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CHILD MATH ACHIEVEMENT: RELATION TO SELF-RELEVANT MATH ATTITUDES, PARENT MATH ATTITUDES, AND PARENT TALK

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ABSTRACT

Children's math achievement is related to various factors, including foundational math skills, math attitudes, and parent math attitudes and behaviors. In this dissertation, I focus on children's early math achievement and aim to improve our understanding of the math achievement-attitude relation, and the role of parent math attitudes and parent talk.

In Study 1, findings suggest that by 1st grade, math anxiety negatively predicts math achievement, over and above foundational math skills, and is particularly detrimental for performance on math tasks at the cusp of children's math skills. Thus, math anxiety may be particularly harmful in school settings, where children are continuously exposed to new math skills at the cusp of their learning levels. In Study 2, I focus on young children from low SES backgrounds, to improve our understanding of factors that might contribute to their low math achievement. Findings suggest that at the start of formal schooling, math achievement plays a role in the development of positive or negative math attitudes regardless of SES backgrounds. Further, parent math expectancy-value is an important predictor of child math achievement, regardless of SES backgrounds. In Study 3, using inverse probability of treatment weighting (IPTW), I found that parent number talk and other talk to toddlers causally affected math achievement in the preschool and elementary school years.

Taken together, findings from this dissertation suggest that interventions focused on increasing parent math expectancy-value and parent talk, hold promise for increasing children's math achievement and potentially for narrowing SES-related math achievement gaps.

INTRODUCTION

Early math skills are essential for the development of later math skills (Geary et al., 2018). Further, math skills are important for later life outcomes, such as health and income (Murnane et al., 1995; Rose & Betts, 2004). Children from lower SES backgrounds have lower math achievement than their peers from higher SES backgrounds, and this SES math achievement gap has widened by about 25% in recent years (Reardon 2011, 2021). Therefore, it is important to examine math achievement and factors that may influence it in children from diverse SES backgrounds. Understanding what factors may influence early math achievement in children from diverse SES backgrounds, is essential to improving our knowledge of how to ensure young children's success in math. In this dissertation, I focus on factors that have been shown to be important predictors of young children's math achievement: foundational math skills, self-relevant math attitudes (i.e., attitudes about one's own relationship with math), key socializers' math attitudes, and key socializers' behaviors (Berkowitz et al., 2015; Levine & Pantoja, 2021; Levine et al., 2010; Schaeffer et al., 2018).

Levine and Pantoja (2021) proposed the Early Math Achievement-Attitude (EMAA) model (see Figure 1) to explain links between child math achievement, child math attitudes, key socializers' behaviors, and key socializers' math attitudes. The EMAA model is based on a comprehensive review of the literature and raises important questions that should be addressed by further research. For example, how impactful are math attitudes at young ages? What do relations that have been examined in families from higher SES backgrounds look like in children from diverse SES backgrounds? Do parent behaviors have a causal effect on their children? With three studies, this dissertation aims to improve our understanding of the links described in the

EMAA model. In the following sections, I elaborate on the specific components and links described in the EMAA model, that this dissertation examines.

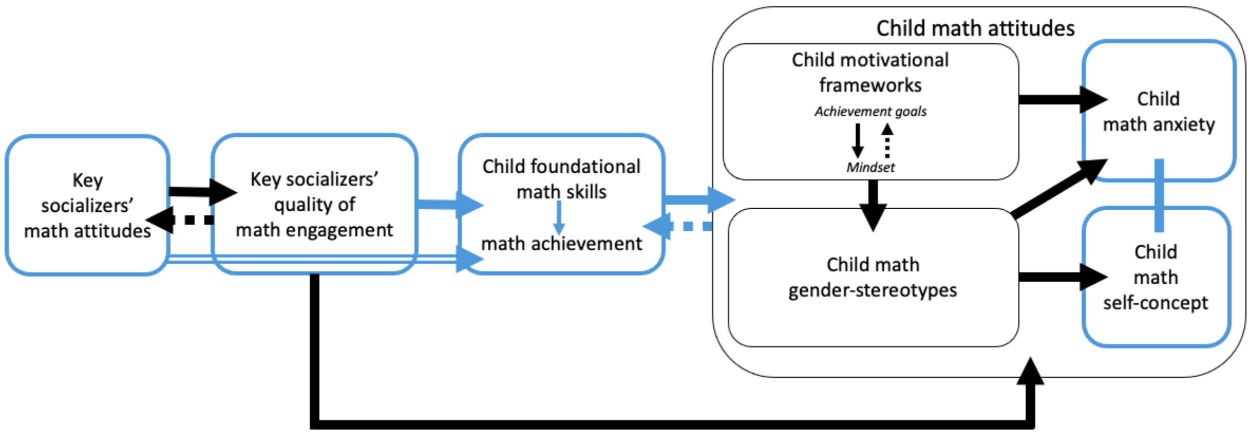


Figure 1. This figure was adapted from Levine and Pantoja (2021) [1]. The Early Math Achievement-Attitude (EMAA) model focuses on the emergence of the relation between math attitudes and math achievement and includes the role of key socializers (e.g., parents). For bidirectional relations, solid lines represent the earlier emerging relation and dotted lines represent the later emerging relation. I have made three small modifications to better show how this dissertation expands on the EMAA model: 1) I included a hollow arrow from key socializers' math attitudes to child math achievement, as this is a relation I examine; and 2) I expand the box for child math achievement and include a link from foundational math skills to math achievement, as this is a relation I examine. Blue arrows and boxes indicate components and links that this dissertation examines.

Self-Relevant Math Attitudes Have Long-Term Impacts on Math Achievement

Math attitudes generally fall under two categories: general math attitudes (i.e., general attitudes about math ability, such as stereotypes and motivational frameworks) and self-relevant math attitudes (i.e., attitudes about one's own relationship with math, such as math anxiety and math self-concept). In this dissertation, I focus on self-relevant math attitudes, as prior research suggests that self-relevant math attitudes become more negative over time (Hembree, 1990; Jacobs et al., 2002; Wigfield et al., 1997), and can significantly undermine math achievement in adolescents and adults (Beilock et al., 2017; Foley et al., 2017; Hembree, 1990; Wigfield &

[1] This figure is from an Accepted Manuscript of an article published by Elsevier in *Developmental Review* on *Oct 26, 2021*, available online: *https://doi.org/10.1016/j.dr.2021.100997*

Eccles, 2000). Additionally, in the long-term, positive or negative self-relevant math attitudes can influence career choices (Wigfield & Eccles, 2020). Recently, research has begun to show that math anxiety and math self-concept negatively predict math achievement in young children as early as 1st grade (Dapp & Roebers, 2018; Dowker et al., 2019; Gunderson et al., 2018; Jameson, 2013; Ramirez et al., 2013, 2016; Vukovic et al., 2013; Valeski & Stipek, 2001; Wu et al., 2012, 2014). However, studies with young children typically examine cross-sectional data. In sum, we have a good understanding of how impactful self-relevant math attitudes are for adolescents and adults, yet we know little about how impactful self-relevant math attitudes might be for young children.

In Study 1, I focus on how impactful a self-relevant math attitude—math anxiety—is for the math achievement of young children from diverse SES backgrounds up to two years later. I control for a foundational math skill—number line estimation—that has been shown to be important for later math achievement, such as math problem solving and arithmetic (Booth & Siegler, 2008; Gunderson et al., 2012; Lyons et al., 2014; Schneider et al., 2018; Siegler & Booth, 2004). By integrating these two lines of research (the math anxiety literature and the number line estimation literature), I provide a more complete picture of child math development.

Further, in Study 1, I examine how math task difficulty can influence how impactful math anxiety might be for young children. Prior research has shown that math anxiety is more predictive of complex math tasks, such as complex calculations in adults and children, and less predictive of simpler math tasks, such as arithmetic problems that can be solved by memory retrieval in adults and geometric reasoning in children (Ashcraft & Faust, 1994; Faust et al., 1996; Vukovic et al., 2013). However, the math tasks that have been examined have differed in multiple ways, making it difficult to determine whether differences in the math anxiety to math performance relation were truly due to math task difficulty, or were instead due to the specific math skill assessed. In Study 1, I examine how the relation of math anxiety to math performance changes based on math task difficulty, by using the same math task and making it easier or more difficult for children. Thus, this study improves our understanding of the circumstances under which math anxiety might be most impactful for young children, by providing a more stringent test of how the relation of math anxiety to math performance differs based on math task difficulty.

The Emergence of the Math Achievement-Attitude Relation

After improving our understanding of how impactful a self-relevant math attitude might be for young children from diverse SES backgrounds in Study 1, I examine how this relation emerges in Study 2, focusing on children from low SES backgrounds. While much research suggests that the relation between self-relevant math attitudes and math achievement becomes bidirectional over time (Eccles & Wigfield, 2020; Namkung et al., 2019), it is important to understand how this relation emerges at the start of elementary school. One step toward understanding how the relation between self-relevant math attitudes and math achievement emerges, is to examine whether the relation is stronger from self-relevant math attitudes to math achievement, or from math achievement to self-relevant math attitudes in young children. This information can improve our understanding of how to break the vicious link between negative self-relevant math attitudes and low math achievement before this link becomes bidirectional and math achievement has been compromised over many years.

Recent research suggests that in young children, math achievement might initially predict self-relevant math attitudes (Arens et al., 2016; Ching et al., 2020; Ganley & Lubienski, 2016; Helmke & van Aken, 1995; Supekar et al., 2015). These findings suggest that math achievement-related experiences may play an important role not only in math learning, but also in the development of positive or negative self-relevant math attitudes, which as mentioned earlier, can have long-lasting impacts on future career choices. However, prior research has focused on one self-relevant math attitude at a time, making it difficult to determine whether apparent relations persist accounting for other math attitudes, and whether math achievement-attitude relations emerge together or over different time frames. By integrating two lines of research (the math anxiety literature and the math self-concept literature), I provide a more complete picture of how the link between self-relevant math attitudes and math achievement emerges in young children.

The Role of Parent Math Attitudes

In Study 2, I examine the role of key socializers' math attitudes in predicting children's math achievement and math attitudes. While key socializers include parents and teachers, in this dissertation I focus on parents, as during toddlerhood and at the start of formal schooling children have been around their parents for much longer than they have been around their teachers, with whom they typically don't spend more than one school year. Additionally, evidence suggests that non-school factors, such as socioeconomic status are an important source of SES-related math achievement gaps, as these gaps increase during the summer, when children are not in school (Downey et al., 2004). I focus on parent math anxiety and math expectancy-value, two parent math attitudes that predict child math achievement as early as 1st grade (Berkowitz et al., 2015; Fredricks & Eccles, 2002; Schaeffer et al., 2018; Wigfield et al., 1997). Parent math anxiety is a self-relevant math attitude for reasons described earlier. Parent math expectancy-value is a child-specific math attitude and refers to parents' expectations of their child's future math achievement and how valuable they think math is for their child's future.

Situated Expectancy-Value Theory posits that parent math expectancy-value influences child math self-concept and math achievement (Eccles & Wigfield, 2020; Wigfield et al., 2006, 2015).

These two lines of research (the parent math anxiety literature and the parent math expectancy-value literature) have not been well integrated, and studies that have examined the relation of these parent math attitudes to child math achievement have typically focused on adolescents or samples from higher SES backgrounds. Thus, it is difficult to determine whether apparent relations persist after accounting for other factors, and whether they are present in young children. In Study 2, I take a more comprehensive approach to understanding the role of parent math attitudes, by examining both the role of parent math anxiety and math expectancy-value on young children's math achievement and self-relevant math attitudes, while accounting for prior child math achievement and self-relevant math attitudes.

The mechanism through which these parent math attitudes predict child math achievement is likely parent behaviors. Parent math anxiety predicts the quantity and quality of number talk they provide to their toddlers and preschoolers (Berkowitz et al., 2021; del Río et al., 2017). In turn, parent behaviors, such as the quantity and quality of their number talk, predict child math achievement (Gunderson et al., 2011; Levine et al., 2010). For this reason, Study 3 examines the role of parent behaviors on child math achievement.

The Role of Parent Behaviors

After improving our understanding of the role of parent math attitudes in Study 2, in Study 3, I examine the role of parent behaviors, which have been shown to be predicted by parent math attitudes. Parent number talk has been shown to predict child math achievement across various stages of child math development, with studies typically focusing on the preschool and elementary school years (Casey et al., 2018; Elliott et al., 2017; Glenn et al., 2018;

Gunderson & Levine, 2011; Levine et al., 2010; Ramani et al., 2015; Susperreguy & Davis-Kean, 2016; Thippana et al., 2020). Further, providing families with math tools that likely increase parent number talk improve child math skills (Berkowitz et al., 2015; Eason et al., 2018; Gibson et al., 2021), although, the effects of short-term interventions often fadeout (Bailey et al., 2016; Espinas & Fuchs, 2022). Thus, there is good reason to expect that parent number talk would affect child math achievement. There is also good reason to expect that other types of parent language input, such as overall talk, would affect child math achievement, as parent syntax and vocabulary input affect child syntax and vocabulary skills (Silvey et al., 2021), both of which are linked to child math skills (Espinas & Fuchs, 2022). Therefore, Study 3 examines both the effect of parent number talk and the effect of parent other talk (i.e., overall talk that excludes number talk) on child math achievement.

