures of arithmetic and numeracy. Yet, by the end of second grade, these effects on some of the same but also more advanced measures had diminished (ES = –0.03 to 0.16) and were no longer significant; by the end of third grade, effects were further reduced (ES = –0.01 to 0.12) on the same follow-up measures administered in second grade.

The diminishing results observed across these studies is sobering. It is critical for students with or at-risk of math disability to maintain the positive effects of intensive interventions. Given the growing body of evidence demonstrating that fadeout is common after successful interventions, it is important to consider factors that may support the persistence of intervention effects.

Factors that Support the Persistence of Intervention Effects

As framed by Bailey et al. (2020), promoting persistence of effects requires intervention to target skills that are: (a) malleable, (b) fundamental for later more complex skills, and (c) unlikely to develop in the absence of intervention. Malleable skills can be developed via explicit instruction (as opposed personality traits, such as conscientiousness; Bailey et al., 2017). Skills that are fundamental for future success equip students to benefit from subsequent classroom instruction. This is important in mathematics, because foundational skills are necessary for success with later curricular units (e.g., intervention consolidates understanding of whole-number knowledge, which is invoked during fractions instruction). Beyond targeting malleable and fundamental skills, interventions should avoid skills that at-risk children likely develop in the absence of intervention.

Bailey et al. (2020) referred to skills that meet these three criteria as *trifecta skills*. An example of a trifecta skill in first-grade mathematics intervention is simple arithmetic. In the early grades, although typically developing children often show rapid development with this skill (Bailey et al., 2016), children at-risk for mathematics disabilities struggle to develop arithmetic competence without intervention (Fuchs et al., 2013, 2021). Instead, they tend to rely on immature counting strategies to solve basic arithmetic problems, which are time-consuming and error-prone strategies. Reliance on such strategies weaken the association between problem stems and correct answers, reducing the ability to rely on math fact retrieval (Geary, 1993). Given its malleability, its fundamental nature for use in a variety of subsequent mathematics topics, and evidence that it is unlikely to develop without explicit instruction (Fuchs et al., 2019; Geary et al., 2007; National Research Council, 2001), arithmetic skill is a strong example of a trifecta skill to target in mathematics interventions for young children at-risk for mathematics disability.

Nevertheless, as demonstrated in four studies assessing persistence of effects, fadeout still occurs when interventions focus on trifecta skills. Each of the prior studies of fadeout (discussed above) used an intervention whose primary aim was to increase arithmetic skill (Bailey et al., 2020; Clarke et al., 2016; Hallstedt et al., 2018) or increase early number knowledge, which is believed to support early arithmetic (Smith et al., 2013). Each study found significant effects favoring intervention over control on proximal measures of arithmetic or early numeracy. Although each study targeted a trifecta skill and found significant positive effects immediately following intervention, fadeout occurred in the following months and years. This pattern indicating fadeout motivated our focus in the present study on another possible explanation, in which the learned knowledge is still present but becomes deactivated because students are unable to transfer this knowledge to novel situations and contexts.

Deactivation Does Not Equal Forgetting

Students can acquire a substantial amount of knowledge during an intensive academic intervention (e.g., Fuchs et al., 2021; Gersten et al., 2020; Kroesbergen & Van Luit, 2003; Stevens et al., 2018). However, there is no guarantee students will remember and continue to use this information over the long term. This may occur because, as evidenced in studies examining summer learning loss (see Cooper et al., 1996, for a meta-analytic review), students rapidly forget information and strategies when they use them infrequently.

Although some may characterize this as simple forgetting, this may not be the case. Basic memory theory (Herrmann, 1996) posits that learned knowledge may be available (stored in memory) but not readily accessible (retrievable; Butler et al., 2020). New retrieval cues, such as visual or verbal prompts to stimulate the original memory, may cue or reactivate a seemingly forgotten memory or skill (Gisquet-Verrier & Riccio, 2012; Tulving & Pearlstone, 1966). Knowledge that is stored in memory but not retrievable has been referred to as *marginal knowledge* (Berger et al., 1999).

Cue-induced reactivation of marginal knowledge can have many benefits. Reactivation of a memory has been shown to induce malleability of the original memory and facilitate the integration of new information (Gisquet-Verrier & Riccio, 2012). When acquiring new knowledge, individuals must reactivate prior knowledge for integration to occur. In studies where participants were re-presented with previously learned material before learning new material, the integration and some of the new material was detected as early as a few minutes after initial reactivation (Gisquet-Verrier & Riccio, 2012; Hupbach et al., 2007). Reactivation also increases the long-term accessibility of the original memory. It does not strengthen the original memory. Rather, it increases the likelihood of accurate retrieval in subsequent prompts (Gisquet-Verrier & Riccio, 2012). Yet, if students are not provided with ample opportunities to reactivate marginal knowledge, it may become deactivated and inaccessible.

It is highly unlikely that a single intensive academic intervention would provide complete inoculation against later academic difficulties for students at risk. Rather, these intensive interventions could be better conceptualized as the first in a series of necessary steps in an ongoing effort to remediate academic difficulty (National Research Council, 1998). This is why explicit instructional support after intervention ends is likely needed to support the persistence of effects. As mentioned, a *sustaining environment* is particularly important in mathematics because of the frequent shift to new topics within the curriculum. A typical first-grade curriculum shifts topics to counting strategies, addition and subtraction strategies, geometry, time, and money, all within the span of a few months. Without explicit instruction, at-risk students struggle to navigate these shifts and are often unable to apply previously learned skills to a new topic (Kroesbergen & Van Luit, 2003). A struggling first-grade student may have difficulty applying the counting strategies learned during a number sense intervention to the novel topic of counting coins or regrouping for double-digit computation without explicit transfer instruction. In addition, specifically, without frequent review of the strategies learned during intervention, students may forget the skills they had previously mastered. In this vein, distributed practice offers potential for improving the retention of learned material.

Does Distributed Practice Make Perfect?

Most people have heard the adage "practice makes perfect." When trying to learn something well, such as arithmetic facts or procedures, a single exposure is usually inadequate for long-term retention. Having subsequent review spaced out over time generally leads to superior learning (Kang, 2016). This phenomenon is called the *spacing effect* (sometimes referred to as the benefit of *distributed practice* or *spaced practice)*. Repeating an item or skill during distributed practice potentially reminds the participant of its previous occurrence and prompts retrieval of the previous presentation from memory (Kang, 2016; Wahlheim et al., 2014).

An extensive literature examines the spaced practice effect in cognitive and educational psychology. Cepeda et al. (2006) analyzed more than 400 studies using distributed practice to increase the retention of learned material. When comparing massed versus spaced practice on the later recall of verbal information, distributed practice resulted in higher verbal recall at all retention intervals. However, a vast majority of these studies focused on the recall of information at very short time periods (i.e., minutes to hours after initial instruction). Of the 400 studies included in the review, only a dozen studies looked at retention one day after the initial instruction. Only six studies looked at retention longer than a week after initial instruction.

One of these studies (Bahrick et al.,1993) assessed retention across the largest interval (56 days) and found promising results when examining the effects of distributed practice during novel foreign language vocabulary instruction with four adult participants. Participants were randomly assigned to receive booster instruction at 14-, 28-, or 56-day intervals for one to five years (to equate total instructional time across conditions). Results showed that intervals of 56 days produced higher foreign vocabulary recall compared to other spacing intervals.