In Study 3, I examine the causal effect of naturalistic parent number talk and parent other talk, provided when toddlers were 14 and 38 months old, on cardinal number knowledge at 46 months and calculation skills in 3rd grade. Cardinal number knowledge is a foundational math skill necessary for more complex math skills (Geary et al., 2018), and calculation is an important numerical skill that predicts children's future use of advanced math strategies (Throndsen et al., 2011). I use inverse probability of treatment weighting (IPTW), a statistical approach that allows us to treat observational data of parent talk provided at different time points as if they came from a randomized experiment. This statistical approach improves our understanding of the causal effects of naturalistic parent talk on child math achievement and the optimal timing of parent talk for math skills assessed years later.

IPTW has been used to examine causal effects of time-varying parent language input on child language outcomes (Silvey et al., 2021). While parent vocabulary input both earlier at 14 months and later at 30 months were key for vocabulary outcomes in kindergarten, syntax input later was key for syntax outcomes. Thus, it is possible that the optimal timing of parent talk may differ for different types of parent talk (i.e., number talk or other talk) and different types of math skills (i.e., cardinal number knowledge at 46 months or calculation skills in 3rd grade).

In addition, in Study 3, I examine what parent and child characteristics, such as household income and child gender, predict different types of parent talk (e.g., parent number talk and other talk provided at 14 months and 38 months). This information enhances our understanding of why children receive differing levels of parent talk. One factor that has been shown to be predictive of parent talk to their children, is family SES (Dailey & Bergelson, 2021; Dearing et al., in prep; Levine et al., 2010; Silvey et al., 2021). A meta-analysis showed that parent education and household income accounted for 12% and 14% of the variation in parent number talk (Dearing et al., in prep). Additionally, family SES may interact with parent math attitudes in predicting their behaviors. For example, Berkowitz et al. (2021) showed that parents with high math anxiety from higher SES backgrounds provide less number talk to their toddlers, but parents from lower SES backgrounds, provide infrequent number talk regardless of their math anxiety. Thus, it is essential to examine factors important for child math development in families from diverse SES backgrounds.

Social Context

Children from lower SES backgrounds perform lower in math compared to their peers from higher SES backgrounds, and this SES math achievement gap has widened in recent years, at least between cohorts assessed in 1970 and those assessed in 2000 (Reardon, 2011, 2021). A widening math achievement gap suggests that children's math experiences and opportunities at home and in school have become more unequal in recent years (Reardon, 2011, 2021). In other words, social contexts or settings, such as the school environment, may differ between children from lower and higher SES backgrounds.

Much of the prior research that motivated the research questions examined in this dissertation focused on families from higher SES backgrounds and non-minority backgrounds, yet there is reason to expect different findings in families from lower SES backgrounds and/or minority backgrounds. For example, for adolescents from lower SES backgrounds and adolescents with lower math achievement, self-relevant math attitudes are less predictive of math achievement (OECD, 2013). Further, correlations between self-esteem and achievement are weaker in African American adolescent males compared to other groups (Osborne, 1995; 1997). Yet, we know little about these relations in young children from low SES backgrounds and/or minority backgrounds. Understanding whether these patterns of weaker relations between self-relevant math attitudes and math achievement hold for children in early elementary school is important, because the math achievement-math attitude relation is likely emerging around then, and it may be easier to break the link at younger ages.

Due to the importance of taking social contexts or settings into consideration, the studies in this dissertation focus on families from diverse racial, ethnic, and SES backgrounds, for whom the home and school environment may differ. In Study 1, most families reported annual household incomes of over $100,000, but about one third of families reported annual household incomes of less than $50,000. Most students were white, about one third were Hispanic, and about 10% were African American or Black. In Study 2, most families were from low SES backgrounds, with an average household income of $29,800, and about 85% of students were African American or Black. We used the same math achievement measure in Studies 1 and 2, and child math achievement was much lower in Study 2, compared to Study 1. In Study 3, families were selected to be representative of the Chicago area in terms of income, race, and ethnicity. Average annual household income was about $62,000. In sum, across three studies, this dissertation represents families from diverse backgrounds.

Study 2 will provide information on the strength of the relations between parent math attitudes, child self-relevant math attitudes and child math achievement in African American or Black children from low SES backgrounds. Children from Study 2 attended schools that predominantly served students with free or reduced-price lunch and classroom math achievement was low, which is important to point out, due to the big-fish-little-pond effect (i.e., equally achieving students have a lower self-concept when attending higher-performing schools than lower-performing schools; Marsh, 1987). It is important to consider that findings from Study 2 may differ in studies where young children from low SES backgrounds attend schools where classroom math achievement or SES are higher on average, as social context (i.e., school experiences) may differ in important ways.

Overview of The Current Research

Together, Studies 1, 2, and 3 address important research questions raised by the EMAA model, improving our understanding of a) how impactful self-relevant math attitudes are at young ages, b) how relations between self-relevant math attitudes and math achievement emerge, c) the role of parent math attitudes and behaviors, and d) what these relations look like in families from diverse SES backgrounds.

In Study 1, I examine the role of a self-relevant math attitude on 1st grade children's math achievement through 3rd grade, controlling for a foundational math skill, thus integrating two lines of research (the math anxiety literature and the number line estimation literature). I also examine how the relation of math anxiety to math performance changes based on math task difficulty. Thus, I aim to improve our understanding of how impactful self-relevant math attitudes are for young children and for math skills of varying difficulty.

In Study 2, I examine the relation of self-relevant math attitudes to math achievement, and the relation of math achievement to self-relevant math attitudes in 1st grade children from low SES backgrounds. Thus, I aim to improve our understanding of how this relation emerges in young children. Further, I examine the role of parent math attitudes on child math achievement. This study integrates different lines of research (the literatures on math anxiety, math self-concept, and parent math expectancy-value) to provide a more complete understanding of child math development for families from diverse backgrounds.

In Study 3, I examine the causal effect of naturalistic parent number talk and parent other talk during two distinct stages of development (at 14 months and at 38 months) on cardinal number knowledge at 46 months and calculation skills in 3rd grade. By using IPTW, I aim to improve our understanding of the causal effect of naturalistic parent number talk and overall talk on child math achievement, and the optimal timing of parent talk. Further, in Study 3, I examine parent and child characteristics that are important predictors of parent talk, improving our understanding of why some children receive more parent talk than others.

Together, the three studies in this dissertation aim to broaden our understanding of factors important for child math development in families from diverse backgrounds.

STUDY 1: CHILDREN'S MATH ANXIETY PREDICTS THEIR MATH ACHIEVEMENT OVER AND ABOVE A KEY FOUNDATIONAL MATH SKILL[2]

The cognitive underpinnings of mathematics development have been extensively examined. One well-known line of research has implicated children's understanding of numerical magnitudes, commonly measured through number line estimation tasks, in their math competence (e.g., Berteletti et al., 2010; Booth & Siegler, 2008; Case & Okamoto, 1996; Fazio et al., 2014; Gunderson et al., 2012; Hoffmann et al., 2013; Lyons et al., 2014; Siegler & Booth, 2004). Another line of research has shown that children's math competence is associated with factors beyond foundational cognitive skills. Notably, children's math anxiety negatively relates to their math achievement as early as 1st grade (Gunderson et al., 2018; Ramirez et al., 2013, 2016).

To date, these two lines of research have not been well integrated, and there are many open questions about how foundational math skills, as well as math anxiety relate to future math achievement. Here we begin to address such questions by asking whether children's early math anxiety (in the fall of 1st grade) predicts their future math achievement (through 3rd grade), controlling for the linearity of their early number line representations (in the fall of 1st grade). One possibility is that math anxiety and number line estimation account for the same variance in future math achievement. However, we hypothesized that math anxiety would matter for children's math achievement, over and above their number line estimation, and that these two factors together would account for significantly more variation in future math performance than number line estimation alone.

[2] This is an Accepted Manuscript with minor modifications of an article published by Taylor & Francis in *Journal of Cognition* on *Nov 3, 2020*, available online: https://www.tandfonline.com/doi/full/10.1080/15248372.2020.1832098

The Relation of Math Anxiety and Math Achievement

Math anxiety, a common phenomenon across the globe (Foley et al., 2017), is defined as the fear or apprehension people experience when doing, or even thinking about, math related activities (Lyons & Beilock, 2012; Richardson & Suinn, 1972). Math anxiety has been shown to interfere with adults' ability to solve math problems both in daily life and in academic situations (Ashcraft, 2002; Beilock et al., 2017; Hembree, 1990; Richardson & Suinn, 1972). Moreover, the negative relation between math anxiety and math performance arises early, affecting children as early as 1st grade (Gunderson et al., 2018; Ramirez et al., 2013, 2016; Wu et al., 2012).

Some researchers have argued that math anxiety may stem from poor math ability (Carey et al., 2016; Fennema, 1989; Ramirez et al., 2018). For example, Ma & Xu (2004) reported that early math performance consistently predicted later math anxiety in 7th through 12th graders, while the reverse relation—that early math anxiety predicted later math performance—was hardly present. In young elementary school students, Gunderson et al. (2018) found that the relation between fall math performance and spring math anxiety was stronger than the reverse relation, but both of these links were significant, suggesting a bidirectional relation.

Also suggesting that math anxiety may stem from weak math skills, adults' math anxiety is related to basic numerical processing skills, such as counting objects (Maloney et al., 2010) and identifying the larger of two single digit numbers (Dietrich et al., 2015; Maloney et al., 2011). Additionally, math anxiety has been found to mediate the relation between adults' magnitude comparison (a skill further discussed below) and their math performance (Lindskog et al., 2017), at least in high math anxious adults (Moscoso et al., 2020), suggesting that math anxiety may play a central role in the relation between ANS and math performance. However, these findings are not consistent as other studies have found no relation between math anxiety and ANS in children and adolescents (Hart et al., 2016; Wang et al., 2015), as well as in adults (Braham & Libertus, 2018; Dietrich et al., 2015). Thus, there are discrepant results that need to be resolved regarding the relation of math anxiety, ANS, and math performance.

In contrast to the view that math anxiety may stem from poor math skills, research with both adults and children suggests that math anxiety can cause poor math performance. For instance, math anxiety has been found to negatively relate to math performance because it takes up working memory resources (e.g., Ashcraft & Kirk, 2001; Ramirez et al., 2013). Further, some studies have found that math anxiety is most detrimental to individuals with high working memory capacities (Beilock & DeCaro, 2007; Ramirez et al., 2013). Math anxiety may lead individuals with high working memory to adopt less efficient strategies when they solve math problems. For example, children with high working memory capacity who were math anxious adopted less efficient, error-prone finger counting strategies, which are typically used by children with lower working memory (Ramirez et al., 2016).

Regardless of the causal connection between math anxiety and math performance, an important question is whether both math anxiety and foundational math skills uniquely contribute to variance in children's math performance. Here we hypothesized that children's math anxiety would explain variance in their math achievement, over and above a foundational math skill. We tested this hypothesis by examining whether math anxiety predicts children's math achievement over and above a foundational math skill that has been found to be predictive of their math achievement: the linearity of children's number line estimation.

The Relation of Number Line Estimation and Math Achievement

The existence of numerical magnitude representations in humans has been extensively examined. Humans and other species have an approximate number system (ANS) through which they represent number magnitudes in an analog and continuous form (Dehaene, 1997; Gallistel & Gelman, 1992). Supporting this view, it is easier for people to distinguish between quantities that are farther apart than those that are closer together (known as the numerical distance effect; Moyer & Landauer, 1967). Additionally, people—at least those in cultures with left to right writing systems—respond to small numbers more quickly with their left hand and large numbers more quickly with their right hand, reflecting a spatial representation of numerical magnitudes, known as the spatial numerical association of response codes (SNARC effect; Dehaene & Changeux, 1993). A recent meta-analysis reported a positive relation between children's ANS acuity and their math achievement (Chen & Li, 2014; but see Leibovich & Ansari, 2016). Children's ability to represent numerical magnitudes is often measured with a number line estimation task.

In a typical number line task, participants indicate the position of a given number on a line that is anchored at both ends, usually with 0 on the left side and a multiple of 10 on the right. Initially, even for number lines that are relatively small in scale, young children place smaller numbers too far apart and larger numbers too close together, which has been characterized as a logarithmic representation of numbers on a number line (Siegler & Braithwaite, 2017; Siegler & Opfer, 2003; Siegler et al., 2011; but see Barth & Paladiño, 2011). Over time and with formal schooling children are able to succeed on number line tasks, placing numbers along the number line in a linear manner. Importantly, they are able to do this successfully for number lines with smaller more familiar right anchors prior to succeeding on placing numbers in a linear manner on number line tasks with larger, less familiar right anchors. The shift to a linear representation of numbers has been found to occur before kindergarten on a 0-10 scale, from kindergarten to 2nd

grade on a 0-100 scale, and from 2nd grade to 4th grade on a 0-1000 scale (Berteletti et al., 2010; Booth & Siegler, 2006; Siegler & Booth, 2004; Siegler & Opfer, 2003).