While those results, along with the findings of the larger meta-analysis, suggest distributed practice may be effective for learning new information, the viability of distributed practice for long-term retention (i.e., greater than 2 years) has yet to be explored. Further, a vast majority of the distributed practice literature focuses on the retention of a small amount of material (such as a list of words or facts) learned on one occasion. Multi-week academic interventions that rely on distributed practice to teach skills that are malleable, fundamental, and unlikely to develop in the absence of intervention should produce persistent, long-term effects. However, the available literature examining the persistence of mathematics intervention effects indicates this is not the case (e.g., Bailey et al., 2020; Clarke et al., 2016; Hallstedt et al., 2018; Smith et al., 2013).

What if the fadeout of intervention effects is not a result of students forgetting the learned material? What if the learned material is still there but lying dormant because students have not received sustained instructional support that explicitly teaches them how to transfer the learned knowledge to new skills? On one hand, given the intensive nature of one-on-one mathematics interventions (sometimes receiving up to 25 hours of one-on-one instruction) and their reliance on distributed practice, the literature indicates knowledge maintenance should persist. On the other hand, the nature of transfer difficulty among students with mathematics difficulties (Brownell et al., 1993; Kang et al., 2019; Watts et al., 2017) indicates they may require periodic contextual prompting to strategically utilize that knowledge.

The Role of Transfer in the Persistence of Intervention Effects

The deactivation of knowledge learned during an intervention may occur if students are unable to transfer this knowledge to new tasks and situations and, therefore, do not access and use the knowledge, causing it to become deactivated. The ability to transfer learning to new tasks and in varying contexts is dependent on the extent to which the student can recognize the similarities between the original task and the new task (Haskell, 2001; Kang et al., 2019). When novel learning tasks share many similarities, both perceptually and structurally, with the original learning task, an automatic transfer (known as *near transfer*, *reflexive transfer*, or *low road transfer*) is likely (National Research Council, 1999; Perkins & Salomon, 1992). As novel learning tasks become more dissimilar from the original taught learning task (known as *far transfer*, *mindful transfer*, or *high road transfer*), automatic transfer is less likely (Kang et al., 2019; National Research Council, 1999).

Unfortunately, individuals with learning disabilities struggle to transfer learning to novel tasks and situations (Brownell et al., 1993). These students often fail to identify the salient task features in different situations, struggle to organize and coordinate information in novel problems, and often focus on isolated or irrelevant details. Additionally, the language and instructional strategies used during interventions often diverges from those used in classrooms

(Claessens et al., 2014), making it even more difficult for individuals with learning disabilities to transfer knowledge.

In the months and years following an intervention, students with a learning disability may require explicit instruction on how to transfer their learned knowledge to more complex tasks that rely on the foundational skills learned during intervention, even as they are required to apply those learned skills in different contexts. They may also require instruction on how to recognize salient features in dissimilar tasks that allow for the application of the knowledge learned during intervention. Without this explicit transfer instruction, it is possible students do not recognize novel tasks as an opportunity to apply the knowledge and skills learned in intervention. Not accessing this knowledge for a prolonged period may cause this information to become deactivated marginal knowledge.

Purpose of the Present Study

The present study was designed to increase understanding of the fadeout phenomenon by testing the possibility that intervention effects may persist following intervention but reside in a deactivated state. This deactivation may occur because post-intervention educational environments do not provide periodic contextual prompting that encourages students to use their learned knowledge and do not explicitly teach students how to transfer this knowledge to novel learning tasks.

Our hypothesis, therefore, was that a brief booster lesson that reviewed the skills and strategies taught in the original intervention would reactivate intervention student's marginal knowledge. Such a claim would be demonstrated by stronger post-booster performance for intervention over control students while controlling for the students' pre-booster lesson scores.

Specifically, in the present study, we first administered a follow-up assessment of arithmetic skill to third graders. Two-thirds of the students had received mathematics intervention in first grade; one-third were in the control group. After the follow-up assessment, we provided a brief booster lesson to all students, regardless of condition in the first-grade study. This booster lesson addressed the strategies taught during the first-grade intervention for solving addition and subtraction problems. After the booster lesson, students were re-tested on the same measures.

CHAPTER 2

METHOD

The sample of students for the present study was derived from the final cohort of a larger, field-based randomized controlled trial (referred to as *parent study*; Fuchs et al., 2021) investigating the effects of embedding language comprehension instruction within word-problem intervention designed to increase the word-problem solving ability of first-grade students identified as at-risk (AR) for mathematics disability. Effects on arithmetic skill were also examined.

Background Information on Parent Study

In the parent study, a latent class approach was used to identify children as AR by combining screening scores across the First-Grade Test of Computational Fluency and the First-Grade Test of Mathematics Concepts and Applications (Fuchs et al., 1990) into a latent factor. The two-subtest Wechsler Abbreviated Scales of Intelligence (WASI; Wechsler, 2011) was also administered. Because the parent study's intervention was not designed to address the needs of students with intellectual disability, students scoring below the 10^{th} percentile on both subtests were excluded.

AR students were randomly assigned to four conditions. One was a control group; the other three involved mathematics intervention delivered for 15 weeks in one-to-one format three times per week; each session was 30 min. One condition focused on number knowledge, another on word problems, and the other on word problems with embedded language instruction.

All three conditions taught strategies to solve basic addition (e.g., 3 + 8 = ___) and subtraction (e.g., 9 – 3 = ___) problems. Specifically, students were taught to know the answer right away (i.e., retrieve the answer from memory if confident of the answer) or use the efficient counting strategy that was taught during intervention. In all lessons, students were required to use this strategy to solve arithmetic problems either in the context of or outside of word problems.

The efficient counting procedures are as follows. For addition, students use a counting-in strategy. They open their hand to show the smaller number, say the bigger number aloud, and count the smaller number into their hand, pushing in one finger at a time until all fingers are in the hand. The answer is the last number counted aloud. For subtraction, students close their fist where they "hold" the "minus" number and count up to the larger number while putting up one finger for each number counted. The answer is the number of raised fingers after counting.

To promote quick responding and encourage the use of efficient counting strategies, the parent study provided speeded practice each session in the form of a game. During this game ("Meet or Beat Your Score"), students have 1 min to solve as many math problems as possible. If students do not know the answer right away, they use the efficient counting strategy for the operation. At the end of the allotted time, students graph the number of problems they solved correctly.

At the end of the first-grade intervention, all three intervention conditions demonstrated significantly stronger arithmetic performance over the control group on the First-Grade Mathematics Assessment Battery (Hedges' g 0.59 – 0.79), when controlling for pretest arithmetic scores (Fuchs et al., 2021). The parent study also conducted annual follow-up to investigate maintenance effects in spring of second grade and spring of third grade. Follow-up assessments, which assessed word reading, single-digit addition and subtraction fluency, double-digit addition and subtraction fluency, word problem solving, and fluent number processing, occurred in two sessions on consecutive days.

Participants in the Present Study

For the present analysis, which occurred when students were in third grade, 95 of the 102 final-cohort students who completed posttesting were located. However, due to the COVID-19 pandemic, schools closed before all the located students were tested. As a result, complete data were obtained for 40 students. Of these, 28 students had received intervention as part of the first-grade parent study; the remaining 12 students had served in the parent study's control group. Students in this sample of 40 students were from 25 third-grade classrooms in 10 schools. Frequencies for gender, race, socioeconomic status, special education category, and English learner status for the sample are presented in Table 3.

Table 3

Demographics of Participant Sample (n = 40)

Variable	*n*	%
Sex		
Female	26	65
Male	14	35
Race		
African American	15	38
Asian	2	5
Caucasian	12	30
Hispanic	10	25
Other[a]	1	2
Special Education Diagnosis		
None	37	93
Developmental Delay	1	3
Occupational Therapy	1	3
Speech/Language	1	3
English Language Learner	6	15
Subsidized Lunch	20	50

Note. [a] One student identified as mixed race, African American and Caucasian.

Table 4 shows demographic variables by condition. We conducted chi-square tests of independence to check for relations between first-grade intervention status (i.e., received intervention vs. control) and demographic variables. Yates' continuity corrections were applied to account for possible overestimation when using a 2 x 2 table (Yates, 1934). No significant associations were revealed for sex, $\chi^2(1, n = 40) = 0.884$, $p = .347$; race, $\chi^2(1, n = 40) = 2.063$, $p = .724$; special education status, $\chi^2(1, n = 40) = 5.251$, $p = .154$; English language learner status, $\chi^2(1, n = 40) = 0.458$, $p = .499$; and subsidized lunch status, $\chi^2(1, n = 40) = 1.071$, $p = .301$.

Table 4

Demographics of Participants by Condition

	Intervention (n = 28)		Control (n = 12)		
Variable	n	%	n	%	(df) χ^2
Sex					(1) 0.884
Female	20	71.4	6	50	
Male	8	28.6	6	50	
Race					(4) 2.063
African American	12	42.9	3	25	
Asian	1	3.6	1	8.3	
Caucasian	8	28.6	4	33.3	
Hispanic	6	21.4	4	33.3	
Other	1	3.6	0	0	
Special Education Diagnosis					(3) 5.251
Developmental Delay	0	0	1	8.3	
Occupational Therapy	0	0	1	8.3	
Speech/Language	1	3.6	0	0	
English Language Learner	3	10.7	3	25	(1) 0.458
Subsidized Lunch	16	57.1	4	33.3	(1) 1.071

Note. No Chi square results were significant. Outliers included.

Table 5 presents frequencies for gender, race, socioeconomic status, special education category, and English learner status for the students (n = 40) who were tested before schools closed due to COVID-19 and those who were not (n = 62). Chi-square tests for independence (with Yates Continuity Corrections, when needed) revealed no significant associations between students who were tested prior to COVID-19 closure with respect to sex, $\chi^2(1, n = 102) = 0.426$, $p = .514$; race, $\chi^2(4, n = 102) = 4.601$, $p = .319$; special education category, $\chi^2(6, n = 102) = 5.138$, $p = .526$; and socioeconomic status, $\chi^2(1, n = 102) = 0.000$, $p = 1.0$. There was, however, a significant association between booster receipt and English learner status, $\chi^2(1, n = 102) = 8.642$, $p = .003$. Examination of adjusted residuals indicated English language learners were less likely to have been tested before school closures due to COVID-19.

Table 5

Demographics of Tested and Not Tested Students

	Tested Students (n = 40)		Not Tested Students (n = 62)		
Variable	n	%	n	%	(df) χ^2
Sex					(1) 0.426
Female	26	65	35	56	
Male	14	35	27	44	
Race					(4) 4.601
African American	15	38	18	29	
Asian	2	5	2	3	
Caucasian	12	30	12	19	
Hispanic	10	25	28	45	
Other	1	2	2	3	
Special Education Diagnosis					(6) 0.514
Developmental Delay	1	3	1	3	
Occupational Therapy	1	3	0	0	
Speech/Language	1	3	4	7	
Learning Disability	0	0	2	3	
Learning Disability + Speech	0	0	1	2	
Learning Disability + OT	0	0	1	2	
English Language Learner	6	15	28	45	(1) 8.642*
Subsidized Lunch	20	50	30	48	(1) 0.000

Note. *significance at p = .003

Measures

Screening Measures from Parent Study Relevant to the Present Study

The parent study screened students for study entry to include AR students for subsequent random assignment to four intervention conditions. Screening measures included the following.

The First-Grade Test of Mathematics Concepts and Applications (Fuchs et al., 1990) samples the typical first-grade concepts/applications curriculum (i.e., numeration, concepts, geometry, measurement, applied computation, money, charts/graphs, and word problems) with 25 items. Each item that involves words is read aloud by the testers. For 20 items, students have 15 s to respond; for five items, 30 s. On a previous sample of students who were the same age and of similar demographics, α was .94 (Fuchs et al., 2013).

The two-subtest Wechsler Abbreviated Scale of Intelligence (WASI; Wechsler, 2011) was also administered. WASI-Vocabulary requires students to identify objects in pictures (3 items) and construct definitions for words (remaining items). At ages 6-11, test stability is .85. WASI-Matrix Reasoning measures a student's nonlinguistic reasoning abilities. For each item, students select 1 of 5 items that complete a visual pattern. At ages 6-11, test stability is .79.

Outcome Measure from Parent Study Relevant to the Present Study

The present analysis relied on the parent study's arithmetic measure used in first grade: the four arithmetic subtests from the First-Grade Mathematics Assessment Battery (Fuchs et al., 2003), combined into a single score. Addition comprises 25 addition problems with answers from 0 to 12, presented vertically on one page, and 25 addition problems with answers from 0 to 18, presented vertically on one page. Subtraction comprises 25 subtraction problems with answers from 0 to 12, presented vertically on one page, and 25 subtraction problems with answers from 0 to 18, presented vertically on a second page. Students have 1 min to write answers on each page. In the current study, α was .97.

District Classroom Calculations Instruction in Grades 1-3

The school district in which the parent study was conducted provides teachers with a detailed scope and sequence to guide instruction that aligns with the state's mathematics standards. The scope and sequence is supplemented with lessons from the Go Math curriculum (Houghton Mifflin Harcourt, 2015), which is also based on the state's mathematics standards. In first grade, a primary academic focus is placed on adding and subtracting within 20 using strategies such as counting on, making sets of 10, using fact families, and composing/decomposing numbers to make 10. In second grade, instruction is focused on fluently adding and subtracting with 30 using multiple strategies. By the end of second grade, students are expected to know from memory all sums of two one-digit numbers and the related subtraction facts. In third grade, addition and subtraction instruction is focused on understanding place value to add and subtract within 1,000.

Booster Lesson Administered for the Present Analysis

All students who were tested before schools closed due to the COVID-19 pandemic also received the booster lesson. The goal of the booster lesson was to provide a brief review of the counting strategies provided during the parent study's intervention to assess whether it produced a differential boost for students who received intervention over students in the control condition; that is, whether a booster session may reactivate prior knowledge. The lesson, which was written to mimic the original counting strategy instruction intervention students received during first grade, was designed with sufficient explicitness to remind tutored students of the skills they learned to solve addition and subtraction problems while in first grade, but without enough review and practice to consolidate the arithmetic solution strategies in control group students who were not otherwise taught these strategies.

The booster lesson lasted 10-15 minutes, with longer duration for students with less attentiveness. The booster used instructional posters and language from the parent study's intervention. See Appendix A for the full scripted booster lesson, which was divided into five parts: (1) review of the two ways to solve a math problem; (2) review of the counting-in strategy for addition problems; (3) review of the counting-up strategy for subtraction problems; (4) guided practice; and (5) speeded practice.

The booster lesson began by reviewing the two ways to solve a math problem. Students were shown a poster with visuals to remind them of these two methods. The research assistant (RA) then reviewed steps for counting in to add, modeled how to solve one addition problem using this strategy; students practiced solving one addition problem using the strategy. This same process was followed to review the counting up strategy for subtraction. Next, students solved four problems, two addition and two subtraction, with guidance from the RA as needed. Finally, students played the parent study's speeded practice game (see description above) for 30 s.