Number line estimation predicts later performance on math problem solving, arithmetic, calculation, and math achievement more broadly (Booth & Siegler, 2008; Gunderson et al., 2012; Lyons et al., 2014; Siegler & Booth, 2004). Additionally, number line estimation correlates with other measures of estimation such as measurement, knowledge of symbolic numbers and numerical order, counting ability, and number comparison (Berteletti et al., 2010; Booth & Siegler, 2006; Fazio, et al., 2014; Hoffmann et al., 2013); and engaging in number line estimation activates brain regions associated with numerical magnitude and spatial processes (Berteletti et al., 2015). Further, number line performance mediates the relation between ANS acuity in kindergarten and arithmetic skill in 1st grade (Wong et al., 2016), and is more strongly related to broad mathematical competence than numerical magnitude comparison or working memory (Schneider et al., 2018). The relation between number line estimation and math competence is robust, as it is found using various accuracy measures of number line estimation and various measures of math performance, suggesting that number line estimation taps into a foundational understanding of number that is broadly important for mathematical thinking (Gross et al., 2018; Schneider et al., 2018). In a meta-analysis, Schneider et al. (2018) reported a strong association between number line estimation and math competence (r=0.441), with 19.6% of the variance in math competence being explained by number line estimation.

While number line estimation explains some of the variance in children's math performance (19.6%; Schneider et al., 2018), there is considerable variation that is not explained by this foundational skill. To gain a better understanding of how children's math achievement develops, we need to consider cognitive factors that have consistently been found to be important (e.g., number line estimation), while also examining the role of non-cognitive factors, particularly children's math anxiety, which have also been associated with math performance. In this study, we strive to provide a more complete picture of children's math development by examining whether math anxiety predicts children's math achievement, controlling for their number line estimation. If math anxiety does not explain additional variance in children's math achievement over and above their number line estimation, that would suggest that math anxiety may reflect the strength of children's foundational math skills. However, if math anxiety accounts for additional variance in math achievement over and above their number line estimation, this would suggest that both foundational math skills and positive emotions about math contribute to math achievement.

The Relation of Math Anxiety to Math Tasks of Varying Complexity

In addition to examining whether math anxiety plays an important role in children's math achievement, it is also important to examine whether children's math anxiety matters for performance on all math tasks, or whether it matters more for particular math tasks. Existing findings suggest that math anxiety is more detrimental to performance on complex math tasks, compared to simple math tasks, and that this is the case for adults and children. For example, adults' math anxiety has been found to matter more for complex arithmetic problems involving calculations, and less for simple arithmetic problems involving memory retrieval (Ashcraft & Faust, 1994; Faust et al., 1996). Similarly, 2nd and 3rd grade children's math anxiety has been found to negatively predict performance on calculations, but not geometric reasoning (as assessed by a simple task in which children described, compared and classified shapes; Vukovic et al., 2013). Additionally, in a meta-analysis Namkung et al. (2019) found that math anxiety is more strongly related to children's performance on advanced math tasks that require multiple steps to complete than simpler math tasks. However, in these studies the math tasks children completed differed not only in their complexity, but also in their format, making it difficult to rule out the possibility that the format of the math tasks, rather than task complexity plays a role in how math anxiety relates to math performance. To address this issue, it is important to use a particular math task and systematically manipulate its complexity.

One study has examined the relation between math anxiety and math performance on a task that can be adjusted to be simpler or more complex, while remaining the same in structure. In a study of adults, Núñez-Peña et al. (2018) found that math anxiety negatively predicted performance on a number line task with less familiar anchors, but not performance on a number line task with more familiar anchors. Similarly, adults' math anxiety has been found to predict fraction number line estimation (Sidney et al., 2019), which could be considered a complex number line estimation task. However, this approach of manipulating math task complexity while keeping the structure of the task the same—as opposed to changing the format of the math task entirely—to examine how the relation between math anxiety and math performance depends on task complexity has not been used in studies of children.

In the current study, we address this question by examining the relation of children's math anxiety to performance on number line tasks that vary the magnitude of the rightmost anchor, which changes the range over which children must estimate their number placements. Additionally, we assessed children longitudinally from 1st grade through 3rd grade, an age range that has been shown to perform differently on number line tasks involving different scales (Booth & Siegler, 2006; Siegler & Booth, 2004; Siegler & Opfer, 2003). Importantly, because the structure of number line tasks remains constant as the task difficulty changes, number line estimation is an ideal task to use to examine how math anxiety relates to math performance over a developmental period when the difficulty of particular number line tasks changes.

We predicted that children's math anxiety would be particularly harmful for performance on challenging math tasks because math anxiety takes up working memory resources that are necessary for solving complex math problems (e.g., Ashcraft & Kirk, 2001; Ramirez et al., 2013). For very easy tasks, it is possible that the ruminations that math anxious children have about their math performance may not interfere with performance because children still have adequate working memory resources to succeed at these tasks. For very difficult tasks on which even low math anxious children do not place numbers in a linear manner, (e.g., a 0-1000 number line task for a 1st grader), math anxiety is also unlikely to contribute to performance because working memory resources are not a determinative factor in predicting their performance. Thus, it is possible that the relation between math anxiety and number line performance will be most marked for the scale that is at the cusp of children's skill at each grade level (i.e., is neither too easy nor too difficult).

We hypothesized that math anxiety would relate to performance on number line tasks with smaller scales in earlier grades and number line tasks with larger scales in later grades, in line with the idea that math anxiety is most detrimental to performance on math tasks at particular levels of difficulty. Such a finding would indicate that the relation between math anxiety and math performance may have reverberating negative effects on math learning, interfering with the learning of new skills and concepts at successive grade levels.

The Current Study

We assessed children's math anxiety, number line estimation on a 0-100 and a 0-1000 scale, and math achievement across five time points (1st grade fall and spring; 2nd grade fall and spring; 3^{rd} grade fall) to better understand how these variables relate to each other and change over time. We address the following two questions. First, does math anxiety predict future math achievement, controlling for number line estimation? Second, does the relation between early math anxiety and number line estimation on different number line scales change over time?

Regarding our first question, we hypothesized that 1^{st} grade math anxiety would remain a significant predictor of future math achievement, controlling for number line estimation, which would support the theory that math anxiety does not simply stem from poor math ability, but rather negatively contributes to math achievement over and above a foundational math skill. Regarding our second question, we hypothesized that 1^{st} grade math anxiety would negatively predict number line estimation on a 0-100 scale when children are younger (in 1^{st} grade), and on a 0-1000 scale later, when children are older (in 3^{rd} and perhaps 2^{nd} grade) reflecting the fact that these number line tasks are appropriately complex at these grade levels (i.e., not too simple and not too difficult).

Method

Participants

The data analyzed in the current study were collected as part of a larger longitudinal randomized control trial, examining the effectiveness of a math app that parents and children engaged with to support children's math learning (Berkowitz et al., 2015; Schaeffer et al., 2018). A demographically diverse sample of 1^{st} graders and their primary caregivers were recruited and followed longitudinally. Families were randomly assigned to an intervention (math) or control (reading) condition. In the current study, our analyses examined participants in the control condition, from the fall of 1^{st} grade through the fall of 3^{rd} grade, as the intervention condition could possibly alter the relations we focus on. The control condition included 176 children from

Chicago area schools. Fourteen children were excluded due to missing data, leaving us with 162 participants (81 girls) at our first time point. At subsequent time points, we had data for 104 to 150 participants, due to sample attrition over time (see Table 1). Caregiver reports of race/ethnicity (N=155) indicated that students were 51% White, 32% Hispanic, 9% African American, 6% Asian or Asian American, and 1% American Indian or Alaskan Native. Annual household income was reported by 138 caregivers with 7% earning less than $15,000, 15% earning $15,000-$34,999, 9% earning $35,000-$49,999, 8% earning $50,000-$74,999, 9% earning $75,000-$99,999, and 53% earning more than $100,000.

Measures and Procedure

In the current study, we focus on a subset of the measures given at each time point. Children completed tasks in their school in a one-on-one session with an experimenter during fall and spring of 1st and 2nd grade and fall of 3rd grade. At each time point, achievement measures were administered during one session, and emotion measures were administered during a session the following school day.[3]

Math Anxiety. Children completed a modified version of the revised Child Math Anxiety Questionnaire (CMAQ-R: Ramirez et al., 2016), which was designed to be appropriate for 1st graders. The 16-item measure asked children how nervous they would feel during various math-related situations. Eight math problems were modified for children in 3rd grade to reflect the type of math knowledge expected of older students. For example, 1st and 2nd graders were asked "How do you feel when you have to solve 34-7?", while 3rd graders were asked "How do

[3] Although subjects were supposed to receive the same number line order throughout all five time points, this was not the case for six subjects due to experimenter error. These six subjects received one of the following orders over the five time points: AAABA, ABBBB, BBABB, BBBAB. Additionally, one child only completed five out of the six trials on the 0-1000 scale due to experimenter error during 1st grade fall, and that child's number line estimation was measured based on those five trials.

you feel when you have to solve 35 divided by 7?" To respond, children pointed to one of five smiley faces displaying an emotional gradient from "not nervous at all" to "very very nervous". Math anxiety was scored on a scale of one (low math anxiety) to five (high math anxiety). Because the math anxiety questionnaire involved specific math problems which changed for older children, we did not compare changes in math anxiety scores.

Number Line Estimation. Children were shown a horizontal line anchored with 0 on the left and 100 or 1000 on the right. To respond, children drew a hatch mark through the number line indicating their estimate of the position of each requested number. Our measure—including the counterbalancing of the two number line tasks and the numbers requested—mirrored the measure developed and used by Siegler and Opfer (2003). Children were randomly assigned to receive Order A or B at all time points[4]. Order A included a 0-1000 scale first (230, 71, 4, 780, 18, 6) and a 0-100 scale second (42, 6, 71, 18, 2, 4). Order B included a 0-100 scale first (3, 25, 86, 6, 2, 67) and a 0-1000 scale second (390, 2, 810, 86, 6, 25). Children received a new blank number line for each trial. Number line estimation was scored by converting children's estimates to the equivalent numerical values and finding the percentage of variance explained (R^2) by the best fitting linear model that related their estimates to the requested numbers (linear R^2). We focused on linear R^2—as opposed to other measures such as percent absolute error (PAE)—in our analyses because this index captures internally consistent placements of children's responses. In other words, linear R^2 captures the linearity of children's estimation of the location of the numbers requested in relation to each other, even if these estimations were not necessarily

[4] In the achievement session, students were randomly assigned to either complete the number line task first followed by measures of academic performance including the Applied Problems subtest and a Vocabulary subtest, or to complete measures of academic performance first followed by the number line task. Students were equally likely to receive the 0-100 or 0-1000 number line tasks first, regardless of whether they started the session by completing the number line task. In the emotion session, students first completed measures of emotions towards a variety of academic subjects, including Theories of Intelligence. Students then completed measures of domain-specific anxiety (math, reading, and spatial), with the order of the domain randomized across students.

mapped onto the number line in an accurate manner (Mix et al., 2016). However, we also ran analyses using PAE, which was scored using the following formula: |Participant estimate – number requested| / scale of estimates, with scale of estimates being 100 or 1000.

Mathematical Achievement. Children completed the Applied Problems subtest of the Woodcock-Johnson III (Woodcock, McGrew, and Mather, 2001). This subtest requires children to answer math word problems of increasing difficulty. Subsequent analyses examined students' W scores, a transformation of raw scores into a Rasch-scaled score of equal interval measurements that represents the child' ability and the task difficulty, which is recommended to measure individual growth. A one-point W score increase roughly represents approximately a half month of learning during a school year. A score of 500 is the approximate average performance of a 10-year-old.

Results

Descriptive Statistics

There was variability in children's performance on all tasks at all time points (see Table 1). During 1st grade fall, math anxiety on average was intermediate, about half-way between low math anxiety and high math anxiety (M=2.55, *SD*=0.72) and ranged from about 2 standard deviations below to 3 standard deviations above the mean. Performance on Applied Problems (M=457.87, *SD*=17.47) ranged from about 4 standard deviations below to 3 standard deviations above the mean. Performance on the 0-100 number line task, as assessed by linear R^2 (M=0.74, *SD*=0.21) was significantly higher than performance on the 0-1000 number line task (M=0.58, *SD*=0.19); t(161)=8.6, *p=<0.001*. The linearity of children's number line estimation on the two scales at various time points is similar to that reported in other studies (e.g., Booth & Siegler, 2006; Opfer & Siegler, 2007; Siegler & Booth, 2004; Siegler & Opfer, 2003).