Fidelity of Testing and Booster Session Administration

Examiners were trained Ph.D. and master's-student RAs, who demonstrated fidelity (i.e., > 90% accuracy) during a mock assessment and booster lesson administration. All testing sessions and booster lessons were audio recorded. Twenty percent of testing sessions and 30% of booster lesson recordings, stratified by RA, were randomly selected to evaluate the fidelity of testing administration and booster lesson implementation. Three RAs independently listened to tapes while referencing a checklist of essential components. For testing sessions, fidelity was 99.34% (SD = 1.48) across RAs. For booster lessons, fidelity was 100% across RAs. See Appendix B for the full fidelity checklist for the booster lesson.

Procedure

Students completed the First-Grade Mathematics Assessment Battery in the first follow-up test session. In the second follow-up session, the 10-15-minute booster lesson was delivered. Immediately following the booster instruction, the four subtests from the First-Grade Mathematics Assessment Battery were re-administered. All testers were blind to study condition during test administration and scoring. All assessments were double scored for accuracy, with agreement exceeding 99%. All data were double entered into databases for verification.

CHAPTER 3

DATA ANALYSES AND RESULTS

Data Analyses

One-way ANCOVAs were applied to the data. Before running the model, data were checked to ensure all statistical assumptions of the ANCOVA were met. To test our hypothesis that a brief booster lesson might mitigate the commonly observed fadeout effect by reactivating marginal knowledge, we conducted a one-way between-groups ANCOVA. First-grade intervention status was the independent variable (i.e., intervention vs. control); the post-booster session composite arithmetic performance (on the four subtests of First-Grade Mathematics Assessment Battery) was the outcome variable; pre-booster session composite arithmetic performance was the covariate. We also conducted a parallel, supplemental one-way between-groups ANCOVA to assess the extent to which students attempted applied intervention strategies before and after the booster lesson. This was indexed indirectly using a composite percentage correct score across the four subtests of attempted problems answered correctly.

Table 6 displays means and standard deviations on the outcome variable on pre- and post-booster lesson arithmetic performance by condition (i.e., first-grade intervention vs. control). Effect sizes (ESs), reported as Hedges' g, were calculated by subtracting the adjusted means and dividing them by the unadjusted standard deviations (Institute of Education Sciences, 2017).

Table 6

Pre- and Post-Booster Performance on Outcome Measures

	Intervention (*n* = 28)		Control (*n* = 12)		Across Groups (*n* = 40)	
Variable	*M*	*(SD)*	*M*	*(SD)*	*M*	*(SD)*
Raw Score						
Addition, 0-12: Pre	13.21	(4.97)	10.5	(8.2)	12.4	(6.13)
Addition, 0-12: Post	13.75	(6.36)	11.17	(7.78)	12.98	(6.82)
Addition, 0-18: Pre	10.39	(4.06)	10.33	(7.25)	10.38	(5.12)
Addition, 0-18: Post	10.32	(4.68)	9.58	(8.05)	10.10	(5.79)
Subtraction, 0-12: Pre	6.75	(3.12)	7.08	(6.65)	6.85	(4.39)
Subtraction, 0-12: Post	6.71	(3.35)	7.42	(6.63)	6.93	(4.50)
Subtraction, 0-18: Pre	5.64	(3.31)	5.67	(6.11)	5.65	(4.26)
Subtraction, 0-18: Post	6.21	(3.24)	6.08	(5.88)	6.18	(4.13)
Composite Score: Pre	36.00	(13.32)	33.58	(27.37)	35.28	(18.31)
Composite Score: Post	37.00	(15.22)	34.25	(27.21)	36.18	(19.26)
Percent Correct of Problems Attempted						
Addition, 0-12: Pre	93.77	(11.46)	86.39	(28.99)	91.56	(18.43)
Addition, 0-12: Post	93.32	(20.06)	89.29	(29.29)	92.11	(22.90)
Addition, 0-18: Pre	91.92	(15.66)	96.33	(7.71)	93.24	(13.81)
Addition, 0-18: Post	89.98	(17.20)	89.26	(27.11)	89.77	(20.30)
Subtraction, 0-12: Pre	85.26	(14.99)	66.92	(39.26)	79.76	(25.74)
Subtraction, 0-12: Post	84.31	(24.27)	72.73	(30.56)	80.83	(26.46)
Subtraction, 0-18: Pre	84.62	(26.24)	60.34	(42.59)	77.34	(33.39)
Subtraction, 0-18: Post	87.42	(24.21)	66.53	(36.13)	81.15	(29.46)
Overall Average: Pre	90.28	(12.29)	80.29	(22.93)	87.28	(16.57)
Overall Average: Post	90.01	(17.83)	80.51	(27.39)	87.16	(21.24)

Note. Outliers included.

Results

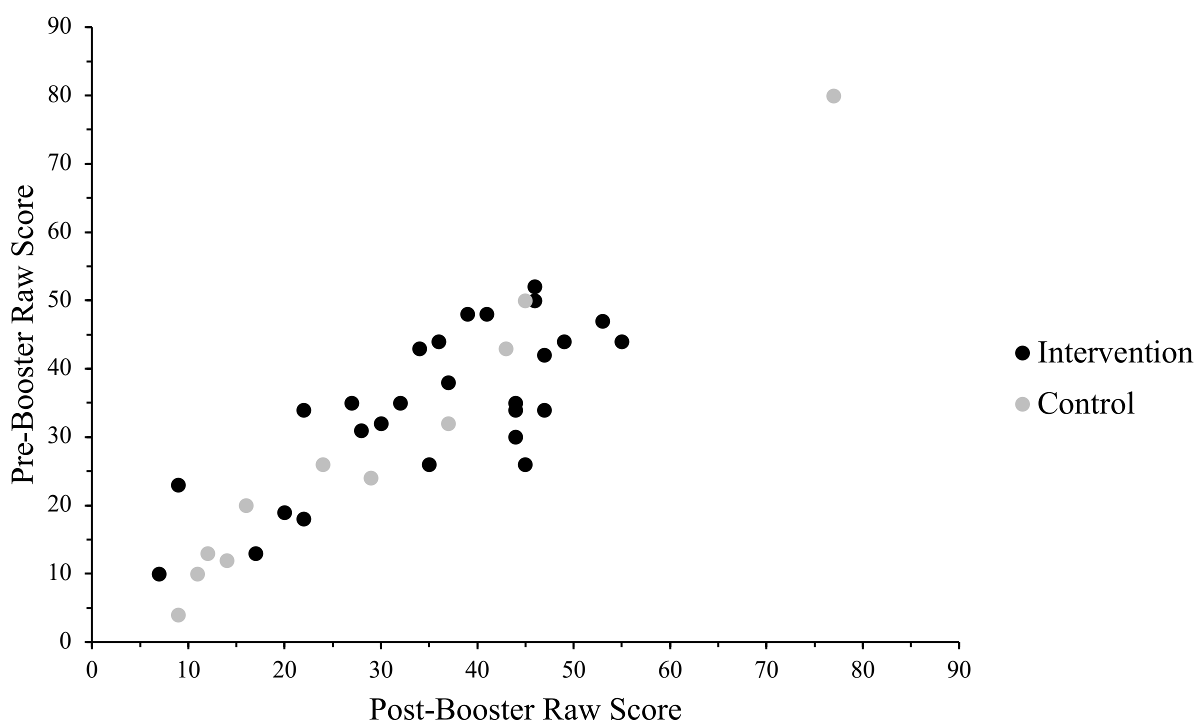
We tested the following ANCOVA assumptions to evaluate the reliability of the covariate, within-group outliers, the linearity of relation between the outcome variable and covariate, homogeneity of regression slope, and homogeneity of variance (Tabachnick & Fidell, 2020). When these assumptions are met, we can be confident that the ratio of the mean square of the effect over the mean square for error is distributed as F under the null hypothesis. To test the assumption of **reliability of the covariate**, Cronbach's alpha was computed to index internal consistency on the four-subtest First-Grade Mathematics Assessment Battery measured prior to the booster lesson. For the current sample, α was .965, reflecting reliability.

To locate **potential outliers** in the dataset, boxplots were created for the covariate and dependent variable separately for the first-grade intervention and first-grade control groups. Data points were deemed outliers when the score was less than the minimum value (i.e., Q1 – 1.5*IQR) or greater than the maximum value (i.e., Q3 + 1.5*IQR). When examining the boxplot on the composite outcome score, two outliers were located. One control group student's score was 94 (control group mean 34.25, *SD* = 27.21). One intervention group student's score was 80 (intervention group mean 37; *SD* = 15.22). On the composite covariate score, one significant outlier was located. The same intervention student's score was 73 (intervention group mean 36, *SD* = 13.32). Scores were double-checked in the original databases to confirm these outliers were not the result of an error in recording. The decision was made to remove these two cases from the present analyses. However, analyses with outliers included are presented in the Supplemental File.

Linearity among the covariate and outcome variable was assessed by inspecting a bivariate scatterplot. See Figure 1 for the scatterplot of the covariate on the outcome variable by group. Visual analysis of the scatterplot concluded there was a linear relationship among the covariate and the outcome variable. To check for **homogeneity of regression slopes**, a multivariate analysis of variance (MANOVA) was conducted. The interaction between the covariate and the independent variable was nonsignificant, $F(5, 8) = 0.527$, $p = .751$, indicating the slope of the DV-CV regression line was the same across groups and that the assumption of homogeneity of regression slopes was met.

Figure 1

Scatterplot of covariate and outcome variable by condition



Note. Scatterplot of covariate (pre-booster raw score) and outcome variable (post-booster raw score) by group.

To test the assumption of **homogeneity of variance** on our outcome measure and covariate, we conducted Levene's test for homogeneity of variance. On our outcome measure, Levene's test was non-significant, $F(1, 36) = 2.752$, $p = .106$, indicating homogeneity of variances between our two groups. On our covariate, Levene's test indicated significant heteroscedasticity in our variances and a violation of the assumption of equal variances, $F(1, 36) = 5.219$, $p = .028$. The larger variance was associated with the control group, the group with the smaller n (control $n = 12$; intervention $n = 28$). A larger variation in the group with the smaller n causes the F test to be too liberal, leading to an increased Type I error rate. Due to this violation, we chose to use a more stringent α level ($\alpha = .025$) when testing main effects, as suggested by Tabachnick and Fidell (2020).

The ANCOVA on the main study outcome, number of correct problems completed post-booster session, revealed no significant difference between conditions, $F(1, 35) = 0.177$, $p = .677$, partial eta squared = .005, ES = 0.08. There was a strong relation between pre-booster composite scores and post-booster composite scores, as indicated by a partial eta squared value of .766. Estimated marginal means are presented in Table 7.

The supplemental ANCOVA on the post-booster percent correct of attempted problems also revealed no significant difference between the two groups on post-booster accuracy of problems attempted, $F(1, 35) = 0.126$, $p = .725$, partial eta squared = .004, ES = –0.08. There was a strong relation between pre-booster accuracy of problem attempted, and post-booster accuracy of problem attempted, as indicated by a partial eta squared value of .643. Estimated marginal means are presented in Table 7.

Table 7

Estimated Marginal Means on Dependent Variable by Group

			95% Confidence Interval	
Group	Adjusted Mean	S. E.	Lower	Upper
Post-Booster Raw Score				
Control	32.676	2.315	27.976	37.376
Intervention	33.836	1.467	30.858	36.814
Post-Booster Percent Correct of Attempted				
Control	86.455	4.047	78.240	94.671
Intervention	84.726	2.524	79.602	89.850

CHAPTER 4

DISCUSSION

The purpose of this study was to increase understanding of the academic intervention fadeout phenomenon by testing the possibility that intervention effects persist following intervention but reside in a deactivated state. This deactivation of knowledge may occur because the subsequent instructional environment does not provide periodic contextual prompting to utilize the skills or because the transfer of skills learned during intervention fails to occur in contexts outside of the intervention. As a result, students do not access and use the learned skills, which become deactivated marginal knowledge. This deactivation may lead to the observed fadeout of effects that are common in follow-up studies of mathematics interventions (e.g., Bailey et al., 2020; Clarke et al., 2016; Hallstedt et al., 2018; Smith et al., 2013).

To test the hypothesis that skills learned during an intervention are stored in memory but are not readily accessible, we provided a brief arithmetic booster lesson to 40 third-grade students who had participated in a successful 15-week intervention study in first grade (Fuchs et al., 2021). Two-thirds of the sample had received an intensive mathematics intervention that included arithmetic instruction while in first grade, and one-third were in the control group.

The goal of the booster lesson was to provide a brief review of the counting strategies to solve addition and subtraction problems used in the parent study's intervention and assess whether the booster lesson produced a differential boost for students who received intervention over control students. Therefore, the booster lesson was designed with sufficient explicitness to remind previously tutored students of the skills they learned to solve addition and subtraction skills in first grade, but without enough review and practice to consolidate these skills in the control group. Because the booster lesson was designed to mimic the original counting strategy instruction students received during the first-grade intervention, we hypothesized the booster lesson would reactivate marginal knowledge in students who received intervention instruction while in first grade. We thus anticipated a stronger post-booster performance for intervention over control on timed arithmetic tasks while controlling for students' pre-booster lesson scores.

Contrary to this expectation, the brief booster lesson did not produce stronger arithmetic performance for intervention over control students on either index of arithmetic outcome. In this section, we discuss plausible explanations for the nonsignificant differences in post-booster performance between groups, including that the nonsignificant findings are likely explained by arithmetic skills developing in the counterfactual condition or result from the prolonged interval of time between the end of the first-grade intervention and the administration of the booster lesson. We then discuss the limitations that should be considered when interpreting student results, and we suggest directions for future research.

For first-grade students at-risk of MD, arithmetic skill seems like a strong trifecta skill that would be resistant to fadeout: Arithmetic is readily malleable through instruction; it is fundamental to later and more complex mathematical skills; and it is unlikely to develop in at-risk children in a typical classroom without explicit intervention. Even so, arithmetic skill may not meet the criteria for a trifecta skill in later grades. Relative to typically achieving children, children with MD struggle to commit basic facts to long-term memory or accurately retrieve them once they are learned (Andersson, 2010; Chan & Ho, 2010; Geary, 1993; Geary et al., 2012b). Yet, most children with MD do eventually acquire mastery of these basic facts without intervention, but one to several years behind their typically achieving peers (Andersson, 2010).

To catch up with their typically achieving peers, children with MD continue to show growth in retrieval of basic facts, especially in first through third grades (Geary et al., 2012b).

Given that at-risk students continue to improve their arithmetic skills between first and third grades, the field conceptualizes arithmetic skill as a delayed rather than deficit skill for students at-risk for MD (Andersson, 2010; Fletcher et al., 2019; Geary et al., 2012a; Jordan et al., 2003). It is thus possible the control group experienced delayed development of arithmetic skill, possibly combined with classroom or intervention instruction with a focus on fluent retrieval of basic facts in second and third grade. By contrast, intervention students maintained the arithmetic skill level they had demonstrated immediately after first-grade intervention. Given this potential for catch-up, simple arithmetic may not meet the third criterion for trifecta skill past first grade.

This raises the following question: If arithmetic skill is a delayed skill that eventually develops in at-risk students, can we save time and money by waiting to provide mathematics intervention until the later grades? We believe the answer to this question is no. This is because arithmetic interventions may represent a *foot-in-the-door process* (see Bailey et al., 2017). These interventions, if delivered at an opportune time, equip a child to develop more complex skills. Foot-in-the-door interventions may help a child speed up what would otherwise represent delayed skill development, thereby allocating cognitive resources, such as working memory or processing speed, to other subdomains of mathematics.