Children who completed the 0-100 number line task first performed better on that scale (M=0.81, *SD*=0.14) than children who completed the 0-1000 number line task first (M=0.68, *SD*=0.024); t(160)=-4.31, *p=<0.001*). Similarly, children who completed the 0-1000 number line task first performed marginally better on that scale (M=0.61, *SD*=0.17) than children who completed the 0-100 number line task first (M=0.55, *SD*=0.21); t(160)=1.97, *p=0.051*).

Table 1. Descriptive statistics for variables at all time points.

Measures	1st fall M (SD), Range (N)	1st spring M (SD), Range (N)	2nd fall M (SD), Range (N)	2nd spring M (SD), Range (N)	3rd fall M (SD), Range (N)
MA	2.6 (0.7), 1-4.8 (162)	2.3 (.78), 1.-4.4 (150)	2.2 (.8), 1-4.3(132)	1.9 (.8), 1-5 (126)	2.1 (.8), 1.1-5 (104)
0-100	.7 (0.21), .1-1 (162)	.9 (.16), .1-1 (150)	.9 (.2), .2-1 (132)	.9 (.1), .2-1 (127)	.9 (.1), .7-1 (105)
0-1000	.6 (0.19), .02-1 (162)	.7 (.16), .2-1 (150)	.7 (.2), .02-1 (132)	.8 (.2), .5-1 (127)	.8 (.2), .3-1 (105)
AP	458 (17), 393-507 (162)	473 (21), 428-526 (149)	477 (18), 427-526 (128)	493 (19), 435-534 (125)	494 (21), 436-539 (104)

MA=math anxiety, 0-100=Linear R^2 (0-100), 0-100=Linear R^2 (0-1000), AP=Applied Problems (W Score)

Correlations

Correlations between measures during 1st grade fall are reported in Table 2. Math anxiety was negatively correlated with performance on the 0-100 number line task but not the 0-1000 number line task. Performance on Applied Problems was correlated with performance on both number line tasks, and the scores on the two number line tasks were correlated.

As shown in Table 3, math anxiety in 1st grade fall was positively correlated with math anxiety at all later time points. Similarly, number line estimation on the 0-100 scale in 1st grade fall was positively correlated with number line estimation on both the 0-100 and 0-1000 scales at all later time point. In contrast, 0-1000 number line estimation in the fall of 1st grade was not correlated with later 0-100 number line estimation except at one time point (2nd grade spring) but was correlated with 0-1000 number line estimation at all later time points. Of note, within each time point, linearity and PAE were highly correlated on both the 0-100 scale (*r*=-0.695 to -0.79), and the 0-1000 scale (*r*=-0.774 to -0.872).

Table 2. Pearson correlations for variables in 1st grade fall (N=162).

1st grade fall measures	1.	2.	3.
1. Math anxiety			
2. Lin R^2 (0-100)	-.163*		
3. Lin R^2 (0-1000)	-.049	.249**	
4. AP (W Score)	-.355**	.316**	.327**

*$p < 0.05$ **$p < 0.01$
AP=Applied Problems (W Score)

Table 3. Pearson correlations between variables at initial time point (1st fall) and later time points (1st spring through 3rd fall).

1st fall measure	1st spring	2nd fall	2nd spring	3rd fall
Math anxiety	.489**	.449**	.311**	.491**
Lin R^2 (0-100) correlation with Lin R^2 (0-100)	.220**	.358**	242**	.338**
Lin R^2 (0-100) correlation with Lin R^2 (0-1000)	.324**	.203**	.345**	.360**
Lin R^2 (0-1000) correlation with Lin R^2 (0-100)	.145	.072	.212*	.149
Lin R^2 (0-1000) correlation with Lin R^2 (0-1000)	.336**	.206*	.207*	.247*

*$p < 0.05$ **$p < 0.01$

Does 1st Grade Fall Math Anxiety Predict Future Math Achievement, Controlling for Number Line Estimation?

In these subsequent analyses examining whether fall math anxiety predicts later math achievement controlling for number line estimation, we used the Hierarchical Linear Modeling (HLM) program (Raudenbush et al., 2013) to account for the nested nature of the data (i.e., time within children) and the maximum likelihood methods in the program to account for missing data. Math anxiety and number line estimation were z-scored and kept as continuous variables. Order of administration of the two number line tasks was analyzed as a centered contrast and coded -1 for order A (0-1000 scale was first) and +1 for order B (0-100 scale was first). Time was coded as 0-4 with 0 being the first time point (1st grade fall) and 4 being the last time point (3rd grade fall).

We also wanted to control for initial performance on the Applied Problems subtest, to ensure that any observed association between initial math anxiety and future Applied Problems would not be explained by the association between initial math anxiety and initial Applied Problems. Because initial performance on the Applied Problems subtest was highly correlated with initial math anxiety and initial number line estimation (0-100 and 0-1000, see Table 2), we controlled for shared variance by regressing Applied Problems on math anxiety and number line estimation (0-100 and 0-100) and saved the residuals. This approach has been used to deal with overlapping variance issues (Durik et al., 2015). These Applied Problems residuals capture the variance in Applied Problems that does not overlap with math anxiety or number line estimation in 1st grade fall. Applied Problems residuals were z-scored and used as a continuous variable.

In model 1, we examined whether math anxiety in 1st grade fall predicted future Applied Problems scores, controlling for number line estimation (0-100 and 0-1000 scale) in 1st grade fall, and Applied Problems residuals in 1st grade fall. Because the order in which children completed the number line tasks influenced their number line estimation (i.e., 0-100 or 0-1000 scale first), we also controlled for order of the number line tasks in model 2. In model 3, we examined whether there was a time x math anxiety interaction effect, a time x number line estimation interaction effect, and a time x Applied Problems residuals effect on future Applied Problems, to test whether these relations changed over time. The results for models 1-3 are reported in Table 4.

In models 1-3, math anxiety negatively predicted future Applied Problems, controlling for number line estimation on the 0-100 scale, number line estimation on the 0-1000 scale, time, and Applied Problems residuals, which were also significant predictors. In models 2-3, the order in which students completed the number line tasks (0-100 or 0-1000 first) did not predict future

Applied Problems performance. In model 3, time x math anxiety, time x number line estimation on the 0-100 scale, time x number line estimation on the 0-1000 scale did not significantly predict future Applied Problems.

In sum, children with higher math anxiety in 1st grade fall had lower scores on future Applied Problems, compared to children with lower math anxiety (see Figure 2). Additionally, children with more linear number line representations in 1st grade fall had higher scores on future Applied Problems, compared to children with less linear number representations (see Figure 3).

Table 4. Hierarchical linear models predicting future Applied Problems (AP) W score over four time points from 1st spring through 3rd fall (N=162).

Predictors	Model 1	Model 2	Model 3
	ß (S.E. ß), *p*	ß (S.E. ß), *p*	ß (S.E. ß), *p*
Math anxiety 1st fall (z-score)	-5.50*** (0.84), *<0.001*	-5.51*** (0.83), *<0.001*	-5.79*** (0.93), *<0.001*
Lin R^2 (0-100) 1st fall (z-score)	4.90 ** (0.79), *<0.001*	4.93*** (0.86), *<0.001*	3.38** (1.24), *0.007*
Lin R^2 (0-1000) 1st fall (z-score)	4.32 *** (0.88), *<0.001*	4.30*** (0.91), *<0.001*	5.32*** (0.96), *<0.001*
AP standardized residuals 1st fall (z-score)	11.06*** (0.79), *<0.001*	11.06*** (0.79), *<0.001*	11.62*** (0.99), *<0.001*
Time (4 time points)	7.86*** (0.43), *<0.001*	7.86*** (0.43), *<0.001*	7.85*** (0.42), *<0.001*
Order 1st fall (100s or 1000s first)		-0.07 (0.90), *0.934*	-0.07 (0.91), *0.931*
Time x math anxiety 1st fall			0.21 (0.47), *0.652*
Time x Lin R^2 (0-100) 1st fall			1.25 (0.88), *0.155*
Time x Lin R^2 (0-1000) 1st fall			-0.80 (0.46), *0.086*
Time x AP standardized residuals 1st fall			-0.35 (0.40), *0.380*

$^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$

AP=Applied Problems (W Score)

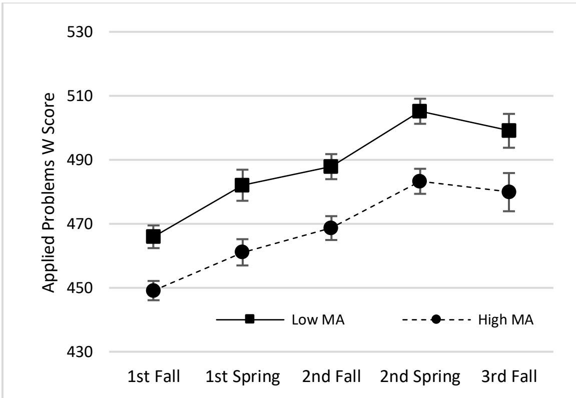
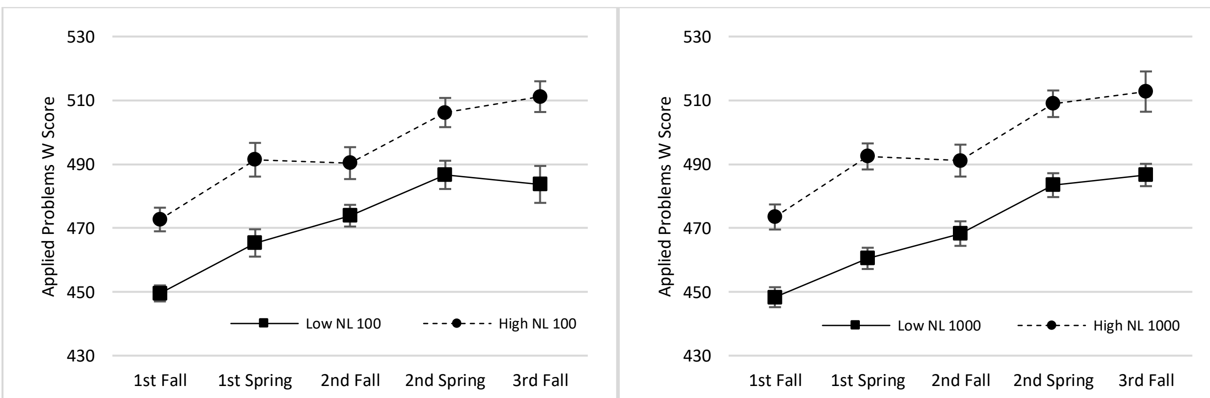


Figure 2. Math anxiety from 1st grade fall predicting Applied Problems W-Score over time. Math anxiety is plotted at 1 SD below the mean and 1 SD above the mean for visual purposes. This relation remained significant controlling for 1st grade number line estimation (0-100 and 0-100) as well as other factors (see Table 2).



a. **b.**

Figure 3. Number line estimation in 1st grade fall predicting Applied Problems W-Score over time. Number line estimation is plotted at 1 SD below the mean and 1 SD above the mean for visual purposes.

We also examined whether math anxiety in 1st grade fall explained variation in future Applied Problems performance, over and above number line estimation (0-100, 0-1000) in 1st grade fall, after covarying out Applied Problems residuals from 1st grade fall. We ran three sets of four regression models with Applied Problems residuals predicting future Applied Problems at each of the four future time points (see Table 5). Initial Applied Problems residuals accounted for 30.1% to 35.1% of the variation in future Applied Problems performance. Next, we added initial number line estimation (0-100 and 0-1000) as predictors to each regression model. After covarying out Applied Problems residuals, number line estimation accounted for an additional 13.2% to 23.2% of the variation in future Applied Problems. Finally, we added math anxiety as a predictor to each regression model, which accounted for an additional 7.1% to 8.9% of the variation in future Applied Problems performance.

Table 5. Adding (a) number line estimation (0-100 and 0-1000) from 1st grade fall, and then (b) math anxiety from 1st grade fall to regression models with Applied Problems residuals from 1st grade fall predicting future Applied Problems W-scores.

Future AP W Score	AP standardized residuals 1st fall	Lin R^2 (0-100 & 0-1000) 1st fall		Math Anxiety 1st fall	
	R^2	R^2 change	Sig. F. change	R^2 change	Sig. F. change
1st spring	0.351	0.162***	*<0.001*	0.082***	*<0.001*
2nd fall	0.337	0.154***	*<0.001*	0.089***	*<0.001*
2nd spring	0.313	0.132***	*<0.001*	0.079***	*<0.001*
3rd fall	0.301	0.232***	*<0.001*	0.071***	*<0.001*

***$p < 0.001$

AP=Applied Problems (W Score)

Does the Relation of Math Anxiety to Number Line Estimation Change Over Time, Depending on the Scale?

Similar to our previous analyses, in these subsequent analyses, we used the HLM program (Raudenbush et al., 2013) and the maximum likelihood methods in the program. Math anxiety was z-scored and kept as a continuous variable. Number line scale was analyzed as a centered contrast and coded -1 for the 0-100 scale and +1 for the 0-1000 scale. Similarly, order of administration of the number line tasks was analyzed as a centered contrast and coded -1 for order A (0-1000 scale was first) and +1 for order B (0-100 scale was first). Time was coded as 0-4 with 0 being the first time point (1st grade fall) and 4 being the last time point (3rd grade fall). Since possible scores for our outcome measure of linear R^2 range from 0-1, for ease of interpretation Beta and standard error values were multiplied by 100.

In model 1, we examined whether math anxiety in 1st grade fall predicted number line estimation over time and tested the following interactions: math anxiety x time, math anxiety x scale, time x scale, and math anxiety x time x scale. In model 2, we controlled for number line order. The results for models 1-2 are reported in Table 6. In models 1-2, there was a significant math anxiety x time x scale interaction effect on number line estimation, such that the relation between math anxiety and number line estimation changed over time depending on the scale. Additionally, math anxiety, time, and number line scale significantly predicted number line estimation. Math anxiety x time, math anxiety x scale, and time x scale did not predict number line estimation. In model 2, number line order significantly predicted number line estimation.

Table 6. Hierarchical Linear Models predicting number line estimation over time (N=162).

Predictors of number line estimation

	Model 1	Model 2
	ß (S.E. ß), *p*	ß (S.E. ß), *p*
Math anxiety 1st fall (z-score)	-2.7007* (1.1361), *0.019*	-2.4559* (1.1245), *0.030*
Time (5 time points)	4.9974*** (0.2861), *<0.001*	4.9982*** (0.2856), *<0.001*
Scale (0-100 or 0-1000)	-9.4063*** (0.7463), *<0.001*	-9.4063*** (0.7463), *<0.001*
Math anxiety x time	-0.0017 (0.2901), *0.995*	0.0022 (0.2902), *0.994*
Math anxiety x scale	1.0698 (0.7832), *0.172*	0.1967 (0.7832), *0.172*
Time x scale	0.1967 (0.2904), *0.498*	0.1967 (0.2904), *0.498*
Math anxiety x time x scale	-0.8491** (0.3089), *0.006*	-0.8491** (0.3089), *0.006*
Order 1st fall (100s or 1000s first)		1.9143* (0.7759), 0.015

$^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$

Note: ß and S.E. values were multiplied by 100 (since linear R^2 scores range from 0-1) for ease of interpretation.

To unpack the three-way interaction, we examined how math anxiety related to number line estimation on the 0-100 scale specifically and on the 0-1000 scale specifically (see Tables 7 and 8). Overall, math anxiety significantly predicted number line estimation on the 0-100 scale, but not the 0-1000 scale**.** However, there was a significant effect of time on number line estimation for both the 0-100 scale and the 0-1000 scale, and importantly, there was a significant math anxiety x time interaction effect on number line estimation for both the 0-100 scale and the 0-1000 scale. For a visual representation of the relation between math anxiety and number line estimation (0-100 and 0-1000) over time see Figure 4.

Table 7. Hierarchical Linear Models predicting number line estimation (0-100) over time (N=162).

Predictors of number line estimation

	Model 1	Model 2
	ß (S.E. ß), *p*	ß (S.E. ß), *p*
Math anxiety 1st fall (z-score)	-3.7803* (1.5460), *0.016*	-3.3215* (1.4569), *0.024*
Time (5 time points)	4.7817*** (0.3466), *<0.001*	4.7902*** (0.3468), *<0.001*
Math anxiety x time	0.8510* (0.4178), *0.042*	0.8546* (0.4205), *0.043*
Order 1st fall (100s or 1000s first)		3.5091*** (0.7533), *<0.001*

$^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$

Note: ß and S.E. values were multiplied by 100 (since linear R^2 scores range from 0-1) for ease of interpretation.

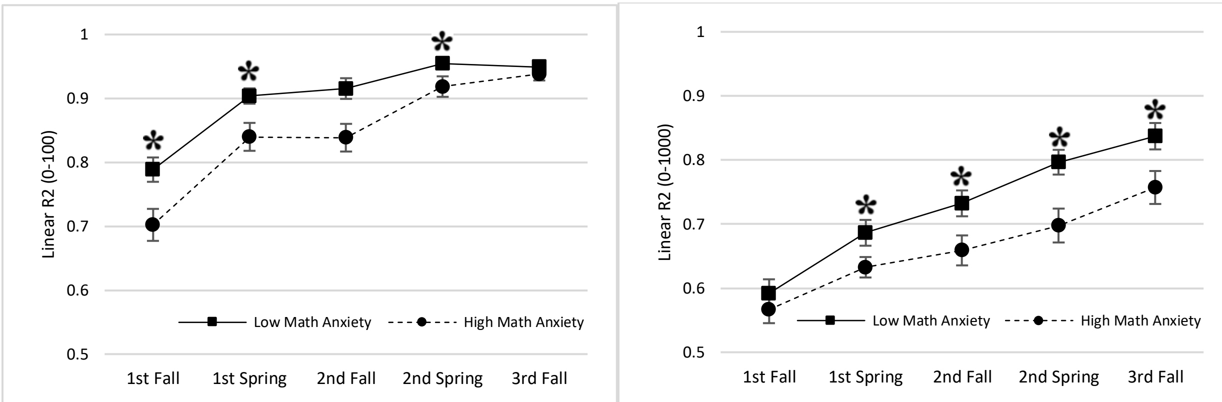
Table 8. Hierarchical Linear Models predicting number line estimation (0-1000) over time (N=162).

Predictors of Math Achievement (from 1st fall)

	Model 1	Model 2
	ß (S.E. ß), *p*	ß (S.E. ß), *p*
Math anxiety 1st fall (z-score)	-1.5996 (1.2002), *0.185*	-1.5742 (1.2061), *0.194*
Time (5 time points)	5.2479*** (0.4669), *<0.001*	5.2479*** (0.4668), *<0.001*
Math anxiety x time	-0.8822* (0.4398), *0.045*	-0.8815* (0.4400), *0.046*
Order 1st fall (100s or 1000s first)		0.1997 (1.0031), *0.842*

$^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$

Note: ß and S.E. values were multiplied by 100 (since linear R^2 scores range from 0-1) for ease of interpretation.



a. ***b.***

Figure 4. Math anxiety from 1st grade fall predicting number line estimation on the 0-100 scale (Figure 3a) and 0-1000 scale (Figure 3b) over time. Math anxiety is plotted at 1 SD below the mean and 1 SD above the mean for visual purposes, but it is analyzed as a continuous variable in our HLM analyses. *Significant relation (p<.05) between children's math anxiety and their number line estimation on a 0-100 or 0-1000 scale at a particular time point

To further test our hypothesis about math anxiety being most detrimental to math tasks at the cusp of the child's ability, we examined the relation between math anxiety and number line estimation on both scales (0-100 and 0-1000) during each of the five time points (see Table 9). Math anxiety in 1st grade fall negatively related to 0-100 number line estimation at the earlier time points (from 1st grade fall through 2nd grade spring, with the exception of 2nd grade fall) but not during the last time point (3rd grade fall). In contrast, math anxiety in 1st grade fall did not relate to 0-1000 number line estimation linearity at the fall 1st grade time point but did at all the later time points.[5]

Although not the focus of this study, exploratory analyses showed no gender differences in math anxiety, math achievement, or number line estimation, and no gender differences in the relation of math anxiety to math achievement or to number line estimation.

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[5] Results were similar with PAE as the index of number line estimation, with a few exceptions. First, in model 3 of Table 4 PAE on the 0-100 scale marginally predicted (p=-0.051) future Applied Problems. Second, although the critical three-way interaction effect of math anxiety x time x scale on PAE was significant in Table 6, in models 1-2 of Table 8 math anxiety significantly predicted PAE on the 0-1000 scale, and there was not a significant math anxiety x time interaction effect on PAE on the 0-1000 scale (p=0.160 to 0.175). Third, in table 9 math anxiety marginally predicted PAE on a 0-100 scale in 1st grade spring (p=0.056) and did not significantly predict PAE on a 0-100 scale in 2nd grade spring.

Table 9. Pearson correlations between 1st grade math anxiety and number line estimation at all time points.

	1st fall	1st spring	2nd fall	2nd spring	3rd fall
Linear R^2 (0-100)	-.163*	-.224**	-.123	-.174*	-.045
Linear R^2 (0-1000)	-.049	-.186*	-.217*	-.270**	-.216*

$^{*}p < .05$ $^{**}p < .01$

Discussion

Regarding our first question, children's early math anxiety from 1st grade fall predicted their future math achievement up to two years later when they were in the fall of 3rd grade, and this relation remained significant controlling for their early number line estimation—a foundational cognitive math skill. To our knowledge, the present study is the first to control for a foundational numerical representation in examining the relation between children's early math anxiety and their future math achievement over the course of the early elementary school years. Our findings contradict those of Skagerlund et al. (2019), in which adults' math anxiety indirectly related to arithmetic through symbolic number comparison. Further, regardless of whether math anxiety stems from poor math skills, math anxiety leads to low math skills, or this relation is bidirectional, our findings suggest that it is important to consider both foundational math skills and emotional factors—in particular, math anxiety—in supporting children's math learning and achievement. In other words, there is more to children's math learning than their math skills alone; and fostering positive emotions toward math, in addition to math skills, is likely to be an effective approach to promoting strong math achievement.

Regarding our second question, math anxiety was more predictive of number line performance when the number line task was appropriately complex (i.e., not too easy or too difficult) as shown by the significant three-way interaction between math anxiety, time, and scale on number line estimation. In particular, math anxiety in 1st grade was associated with number line estimation on the 0-100 scale in 1st and 2nd grade, but as this task became easy for children in 3rd grade, math anxiety no longer predicted performance. In contrast, math anxiety in 1st grade was associated with number line estimation on the 0-1000 scale from 1st grade spring through 3rd grade fall when this task was challenging but not too difficult, but not during 1st grade fall when the task was likely too difficult regardless of children's math anxiety. Number line estimation was an ideal task to capture this developmental trajectory, as the structure of the task remained the same but the complexity of the task changed when the rightmost anchor was larger. Our results suggest that math anxiety was particularly harmful when the number line estimation task was not too easy and not too difficult. This finding suggests that perhaps math anxiety plays a reverberating role in children's math learning in school—a place where children are consistently introduced to new concepts and skills that are at the cusp of their learning level.

One limitation of the current study is that we did not test whether the relation of math anxiety to number line estimation varied based on individual children's math skills. For example, while number line estimation on the 0-1000 task is generally very difficult for 1st graders, for some 1st graders it may have been easier, and for others it may have been especially difficult. Choe et al. (2019) showed that math anxiety positively predicted adults' avoidance of difficult math tasks, and these tasks were calibrated to individuals' own math skills. Future research could use a similar method with young children to test whether the relation of math anxiety to math performance differs based on how difficult the math task is based on children's own math skills.

Another limitation of our study is its correlational design, which impedes us from making causal claims. Further, we show that math anxiety is a significant predictor of future math achievement controlling for an important foundational number representation (linearity of number line estimation), but we do not know what the relation between math anxiety and later math achievement would be controlling for additional foundational math skills (e.g., magnitude comparison). Nonetheless, the robust relation between number line estimation and math competence provides evidence that math anxiety matters for children's math achievement, over and above this important foundational numerical representation.

Further research should examine whether intervening on math anxiety alone, foundational math skills alone, or both math anxiety and foundational math skills simultaneously would most positively influence children's short-term and long-term math achievement. While some interventions have focused on improving number line estimation (Siegler & Ramani, 2009), and others have targeted the negative effects of anxiety on math performance (Ramirez & Beilock, 2011; Rozek et al., 2019), intervening on both of these predictors simultaneously might have a greater impact on math achievement. Further, having educators, as opposed to researchers administer these interventions might prove most fruitful and be a more sustainable way to foster children's positive feelings about math. Future research should examine whether there is a particular age at which math anxiety interventions might be most beneficial, and whether administering these interventions at one time point or administering them across multiple time points might differentially impact math performance.

In conclusion, our findings support the theory that math anxiety does not simply stem from poor math ability, but rather is separable from math ability as it predicts math achievement over and above a foundational math skill—number line performance. Furthermore, math anxiety appears to be most predictive of math performance on tasks that are appropriately complex (i.e., tasks that are at the cusp of children's knowledge), suggesting that math anxiety can have reverberating effects as children encounter new and challenging math tasks. Taken together, our findings suggest that children’s math achievement depends not only on fostering foundational cognitive skills, but also on fostering their positive emotions toward math.

Because findings from this study suggest that math anxiety can be harmful for math achievement as early as 1st grade, in the next study I focus on how the math achievement-attitude relation emerges at the start of formal schooling, focusing on both math anxiety and math self-concept. I also examine the role of key socializers (parents), as young children spend a lot of time with their parents, who have been shown to play an important role in children’s early math skills (Levine & Pantoja, 2021; Levine et al., 2010; Berkowitz et al., 2015). Further, I focus on families from low SES backgrounds, who on average have lower math achievement than their peers from higher SES backgrounds as early as the start of kindergarten (Dearing et al., in prep; Jordan & Levine, 2009; Reardon, 2011, 2021). It is important to improve our understanding of the factors that relate to children’s early math skills, as this information can inform the development of interventions that aim to improve early math achievement and math attitudes.