The effects of foot-in-the-door processes may not be permanent, and it may not be necessary for a student to retain the immediate target skill of the intervention to continue to benefit from the intervention and transfer the skills learned during intervention to novel and more complex skills. When developing a new skill, individual's mental processing is highly controlled, requiring active attention and high cognitive demands (Strayer & Kramer, 1994).

With increased practice, the processes used to perform the task become stronger and more efficient, requiring less active attention and reduce the demand on cognitive resources. This increased efficiency leads to a greater degree of automaticity of the skill and facilitate transfer of this skill to new situations and tasks. For example, in the parent study, at-risk students who received intervention had significantly stronger performance over control (Hedges' g 0.59 – 0.79) on arithmetic fluency measures following intervention (Fuchs et al., 2021). Repeatedly solving an arithmetic combination, even when using a counting strategy, strengthens the association between the problem stem and the answer. This strengthened association allows the arithmetic combination to become a routine fact that can be recalled at a later time (Siegler & Shrager, 1984). An increased reliance on automatic retrieval of basic facts can support students in later, more complex calculations (Fletcher et al., 2019) and help students develop procedural and conceptual knowledge of abstract mathematical principles such as decomposition, commutativity, and the associative law (Gersten et al., 2005). For example, if a child can easily retrieve *some* basic facts from memory (e.g., 6 + 6 = 12), then they can use this information to solve unknown problems (e.g., 6 + 7 = __), using other strategies such as decomposition (e.g., 6 + 6 + 1 = 13; Gersten et al., 2005; Jordan et al., 2003).

This is in line with other work showing that even mathematically proficient adults use multiple mental strategies instead of automatic retrieval to solve some arithmetic combinations (LeFevre et al., 2003). Although the original counting strategy may not be the student's primary method for solving arithmetic combinations, the student still benefits from the intervention because the counting strategy may have facilitated the automatic retrieval of some basic facts. While this shift toward automatic retrieval may be difficult and delayed in students at-risk for

MD (Geary et al., 2012b), it is a critical step toward the mastery of later subjects such as high school algebra (National Mathematics Advisory Panel, 2008).

The prior work examining the possibility that arithmetic interventions serve as a foot-in-the-door process has produced mixed results. In this study's parent study, Fuchs and colleagues (2021) found that improvement in arithmetic skill did not translate into stronger word-problem solving ability. Students who received 15 weeks of number knowledge intervention performed significantly better than at-risk students in the control group on arithmetic tasks (ES = 0.59); however, the benefits from the number knowledge intervention were not seen on word-problem solving tasks (ES = 0.09). By contrast, in a correlational study that included students with and without risk for MD, Fuchs and colleagues (2016) found that every unit increase in automatic retrieval in fourth grade was associated with an increase of 0.09 standard deviation units in word-problem solving.

This suggests that the foot-in-the-door process may work better for not-at-risk classmates than for students with MD. Because the fluent and accurate retrieval of basic facts requires the use of mature strategies (Gersten et al., 2005), students with MD may need more direct instruction and guidance in strategy use than their not-at-risk classmates. Future work should further examine the possibility that arithmetic interventions act as a foot-in-the-door process to support the automatic retrieval of arithmetic facts and also examine the potential influence of added transfer instruction. Studies should also examine moderators of the foot-in-the-door process, such as a history of difficulty with mathematics.

A second explanation as to why the booster lesson did not produce a significantly stronger post-booster performance for intervention over control students is the extended interval of time between the intervention and the delivery of the booster lesson. While an extensive literature supports the use of distributed practice to support retention, much of the published literature has examined retention only hours or days after the initial exposure. For example, Bahrick et al. (1993) found that distributed practice delivered in 56-day intervals produced superior recall of information compared to distributed practice delivered in 14- or 28-day intervals.

By contrast, the present study's interval between intervention completion and the booster lesson was two years. This is a lengthy period to reactivate a multi-step counting strategy, especially if the student has not frequently used the strategy during this time. If the subsequent educational environment did not provide frequent contextual prompting to encourage students to practice their learned strategies and explicit instruction to transfer those strategies to more complex tasks, it is probable these skills were actually forgotten in the two-year follow-up period. Had the booster lesson been delivered at 8-week intervals (as in the Bahrick et al., 1993 study), we may have observed superior retention. The possibility that more frequent booster lessons are needed to support the retention of skills learned during intervention opens the door for future research to identify optimal spacing to support retention of learned skills among students with MD.

To evaluate the long-term effectiveness of an intervention, it is necessary to examine the subsequent progress of students who took part in the intervention to determine if students who initially benefitted from receipt of the intervention continue to benefit from the intervention and what degree of subsequent instructional support is required to maintain initial gains. Although the present study’s booster lesson did not provide a differential boost for intervention over control, it is possible that booster lessons provided earlier and at more frequent intervals would produce a differential boost. For example, in a recent study (Nelson et al., 2020), students who received an intensive early reading intervention were provided with brief (2 – 3 min), weekly follow-up opportunities to practice intervention targets with feedback on their performance (Nelson et al., 2020). Students who received the initial intervention plus weekly practice opportunities were more likely to meet end-of-year benchmarks than students who received the initial intervention but did not receive the follow-up practice opportunities. These brief additional practice opportunities may have facilitated the retention of learned material because they provided students with frequent and contextual prompting to use the learned skills.

To understand how brief practice lessons can support intervention maintenance, future research should aim to determine the optimal delivery interval needed to maintain skills and determine if these intervals vary by skill or grade level. Given the significant fadeout seen in the months and years following mathematics interventions (e.g., Bailey et al., 2020; Clarke et al., 2016; Hallstedt et al., 2018; Smith et al., 2013), it seems a worthwhile investment to determine the optimal spacing of delivery (e.g., weekly, monthly) needed to support persistence of effects.

Before concluding, we note several study limitations that should be addressed in future research. First, we were unable to include all students who previously participated in the parent study. While we originally intended to include 95 students, we located and delivered the booster lesson to only 40 students before schools closed due to the COVID-19 pandemic. Unfortunately, this resulted in unequal sample sizes between our two study conditions (i.e., intervention $n = 28$; control $n = 12$). While the ratio intervention to control group sample size met the recommended 4:1 ratio when running an ANCOVA (Tabachnick & Fidell, 2020), the group with the smaller n (control group) had larger variation in the data. This heteroscedasticity combined with our overall small sample size causes the F test to be too liberal, leading to an increased Type I error rate. We adjusted for this increased Type I error rate by applying a more stringent alpha level when testing main effects; however, future work should address larger samples with relatively equal group sizes.

Second, our study did not account for preexisting differences in student demographics or home environments. Prior work has found that approximately 72% of mathematics intervention fadeout is attributable to differences in student-level variables such as socioeconomic status and parent education level (Bailey et al., 2016). It is possible these variables affect the reactivation of marginal knowledge after a booster lesson. Unfortunately, due to this study's small sample size, we did not have the power to examine these variables. Future work should examine demographic variables as potential moderators of booster lesson effects.

Third, our accuracy variable, the composite percentage correct of attempted problems across the four subtests, was constrained in the context of our fluency measure. That is, the accuracy variable was computed based on the number of problems the student attempted during the timed test. This may not represent an accurate portrayal of arithmetic accuracy. Future work should continue to investigate the use of booster lessons on accuracy and fluency, using a more precise measure of accuracy.