STUDY 2: CHILD MATH ACHIEVEMENT: RELATION TO CHILD AND PARENT MATH ATTITUDES IN FAMILIES FROM LOW SES BACKGROUNDS

Negative self-relevant math attitudes—attitudes that reflect how one thinks and feels about their own relationship with math—can have long-term consequences, as they predict adolescents' and adults' career choices (Wigfield & Eccles, 2020). Negative parent math attitudes can also have long-term consequences, as they predict adolescents' future high school courses and career choices (Bleeker & Jacobs, 2004; Wigfield et al., 2015). Self-relevant math attitudes and parent math attitudes are predictive of math achievement among children and adolescents (Eccles & Wigfield; Levine & Pantoja, 2021). Thus, improving our understanding of how robust these relations are and how they emerge is essential. The current study is unique, as we focus on three important features: 1) multiple child and parent math attitudes, 2) bidirectional child math achievement-attitude relations, and 3) families from low SES backgrounds. We elaborate on these three features below.

First, prior studies examining parent and child math attitudes typically focus on one math attitude at a time, making it difficult to determine whether their relation to math achievement holds controlling for other math attitudes. We focus on child math anxiety and math self-concept, two self-relevant math attitudes that have been consistently shown to predict math achievement by 1st grade (Ching et al., 2020; Dapp & Roebers, 2018; Dowker et al., 2019; Jameson, 2013; Pantoja et al., 2020; Ramirez et al., 2013, 2016; Vukovic et al., 2013; Valeski & Stipek, 2001; Wu et al., 2012, 2014). We focus on parent math anxiety (a self-relevant math attitude) and parent math expectancy-value (i.e., parents' expectations and value of their child's math achievement; a child-specific math attitude). These parent math attitudes also predict child math achievement by 1st grade (Berkowitz et al., 2015; Fredricks & Eccles, 2002; Schaeffer et al., 2018; Wigfield et al., 1997), and predict important parent math behaviors that have been shown to predict child math achievement (Berkowitz et al., 2021; Simpkins et al., 2012; Wigfield et al., 2006, 2015).

Second, prior studies with children have typically examined how math attitudes predict math achievement, and not the reverse. Because self-relevant math attitudes become more negative over time (Hembree, 1990; Jacobs et al., 2002; Wigfield et al., 1997), it is important to understand how the math achievement-attitude relation emerges. For example, does math anxiety initially undermine math achievement, or does low math achievement—likely linked to negative math experiences—initially lead to math anxiety? Math achievement-related experiences might include the classroom learning environment (e.g., an emphasis on performance and comparisons to peers) and messages conveyed by parents and teachers. Early elementary school is an important time to examine the emergence of math achievement-attitude relations, as children are likely beginning to develop positive or negative math attitudes at the start of formal schooling. Further, it may be easier to break the math achievement-attitude link when children are younger, and their math achievement has not been compromised over many years.

Third, prior research has focused on families from middle to higher SES backgrounds. We focus on children from lower SES backgrounds, who compared to their peers from higher SES backgrounds, tend to have lower math achievement (Jordan et al., 2009; Larson et al., 2015; Reardon, 2011). The SES math achievement gap has widened by about 25%, at least between cohorts assessed in 1970 and those assessed in 2000, suggesting that children's math experiences and opportunities at home and in school have become more unequal in recent years (Reardon, 2011, 2021). Children from low SES backgrounds have the most to gain from information on how to improve their math achievement. Yet, we know little about the role of young children's own and their parents' math attitudes in their math achievement. Among adolescents from lower SES backgrounds, math anxiety and math self-concept are less predictive of math achievement (OECD, 2013). Whether this is the case for young children is unclear. If child and parent math attitudes are less predictive of child math achievement in families from lower SES backgrounds, that would suggest that focusing on these attitudes will not help narrow the SES math achievement gap. If child and parent math attitudes predict child math achievement across SES backgrounds, that would suggest that fostering positive child and parent math attitudes, could be one way to narrow the SES math achievement gap.

In sum, by examining longitudinal relations among child and parent math attitudes and child math achievement in families from low SES backgrounds, the current study addresses important unanswered research questions. Findings from the current study hold potential for informing interventions aimed at enhancing math outcomes among this underserved and understudied population.

Math Anxiety

Math anxiety is a feeling of fear or nervousness when one does or anticipates doing math (Hembree, 1990; Lyons & Beilock, 2012; Richardson & Suinn, 1972; Young et al., 2012). The negative relation between math anxiety and math achievement is present by 1st grade and persists over and above foundational math skills (Ching et al., 2020; Erturan & Jansen, 2015; Gunderson et al., 2018; Harari et al., 2013; Jameson, 2013; Ramirez et al., 2013, 2016; Vukovic et al., 2013; Wu et al., 2012, 2014; see Chapter 1 of this dissertation). Examining the direction of the relation between math anxiety and math achievement in young children will enhance our understanding of how the relation emerges. Below, we describe the three theories on the direction of the relation of math anxiety with math achievement.

Reciprocal Theory posits that math anxiety and math achievement influence each other, such that math anxiety results from poor math skills, and in turn cause poor math achievement through math avoidance and working memory depletion (Ashcraft et al., 2007; Carey et al., 2016). Supporting this theory, in a meta-analysis, the relation of math anxiety to later math achievement was as strong as the reverse relation in students from 1st grade to high school (Namkung et al., 2019). However, studies that have shown a reciprocal relation between math anxiety and math achievement in young children have found math achievement to predict math anxiety more strongly than the reverse (Cargnelutti et al., 2017; Gunderson et al., 2018), suggesting that negative math achievement-related experiences may initially lead to high math anxiety.

Cognitive Interference Theory posits that math anxiety influences math achievement through anticipation and avoidance of math, and disruption of working memory resources (Carey et al., 2016). Evidence supporting this theory comes from intervention studies with adolescents and adults, in which writing about worries before a math test reduces the negative effect of math anxiety on math achievement (Jamieson et al., 2010, 2016; Park et al., 2014; Ramirez & Beilock, 2011). Given that math anxiety eventually has a detrimental effect on math achievement, understanding how to break this link before math anxiety compromises math achievement over many years is essential.

Deficit Theory posits that poor math achievement and negative memories of math lead to higher math anxiety (e.g., Carey et al., 2016; Ramirez et al., 2018). Evidence supporting this theory comes from correlational studies of adolescents and young children. For example, in a longitudinal study of 7th through 12th graders, early math achievement consistently predicted later math anxiety, while the reverse relation was much weaker (Ma & Xu, 2004). Similar findings are reported in studies across one school year with adolescents (Geary et al., 2019; Wang et al., 2020) and 1st grade children (Ching et al., 2020). Additional evidence comes from experiments, in which young children receive math tutoring. For example, for 3rd grade children with high math anxiety, intensive math tutoring improved math learning and reduced math anxiety (Supekar et al., 2015). Thus, existing evidence suggests that in young children, math achievement or related experiences initially influence math anxiety.

These theories were developed to explain the relation of math anxiety and math achievement over time. Levine and Pantoja (2021) posit that when the relation between math attitudes and math achievement emerges in young children, math achievement-related experiences influence math anxiety. They emphasize that additional research should test this theory and should examine these relations in families from diverse backgrounds. Levine and Pantoja (2021) also emphasize that research should account for more than one math attitude at a time, which is why we examine another self-relevant math attitude: math self-concept.

Math Self-Concept

Math self-concept refers to one's self-perception or beliefs about their *current* competence in math (Eccles et al., 1993; Eccles & Wigfield, 2002). Measures of self-concept typically also assess one's *expectations* of future math achievement and their perception of math task *difficulty*, as these constructs are empirically indistinguishable from one's perception of their current math competence (see Levine & Pantoja, 2021). The relation of math self-concept with math achievement is present as early as 1st grade (Dapp & Roebers, 2018; Dowker et al., 2019; Herbert & Stipek, 2005; Valeski & Stipek, 2001), and as early as 6 years old in German children, before the start of formal schooling (Arens et al., 2016; Marsh et al., 2002). Similar to math anxiety, there are three theories on the direction of the relation between math self-concept and math achievement: the relation is reciprocal, math self-concept influences math achievement, and math achievement influences math self-concept (Eccles & Wigfield, 2020).

Findings from a meta-analysis show that the relation between math self-concept and math achievement becomes stronger from elementary to high school (Ma & Kishor, 1997). Thus, we need to understand how this relation emerges in young children, before it has become much stronger in adolescence. Studies of adolescents suggest a reciprocal relation, as math self-concept predicts math achievement as strongly or more strongly than the reverse (Arens et al., 2017; Marsh et al., 2005; Möller et al., 2011; Pinxten et al., 2014; Sewasew et al., 2018).

Situated Expectancy-Value Theory posits that previous math achievement-related experiences influence one's *interpretation* of these experiences, which influence affective reactions and memories of math, which then influence math self-concept (Eccles & Wigfield, 2020). Indeed, findings from studies of young children suggest that math achievement may initially influence math self-concept. Studies of preschool, elementary, and middle school students find that math achievement predicts math self-concept more strongly than the reverse (Arens et al., 2016; Ganley & Lubienski, 2016; Helmke & van Aken, 1995). For example, in a study of elementary and middle school students, math achievement predicted math self-concept, while the reverse relation was much weaker (Ganley & Lubienski, 2016). A study of 5- to 6-year-olds in Germany found similar results of math achievement predicting math self-concept more strongly than the reverse (Arens et al., 2016). Thus, similar to findings of math anxiety, there is some suggestion that when the relation emerges, math achievement-related experiences first influence math self-concept. Next, we discuss what is known about how math self-concept and math anxiety relate to each other.

Math Anxiety and Math Self-Concept

Self-relevant math attitudes are likely linked. One possibility is that high math anxiety could initially leads to lower math self-concept. A second possibility is that lower math self-concept initially leads to higher math anxiety. A third possibility is that when the relation emerges, math anxiety and math self-concept influence each other. Studies with adolescents find a reciprocal relation, with math self-concept predicting math anxiety more strongly than the reverse (Ahmed et al., 2012; Wang et al., 2020). Thus, at least eventually, self-relevant math attitudes likely influence each other.

Studies of young children show that math anxiety and math self-concept are concurrently related by 2nd grade (Jameson, 2014; Justicia-Galiano et al., 2017; Kaskens et al., 2020). However, to our knowledge, research has not examined this relation longitudinally in young children. Understanding whether one of these self-relevant math attitudes influences the other, could inform decisions on whether interventions should focus on one or both of these attitudes in young children. The current study is the first, to our knowledge, to examine the longitudinal relation of math anxiety and math self-concept in young children from low SES backgrounds.

Parent Math Anxiety

Parent math anxiety predicts adolescents' math achievement and math anxiety (Casad et al., 2015; Soni & Kumari, 2017), as well as young children's math achievement (Berkowitz et al., 2015; Schaeffer et al., 2018). The link between parent math anxiety and child math achievement can be broken if parents with high levels of math anxiety are provided with tools that encourage positive math interactions with their children (Berkowitz et al., 2015; Schaffer et al., 2018). These findings suggest that the relation of parent math anxiety to child math achievement is likely connected to differences in parent math behaviors.

Adult math anxiety predicts their math avoidance (Choe et al., 2019). Findings from recent studies suggest that the math anxiety and math avoidance link extends to the quantity and quality of parent math engagement with their toddlers and preschoolers (Berkowitz et al., 2021; del Río et al., 2017). In turn, parent math engagement, including the quantity and quality of number talk, predicts child math achievement (Casey et al., 2018; Elliott et al., 2017; Gunderson & Levine, 2011; Levine et al., 2010; Ramani et al., 2015; Susperreguy & Davis-Kean, 2016; Thippana et al., 2020). However, these relations may vary for families from different SES backgrounds. For example, Berkowitz et al. (2021) found an interaction effect between parent SES and math anxiety on parent number talk. Parents with high math anxiety from higher SES backgrounds provided less number talk, but parents from lower SES backgrounds, provided infrequent number talk regardless of their math anxiety. These findings highlight the importance of examining the role of parent math attitudes in families from diverse SES backgrounds.

Parent Math Expectancy-Value

Parent math expectancy-value is a child-specific math attitude, as it involves parents' expectations of how well their children will do in math, and how valuable they think math is for their child's future. Parent math expectancy-value is thought to be influenced by personal characteristics (e.g., aptitude and temperament), cultural milieu (e.g., family demographics and stereotypes about math), and children's math achievement (Eccles & Wigfield, 2020). Situated Expectancy-Value Theory posits that child math self-concept and math achievement are influenced by parent child-specific math attitudes (Eccles & Wigfield, 2020; Wigfield et al., 2006, 2015). The relation of parent math expectancies to child math self-concept and math achievement has been shown as early as 1st grade in families from middle to high SES backgrounds, and this relation is stronger in older children (Fredricks & Eccles, 2002; Wigfield et al., 1997). Parent math expectancies can have long-term effects, as in studies of families from middle to higher SES backgrounds, they predict future high school courses and career choices (Bleeker & Jacobs, 2004; Wigfield et al., 2015). Given findings, discussed above, suggesting that there are SES differences in the role of parent math anxiety, it is important to examine the role of parent math expectancy-value in young children from low SES backgrounds.