A final limitation is the absence of information regarding school intervention support during the study's follow-up period. Students may have received school-based mathematics interventions as part of their school's tiered support system in the months and years following the first-grade intervention. It is possible the nonsignificant effect of the booster lesson is a result of schools providing arithmetic intervention to the parent study's control group students but not to the parent study's intervention students. This might occur because students who received first-grade intervention likely demonstrate stronger mathematics performance between start of second grade and end of third grade. Future work should obtain information on school-provided intervention during the follow-up period, including instructional focus and intensity.

Finally, future research should continue to investigate the role of booster lessons in reactivating marginal knowledge by exploring the use of distributed practice on skill retention and activation not only for arithmetic but also for other interventional focal skills. The goal is to help schools plan efficient and effective post-intervention services to optimize the maintenance of intervention effects.

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Appendix A
Counting Strategies Booster Lesson Script

Posters:	Open Hand with Plus Sign and Closed Hand with Minus Sign Know It or Count! Count IN to Add Count UP to Subtract
Worksheets:	Booster - 1 Booster - 2 MOBYS Graph
Materials:	Flashcards Timer Pencil Crayons

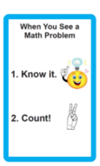
INTRODUCTION

When you were in 1st grade, your teacher was ______. You learned a lot about solving math problems with ______.

You may have also had intervention in your school or tutoring with a Vanderbilt tutor on a math program called Pirate Math or Galaxy Math.

Today we'll review some of the things you learned about math in 1st grade. At the end we'll play a game called Meet or Beat Your Score. You may remember playing this game with your Vanderbilt tutor or your school interventionist. If you work hard and follow directions, you can earn a prize from my prize box (point to prize box)**.**

COUNTING STRATEGY REVIEW



(Display Know It or Count poster and Booster worksheet 1. Use cover sheet so that only the top row of problems on worksheet is displayed.)

There are two ways to get the answer to a math problem like this (point to 1 + 1). **The first way is to know it** (point to #1 on Know It or Count poster)**. If you know it right away, pull the answer right out of your brain. Like this: I know the answer to 1 + 1 right away. 1 plus 1 equals 2** (write 2 on worksheet)**. I pulled the answer right out of my brain. You try. What's the answer to 2 + 2** (point)**?** (Student.) **Right! 2 plus 2 equals 4.** (Student writes 4 on worksheet.) **You just pulled the answer out of your brain** (point to Know It or Count poster)**. You can put your pencil down.**

But if you don't know the answer right away, you figure out the answer by counting (point to #2 Know It or Count poster)**. To add, you count <u>in</u>.**

Display Open Hand with Plus Sign and Closed Hand with Minus Sign poster. Fold poster so that only the Open Hand side is displayed.

Look at this picture of an open hand (point to picture of open hand on poster)**, it has a plus sign on it. This reminds you to start adding with your hand open.**

(Display Count IN to Add poster.) **This poster reminds you how to count in to add.**

Slide cover sheet down to display next row of problems on Booster worksheet 1.

Let's practice with this problem (point to each part of problem as you read)**: 8 plus 7.**

> If the student quickly blurts out the answer, say: **It's great you knew the answer right away! Let's pretend you don't know the answer. That way, you can practice how to find answers when you <u>don't</u> know the answer right away.**

This problem has a plus sign (point to plus sign on worksheet)**. It's addition. So you count in. Step 1** (point to poster) **says, "Open hand." Open your hand to show the smaller number. In the problem, 8 plus 7, what's the smaller number** (point to 8 and 7)**?** (Student.) **Yes, 7 is the smaller number. So, I open my hands, and I show 7 fingers.** (Open both hands and hold up 7 fingers. Leave fingers up while continuing with next step.)

Step 2 (point to poster) **says, "Count in." Start counting with the bigger number. The bigger number is 8. So, I start counting with 8 and I push one finger into my hand for each number I count. Watch how I count in: 8** (bump fist to wrist)**, 9** (push down 1st finger)**, 10** (push down 2nd finger)**, 11** (push down 3rd finger)**, 12** (push down 4th finger)**, 13** (push down 5th finger)**, 14** (push down 6th finger)**, 15** (push down 7th finger)**.**

(Point to step 3 on poster.) **The answer is the last number I say. The last number I said was 15. So I write 15 here** (write 15)**. 8 plus 7 equals 15.**

Now you try (point to next problem)**. Read this problem.** (Student.) **Right! 5 plus 9. It's addition so start with your hand open. Now show me how you count in to add.** Allow student to complete the problem aloud using the steps discussed. If student makes an error, help as shown below.

If needed: **That's not quite right. Remember Step 1** (point to poster)**, open your hand and show the smaller number. The smaller number is 5. Hold up 5 fingers to show the smaller number.** (Student holds up 5 fingers.)

If needed: **Remember Step 2** (point to poster), **count in to add. Start counting with the bigger number. The bigger number is 9. Start counting with 9. Push one finger into your hand for each number you count.**

If needed: **That's not quite right. You start at 9** (bump fist to wrist). **Now count 10** (push down 1st finger), **11** (push down 2nd finger), **12** (push down 3rd finger)**, 13** (push down 4th finger)**, 14** (push down 5th finger)**.**

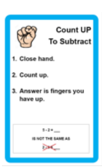
If needed: **What's the last number you said** (model the counting if needed)**.** (Student.) **Write your answer.** (Student writes.)

Yes! 5 plus 9 equals 14. Remember. When you add, open your hand to show the smaller number and then start counting with the bigger number. The answer is the last number you say.

Now let's talk about subtraction. You also use your hand to subtract. But when we subtract we count _up_. This is different from counting _in_ to add.

Display Open Hand with Plus Sign and Closed Hand with Minus Sign poster. Fold poster so that only the Closed Hand side is displayed.

Look at this picture of a closed hand (point to picture of closed hand on poster)**, it has a minus sign on it. This reminds you to start subtracting with your hand closed.**



(Display Count UP to Subtract poster.) **This poster reminds you how to count up to subtract.**

Slide cover sheet down to display final row of items of Booster worksheet 1.

Let's practice with this problem (point to each part of problem as you read)**: 11 minus 3. This problem has a minus sign** (point to minus sign on worksheet)**. It's subtraction. So you count up.**

Step 1 (point to poster) **says, "Close hand." Close your hand to hold the minus number. What's the minus number?** (Student.) **Yes, 3 is the minus number. So I close my hand and hold the minus number, 3.** (Close hand to make a fist. Leave hand in first while continuing with next step.)

Step 2 (point to poster) **says, "Count up." Start counting with the minus number. So, I start counting at 3 and count up to the other number, 11. Watch how I count up: 3** (close hand)**, 4** (hold up 1 finger)**, 5** (hold up 2 fingers)**, 6** (hold up 3 fingers)**, 7** (hold up 4 fingers)**, 8** (hold up 5 fingers)**, 9** (hold up 6 fingers)**, 10** (hold up 7 fingers)**, 11** (hold up 8 fingers)**.**

Point to step 3 on poster. **The answer is the number of fingers you used to count up. I used 8 fingers to count up** (show hands)**. So, the answer is 8.** (Write 8.) **11 minus 3 equals 8.**

Now you try (point to next problem)**. Read this problem.** (Student.) **Right! 13 minus 8. Is this addition or subtraction?** (Student.) **Yes, it's subtraction. Is your hand open or closed?** (Student.) **Right. Now show me how to count up to subtract.**

Allow student to complete the problem aloud using the steps discussed. If student makes an error, help as shown below.