The Current Study

Our study is the first, to our knowledge, to examine longitudinal relations between parent math attitudes, child math attitudes and child math achievement in families from low SES backgrounds. We assessed child math anxiety, math self-concept, and math achievement at the beginning (fall) and end (spring) of 1st grade to better understand how relations between these variables emerge. We assessed parent math expectancy-value and math anxiety at the beginning (fall) of 1st grade to better understand how these parent math attitudes relate to each other, and how they predict child self-relevant math attitudes and math achievement.

The current study examines longitudinal relations between child self-relevant math attitudes, child math achievement, and parent math attitudes in 1st grade children from low SES backgrounds. Specifically, we ask: 1) Do children's early self-relevant math attitudes predict later math achievement? 2) Does children's early math achievement predict later self-relevant math attitudes? 3) Do parent math attitudes predict child math achievement and self-relevant math attitudes? Our primary hypothesis was that child math achievement would predict child self-relevant math attitudes more strongly than the reverse relation. Our secondary hypothesis was that parent math expectancy-value would predict child math achievement and self-relevant math attitudes. Additionally, we expected a longitudinal relation between child math anxiety and math self-concept but were agnostic about the direction of the relation. As an exploratory question, we also compare child and parent math attitudes between participants from this study (from low SES backgrounds), and participants from Study 1 (from higher SES backgrounds).

Method

Participants

The data analyzed in the current study were collected as part of a large longitudinal randomized control trial, examining the effectiveness of a math app that parents and children engaged with to support children's math learning. Primary caregivers and their 1st graders were recruited and randomly assigned to an intervention (math) or control (reading) condition. Because there were no condition effects on any of the measures analyzed in the current study (see Table 10), we examined data from participants across conditions, and controlled for condition as a precaution. Our study included 484 1st grade children (232 girls; 238 in the control condition) from Chicago schools. Primary caregivers (415 mothers, 37 fathers, 13 grandparents and 8 other), referred to as parents for simplicity, reported an average household income of $29,800 (SD=$27,364), and reported that 83.6% of children were African American or Black, 3.7% were Hispanic or Latino/a, 0.6% were White, and 11.9% were another race or two or more races. Note that on average, families were just above the poverty line for a family of four in Chicago ($24,858, U.S. Fontenot et al., 2018).

An additional seven children and their primary caregivers were initially assessed but were excluded because children had a sibling participating in the study and only one of them was randomly chosen to be included in analyses. Children and parents were included in all analyses for which they had relevant data. Missing data occurred due to parents not completing individual questionnaires, children having problems completing individual tasks, experimenter error, or because children transferred schools between fall and spring (see sample sizes in Table 1). Our path analyses used full information maximum likelihood estimation, which includes all available data to estimate model parameters, an unbiased and efficient method to deal with missing data (Enders & Bandolos, 2001; Muthén & Muthén, 1998-2017).

Procedure

We contacted schools in the Chicago area that predominantly served children that qualified for free or reduced-price lunch. Parents of 1st graders from participating schools who were interested in participating, signed permission forms and completed questionnaires at the beginning of the school year. Child measures were administered one-on-one by an experimenter at the beginning (fall) and end (spring) of 1st grade. Below we describe the subset of measures analyzed in the current study.[6]

Child Measures

Math Anxiety. Children completed the revised Child Math Anxiety Questionnaire (CMAQ-R: Ramirez et al., 2016). The 16-item measure asked children how nervous they would feel during various math-related situations, such as solving 34-7, or taking a math test. To respond, children pointed to one of five smiley faces displaying an emotional gradient from "not nervous at all" to "very very nervous". Math anxiety was scored on a scale of one (low math anxiety) to five (high math anxiety). Cronbach's Alpha was .76 in fall and .79 in spring.

Math Self-Concept. Children completed a commonly used math self-concept measure developed by Eccles et al. (1993), which included the following items: 1) How good at math are you; 2) If you were to list all the students in your class from the worst to the best in math, where

[6] We included several other child and parent measures that were not analyzed for the current study. Child measures not analyzed in the current study include: WJ Math Fluency, WJ Picture-Vocabulary, Number Line, Forward and Backward Letter Span, Visuo-Spatial Working Memory, Theories of Intelligence, School Subject Preference, Reading and Spatial Anxiety, Self-Efficacy, and Stereotype Drawing Task. Parent measures not analyzed in the current study include: Theories of Intelligence, Homework Help Frequency and Confidence, Engagement in Math Activities, Reading Anxiety, and School Subject Enjoyment.

would you put yourself; 3) Some kids are better in one subject than in another. For example, you might be better in math than in reading. Compared to most of your other school subjects, how good are you in math; 4) How well do you think you will do in math this year; 5) How good would you be at learning something new in math; 6) In general, how hard is math for you? Children responded on scale from one (low math self-concept) to seven (high math self-concept). Cronbach's Alpha for math self-concept was .51 in fall and .56 in spring, which is lower than that reported by Eccles et al. (1993) in a sample of 1st graders from middle-SES backgrounds (0.71). Although this is below the suggested level of .70 (Cronbach, 1951), studies with young children commonly report a lower alpha level (Giles & Heyman, 2003; Erdley et al., 1997; Gunderson et al., 2013; Ramirez et al., 2013). While low internal consistency does not indicate poor validity (McCrae, et al., 2011), our analysis approach accounts for measurement error, as described in the Results section.

Math Achievement. Children completed the Applied Problems subtest of the Woodcock-Johnson IV (Schrank et al., 2014). This subtest requires children to answer math word problems that increase in difficulty. We examined students' W scores, a transformation of raw scores into a Rasch-scaled score of equal interval measurements that represents ability and task difficulty. A one-point W score increase roughly represents approximately a half month of learning during a school year. The average W score of a 6-year-old and 7-year-old is 448 and 465 (McGrew et al., 2014).

Reading Achievement. As a divergent measure, children completed the Letter-Word Identification subtest of the Woodcock Johnson IV (Schrank et al., 2014). This subtest requires children to read letters and words out loud. We examined students' W scores, a transformation of raw scores into a Rasch-scaled score of equal interval measurements that represents ability and task difficulty. A one-point W score increase roughly represents approximately a quarter month of learning during a school year. The average W score of a 6-year-old and 7-year-old is 419 and 455 (McGrew et al., 2014).

Parent Measures

Math Expectancy-Value. Parents completed a questionnaire on their expectations and value of math for their child (Schaeffer et al., 2018), in which they responded to the following questions: 1) How is your child doing in math; 2) How much natural talent does your child have in math; 3) How important do you think math is for your child; 4) How well do you think your child will do in math in the future. Parents responded on a scale of one (low math expectancy-value) to five (high math expectancy-value). Cronbach's alpha was .69.

Math Anxiety. Parents' math anxiety was measured using the short-Mathematical Anxiety Rating Scale (Alexander & Martray, 1989). Parents responded to this 25-item measure on a scale of one (low math anxiety) to five (high math anxiety). Cronbach's alpha was .98.

Control Variables

In our analyses, we controlled for variables that could potentially confound relations between our variables of interest. We did not control for SES, as our sample was already a very low SES sample (see Participants section). We controlled for gender, as boys have been shown to have more positive math attitudes than girls (see Levine & Pantoja, 2021). While there were no significant differences between conditions for any of our variables (see Table 10), we controlled for condition as a precaution. We also controlled for parent math education, as a proxy for parent math knowledge. Parents with more math knowledge could potentially have lower math anxiety and higher math expectancy-value, given the math achievement-attitude link, and could potentially be more equipped to provide math support to their children. We describe our measure of parent math education below.

Math Education. Parents reported the math classes they took in high school and college if applicable. Parent math education was coded based on the highest-level of math completed: no math classes=0, high school algebra=1, high school geometry=2, high school algebra 2=3, high school calculus=4, college algebra, geometry, or algebra 2=5, college calculus=6.

Results

Descriptive Statistics

Descriptive statistics of all measures for the full sample, within condition and within gender are shown in Table 10. There were no significant differences between conditions. Girls reported higher math anxiety than boys at the end of 1st grade, $t(439)=-2.66$, $p=.008$, and there were no other statistically significant gender differences. Preliminary path analyses within condition and within gender showed similar relations among math achievement, math anxiety and math self-concept. Therefore, in our analyses, we included the full sample and treated condition and gender as covariates.

Compared to studies of children from higher SES backgrounds, children in our sample had very low math achievement but had similar math attitude levels. Child math self-concept was high, in line with findings from studies of 1st graders from higher SES backgrounds (Eccles et al., 1993), and findings that the average American regards themselves as above average, regardless of SES (Baumeister et al., 2003). While the focus of Study 1, which included children from diverse (but mostly higher) SES backgrounds, was not on parents, parent math expectancy-value and math anxiety were assessed as part of a large longitudinal study (see Berkowitz et al., 2015; Schaeffer et al., 2018). Thus, we compared parent math attitudes in Study 1 and Study 2.

Parents in Study 2 (from lower SES backgrounds) had lower math expectancy-value (M=4.12, SD=.59) than parents in Study 1 (from higher SES backgrounds; M=4.28, SD=.56; t=2.93; p=.004, d=.28). Similarly, parents in Study 2 had higher math anxiety (M=2.40, SD=1.10) than parents in Study 1 (M=2.12, SD=.76; t=3.24; p=.001, d=.26).

Table 10. Descriptive statistics for the full sample, within conditions and within gender

		Full Sample	Control Condition	Intervention Condition	Boys	Girls
	N	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**
Child Applied Problems (fall)	469	441.45 (16.16)	442.42 (15.33)	440.50 (16.91)	441.51 (16.55)	441.38 (15.80)
Child Letter-Word Identification (fall)	469	415.28 (36.36)	418.06 (35.06)	412.53 (37.46)	413.29 (37.62)	417.27 (35.01)
Child math anxiety (fall)	469	2.64 (0.76)	2.66 (0.74)	2.62 (0.79)	2.59 (0.78)	2.68 (0.75)
Child math self-concept (fall)	470	5.89 (0.98)	5.86 (0.95)	5.92 (1.00)	5.84 (1.07)	5.94 (0.87)
Child Applied Problems (spring)	440	448.25 (15.73)	448.30 (15.74)	448.20 (15.76)	448.81 (16.97)	447.7 (14.40)
Child Letter-Word Identification (spring)	441	437.76 (34.77)	440.31 (33.82)	435.12 (35.61)	436.39 (37.03)	439.14 (32.38)
Child math anxiety (spring)	441	2.43 (0.78)	2.45 (0.79)	2.41 (0.77)	**2.34***** (0.79)	**2.53***** (0.76)
Child math self-concept (spring)	442	6.03 (0.97)	6.04 (0.97)	6.01 (0.97)	6.04 (0.97)	6.01 (0.98)
Parent math expectancy-value (fall)	476	4.12 (0.59)	4.14 (0.60)	4.1 (0.58)	4.1 (0.61)	4.14 (0.57)
Parent math anxiety (fall)	482	2.40 (1.10)	2.38 (1.11)	2.41 (1.09)	2.31 (1.05)	2.48 (1.14)
Parent math education	466	4.15 (1.61)	4.15 (1.62)	4.16 (1.59)	4.12 (1.57)	4.18 (1.64)

Raw Correlations

Correlations among all measures are shown in Table 11. While raw correlations are ambiguous since we do not account for any other variables, we discuss raw relations between key variables below.

In the fall, all child variables were significantly correlated with each other, except math anxiety was not significantly correlated to math achievement or reading achievement. In the spring, all child variables were significantly correlated with each other. In addition, fall math achievement, reading achievement and math self-concept were significantly correlated with all child variables in the spring, except fall reading achievement was marginally correlated to spring math anxiety. Fall math anxiety was significantly correlated to spring math anxiety and math self-concept, but there was no evidence of a correlation to spring math achievement or reading achievement.

Parent math expectancy-value was significantly correlated with fall and spring child math achievement, reading achievement, and math self-concept. Parent math anxiety was significantly correlated with spring child math achievement and math self-concept, and marginally correlated with fall child math achievement, but was not significantly correlated to any other child variables. Parent math expectancy-value and math anxiety were not significantly correlated with each other. Parent math education was significantly correlated with parent math expectancy-value and parent math anxiety. Parent math education was also correlated with fall and spring child math achievement and reading achievement.