> If needed: **That's not quite right. Remember Step 1** (point to poster)**, close your hand to hold the minus number. The minus number is 8** (point)**. Close your hand and hold the minus number, 8.** (Student makes fist.)
>
> If needed: **Remember Step 2** (point to poster)**, count up to subtract. Start counting with the minus number. So, I start counting at 8 and count up to the other number, 13.**
>
> If needed: **That's not quite right. You hold the minus number, 8, in your hand** (close hand) **and count 9** (hold up 1 finger)**, 10** (hold up 2 fingers)**, 11** (hold up 3 fingers)**, 12** (hold up 4 fingers)**, 13** (hold up 5 fingers)**.**
>
> If needed: **How many fingers did you use to count?** (Student.) **Write your answer.** (Student writes.)

Yes, 13 minus 8 equals 5. Remember. When you subtract, start with a closed hand to count up. Close your hand to hold the minus number and then count up to the other number. The answer is the number of fingers you used to count.

That was good work! Now let's practice using Count IN and Count UP to solve some addition and subtraction problems.

Display Booster worksheet 2. RA and student work through problems together. For each problem, RA should:

1. Ask student to read problem aloud.
2. Ask **"Do you know it right away or should you count?"** If student knows answer right away, allow them to write answer and move on to next problem.
3. Ask **"Is this addition or subtraction?"**
4. Ask **"Should you use an open hand or closed hand?"**
5. Guide student through Count IN or Count UP steps as needed (see table below).

Count In to Add	Count Up to Subtract
If needed: **That's not quite right. Remember Step 1** (point to poster)**, open your hand to show the smaller number. The smaller number is XX. Hold up fingers to show the smaller number.** (RA and student hold up fingers.) If needed: **Remember Step 2** (point to poster), **count in. Start counting with the bigger number. The bigger number is XX. Push one finger into your hand for each number you count.** If needed: **That's not quite right. You start at XX** (bump fist to wrist). **Now count in.** (Model Counting In for student). If needed: **What's the last number you said** (model the counting if needed)**.** (Student.) **Write your answer.** (Student writes.) RA read entire problem with answer aloud.	If needed: **That's not quite right. Remember Step 1** (point to poster)**, close your hand to hold the minus number. The minus number is XX** (point)**. Close your hand to hold the minus number.** (RA and student make fist.) If needed: **Remember Step 2** (point to poster)**, count up to subtract. Start counting with the minus number. So, I start counting at XX and count up to the other number, XX.** If needed: **That's not quite right. You hold the minus number in your hand** (close hand) **and count.** (Model Counting Up for student). If needed: **How many fingers did you use to count?** (Student.) **Write your answer.** (Student writes.) RA read entire problem with answer aloud.

GAME

Display Open Hand with Plus Sign and Closed Hand with Minus Sign, Count IN to Add, and Count UP to Subtract posters.

Now let's play a game! It's called Meet or Beat Your Score. You may have played this with your Vanderbilt tutor or with your school interventionist. (Show flash cards.) **Each card has one math problem on it. I'll show you one card at a time. Look at the problem and tell me the answer as fast as you can.**

For each problem, there are two ways to get the answer. You know it or you count like I just showed you. If you don't know the answer right away, count IN or count UP to find the answer (point to both posters)**.**

If you get the right answer, I put it in a pile on the table. If you don't get it correct, I'll show you how to count to get the answer. Then, I show you the next card.

You have 30 seconds to answer as many math problems as you can. I hold up a problem. You give me the answer.

Let's practice. (Hold up flash card.) **What's the answer? If you don't know the answer, count.** *(Student.)*

> If student answers problem correctly:
> Say to student, **That's correct!**
>
> If student answers incorrectly or pauses 3 sec:
> Model the counting strategy on the back of flashcard.

That was good practice! Now I'll set the timer for 30 seconds and you'll try again. At the end of 30-seconds, we'll count the number of cards in the pile.

Are you ready? Let's try. (Set timer and show flash cards for 30 seconds.)

> If student answers problem correctly:
> 1. Place card in a pile on the table.
> 2. Present next card to student.
> 3. Continue until timer beeps.
>
> If student answers incorrectly or pauses 3 sec:
> Model the counting strategy on the back of flashcard and place card in pile.

When the timer beeps: **Great job! Let's count the cards in the pile.** (Count cards.) **You answered XX math problems correctly.**

Now, let's graph your score. Since you answered XX math problems correctly, we are going to color your graph to XX. (RA outline total number of correct problems on graph and allow student to color.)

Thank you for working so hard! Now let's move on to our last activity.

Appendix B
Counting Strategies Booster Lesson Fidelity Checklist

Scoring Code: Yes Behavior Demonstrated
No Behavior Omitted

Introduction			
1.	RA states student ID and date aloud on recorder.	Yes	No
2.	RA reads from script to introduce tutoring session.	Yes	No
Counting Strategy Review			
3.	RA explains the two ways to solve a math problem: Know it or Count.	Yes	No
4.	RA explains that to add, you start with your hand open.	Yes	No
5.	RA models Count In to Add, referencing steps on poster.	Yes	No
6.	RA has student Count In to Add, providing assistance as needed.	Yes	No
7.	RA reminds student to begin with an open hand to add and start counting with the bigger number when adding.	Yes	No
8.	RA explains that to subtract, you start with your hand closed.	Yes	No
9.	RA models Count Up to Subtract, referencing steps on poster.	Yes	No
10.	RA has student Count Up to Subtract, providing assistance as needed.	Yes	No
11.	RA reminds student to begin with a closed hand to subtract, to start counting at the minus number, and to count up to the other number when subtracting.	Yes	No
12.	RA and student use Count In to Add and Count Up to Subtract to complete problems on Booster worksheet 2.	Yes	No
13.	RA uses appropriate corrective feedback throughout lesson.	Yes	No
Game			
14.	RA introduces and explains how to play Meet or Beat Your Score.	Yes	No
15.	RA has student practice solving one math problem, providing assistance if needed.	Yes	No
16.	RA and student play Meet or Beat Your Score for 30 seconds.	Yes	No
17.	RA uses appropriate corrective feedback (from back of flashcard), if needed.	Yes	No
18.	RA and student graph score.	Yes	No

Upon completion of lesson:

2 = Highly Effective	*1 = Moderately Effective*	*0 = Ineffective*	*N/A = Not Applicable*

_________ Rate the tutor's effectiveness in explaining the lesson's concepts and strategies.

_________ Rate the tutor's effectiveness in providing feedback for correct and incorrect responses to extend student understanding.

_________ Rate this lesson's consistency with the lesson script, while maintaining lesson flow.

Appendix C
Outliers Included Analyses

With outliers included, the ANCOVA on the main study outcome, number of correct problems completed post-booster session, revealed no significant difference between conditions, $F(1, 37) = 0.024$, $p = .878$, partial eta squared = .001. There was a strong relation between pre-booster composite scores and post-booster composite scores, as indicated by a partial eta squared value of .850. Estimated marginal means are presented in Table S1. The ANCOVA on the supplemental score post-booster percent correct of attempted problems also revealed no significant difference between the two groups on post-booster accuracy of problems attempted, $F(1, 37) = 0.130$, $p = .721$, partial eta squared = .003. There was a strong relation between pre-booster accuracy of problem attempted and post-booster accuracy of problem attempted, as indicated by a partial eta squared value of .655. Estimated marginal means are presented in Table S1.

Table C1

Estimated Marginal Means on Dependent Variable by Group with Outliers Included

			95% Confidence Interval	
Group	Adjusted Mean	S. E.	Lower	Upper
Post-Booster Raw Score				
Control	35.890	2.206	31.421	40.360
Intervention	36.297	1.443	33.373	39.221
Post-Booster Percent Correct of Attempted				
Control	87.107	3.739	79.532	94.682
Intervention	85.476	2.405	80.603	90.350