Table 11. Correlations among all measures

		Applied Problems (fall)	Math anxiety (fall)	Math self-concept (fall)	Letter-Word Identification (fall)	Applied Problems (spring)	Math anxiety (spring)	Math self-concept (spring)	Letter-Word Identification (spring)	Parent math expectancy-	Parent math anxiety (fall)
Math anxiety (fall)	Pearson Correl	-0.033									
	Sig.	0.472									
	N	466									
Math self-concept (fall)	Pearson Correl	.202**	-.242**								
	Sig.	<.001	<.001								
	N	466	469								
Letter-Word Identification (fall)	Pearson Correl	.533**	-0.026	.159**							
	Sig.	<.001	0.574	<.001							
	N	466	466	466							
Applied Problems (spring)	Pearson Correl	.675**	-0.007	.172**	.546**						
	Sig.	<.001	0.891	<.001	<.001						
	N	437	438	438	437						
Math anxiety (spring)	Pearson Correl	-.202**	.393**	-.184**	-0.087	-.154**					
	Sig.	<.001	<.001	<.001	0.069	0.001					
	N	437	440	441	437	435					
Math self-concept (spring)	Pearson Correl	.191**	-.225**	.389**	.106*	.126**	-.349**				
	Sig.	<.001	<.001	<.001	0.026	0.008	<.001				
	N	438	440	441	438	436	440				
Letter-Word Identification (spring)	Pearson Correl	.526**	-0.031	.143**	.916**	.548**	-.099*	.125**			
	Sig.	<.001	0.515	0.003	<.001	<.001	0.039	0.009			
	N	438	439	439	438	440	436	437			
Parent math expectancy-value (fall)	Pearson Correl	.299**	-0.008	.142**	.331**	.347**	-0.079	.161**	.309**		
	Sig.	<.001	0.869	0.002	<.001	<.001	0.098	<.001	<.001		
	N	461	461	462	461	433	434	435	434		
Parent math anxiety (fall)	Pearson Correl	-0.082	-0.059	0	-0.032	-.094*	-0.013	.095*	-0.025	-0.022	
	Sig.	0.076	0.201	1	0.492	0.05	0.784	0.046	0.599	0.632	
	N	467	467	468	467	439	440	441	440	476	
Parent math education (fall)	Pearson Correl	.157**	0.054	0.032	.178**	.197**	0.01	0.047	.176**	.170**	-.181**
	Sig.	<.001	0.249	0.501	<.001	<.001	0.836	0.337	<.001	<.001	<.001
	N	452	452	453	452	424	426	427	425	462	464

** Correlation is significant at the 0.01 level (2-tailed).

* Correlation is significant at the 0.05 level (2-tailed).

Main Analyses

One purpose of our path analyses was to examine whether apparent correlations that are commonly reported between child math achievement, math anxiety and math self-concept hold for young children from low SES backgrounds. Further, we wanted to examine whether these relations hold longitudinally in one model with these three variables (assessed in spring of 1st grade) as outcomes and controlling for these three variables from fall of 1st grade. We conducted cross-lagged path analyses in MPlus (Muthén & Muthén, 1998-2017). We used ESTIMATOR = MLR, maximum likelihood parameter estimates with standard errors that are robust to non-normality and non-independence of observations to account for the non-normal distribution of child math self-concept (see Table 1), and TYPE = COMPLEX to account for shared student-level variance within classrooms. Child math anxiety and math self-concept, and parent math expectancy-value and math anxiety were modelled as latent variables to account for measurement error. Math achievement and reading achievement were modelled as manifest variables (i.e., variables that can be directly measured or observed) in line with previous studies that have included child achievement in a cross-lagged path analysis (Gunderson et al., 2018).

Child Math Achievement and Self-Relevant Math Attitudes. Model 1, shown in Figure 5, examined bidirectional relations between child math achievement, math anxiety and math self-concept. The model had good fit: root mean square error of approximation (RMSEA) = .030, Comparative Fit Index (CFI) = .828, Tucker-Lewis Index (TLI) = .816, and standardized root mean square residual (SRMR) = .051.

Fall math achievement significantly negatively predicted spring math anxiety (ß=-.20, $p<.001$) more strongly than the reverse (ß=.03, $p=.508$.; see Figure 6 for a visual representation). In other words, a one unit increase in the standard deviation of fall math achievement is expected to result in a 20% decrease in the standard deviation of spring math anxiety. Longitudinal relations between math achievement and math self-concept were not statistically significant.

Concurrently, math anxiety and math self-concept were significantly negatively related in fall (ß=-.31, $p<.001$) and spring (ß=-.42, $p<.001$). Further, fall math achievement and math self-concept were significantly positively related (ß=.27, $p<.001$). Each key variable significantly predicted itself from fall to spring. Gender predicted spring math anxiety, such that girls (M=2.53, SD=0.76) had higher math anxiety than boys (M=2.34, SD=0.79). Gender also predicted fall math self-concept, such that girls (M=5.94, SD=0.75) had higher math self-concept than boys (M=5.84, SD=0.78). Condition did not predict any of our key variables.

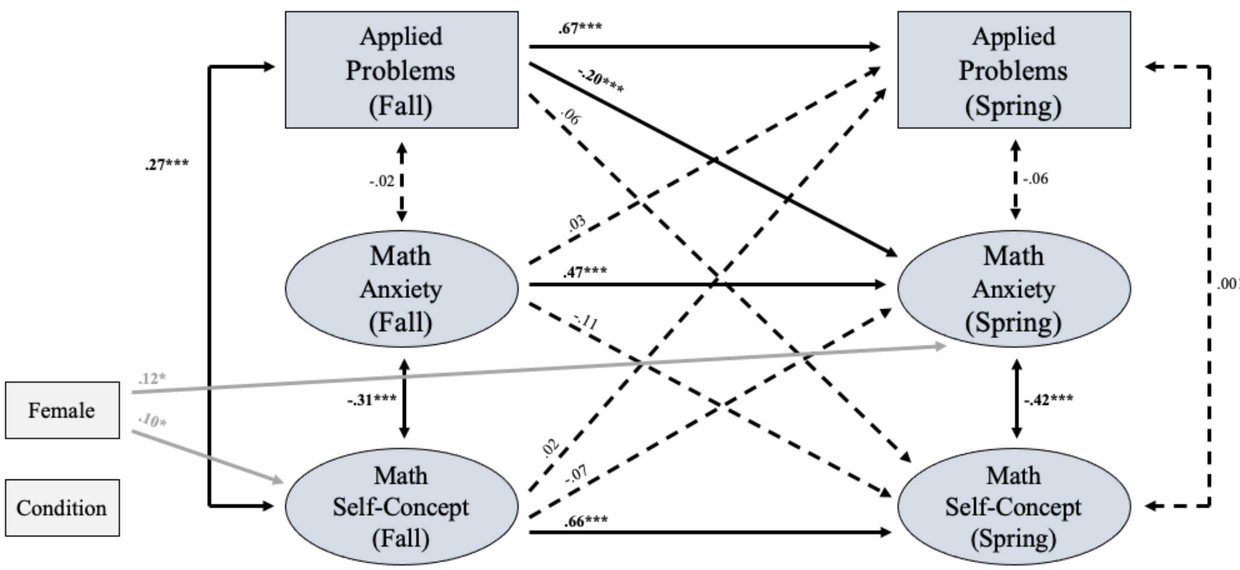
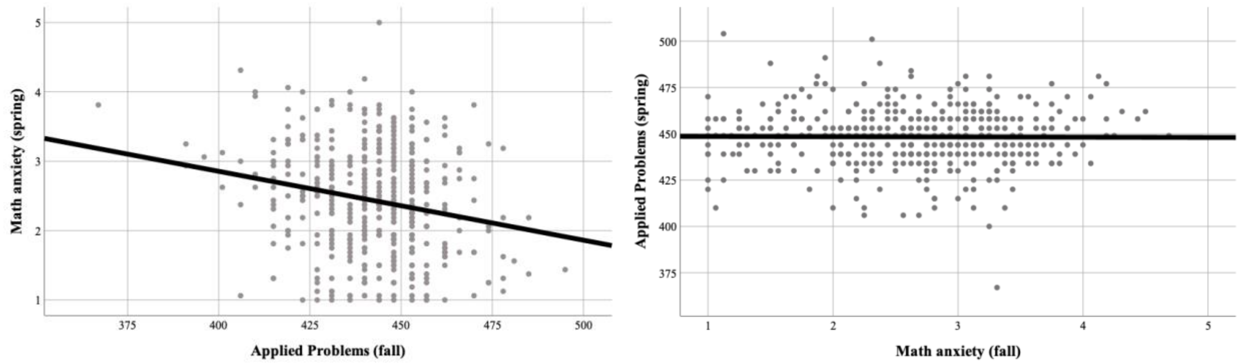


Figure 5. Cross-lagged path analysis for Model 1, showing concurrent and longitudinal relations between math achievement, math anxiety and math self-concept. Significant relations at the $p<.05$ level are represented with solid lines. Nonsignificant paths are represented with dotted lines. Nonsignificant relations with control variables are not shown.
$^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$



a. **b.**

Figure 6. Scatter plot of fall Applied Problems with spring math anxiety (a) and fall math anxiety with spring Applied Problems (b).

Parent Math Attitudes, Child Self-Relevant Math Attitudes and Math Achievement. In Model 2, shown in Figure 7, we built on Model 1, by adding parent math expectancy-value and parent math anxiety as predictors. Parent math education was included as a control variable to account for parent math knowledge. The model had good fit: RMSEA = .057, CFI = .734, TLI = .723, and SRMR = .053. Paths that were significant in Model 1, remained significant in Model 2. Below we discuss the new paths that were tested in Model 2.

Parent math expectancy-value significantly positively predicted child fall math achievement (ß=.39, $p<.001$) and math self-concept (ß=.24, $p<.001$), and child spring math achievement (ß=.18, $p<.001$; see Figure 8 for a visual representation). In other words, a one unit increase in the standard deviation of parent math expectancy-value is expected to result in a .18 increase in the standard deviation of child spring math achievement. Parent math anxiety did not significantly predict any child variables. Parent math education, a control variable, significantly positively predicted parent math expectancy-value and significantly negatively predicted parent math anxiety. Parent math education also significantly positively predicted child fall and spring math achievement.

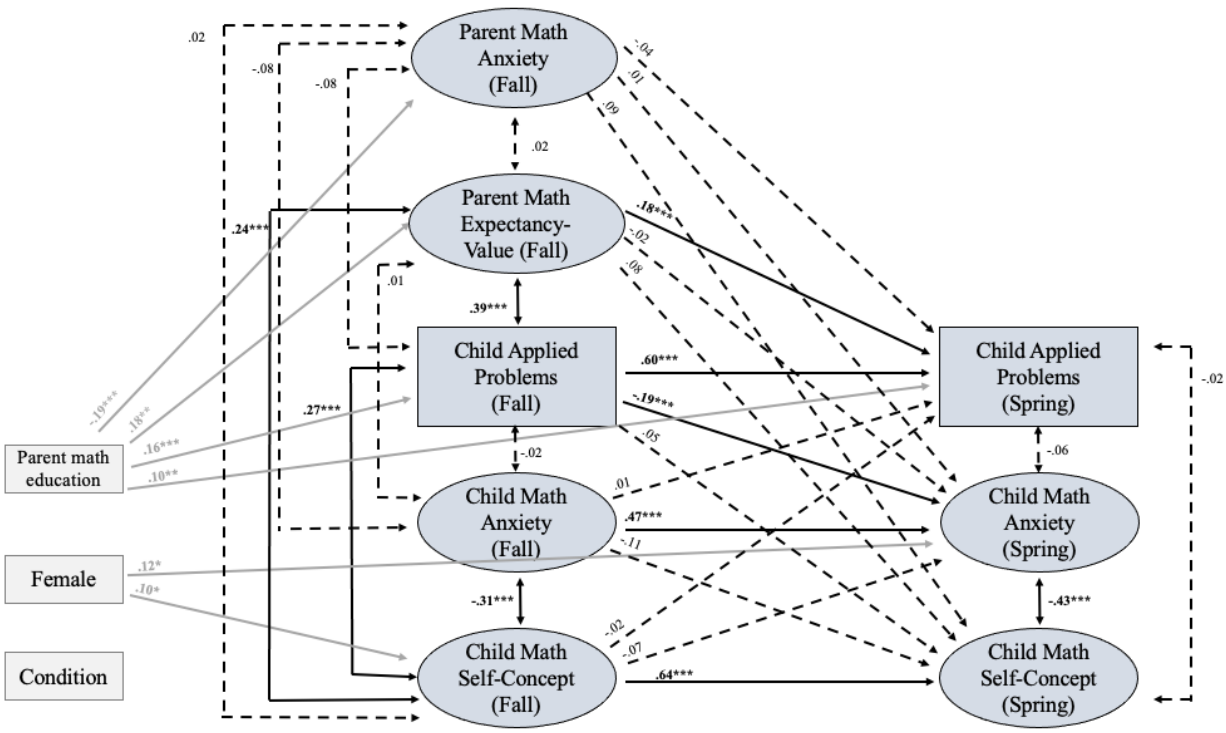


Figure 7. Cross-lagged path analysis for Model 2 showing concurrent and longitudinal relations between math achievement, math anxiety and math self-concept, as well as parent math anxiety and math expectancy-value. Significant relations at the $p<.05$ level are represented with solid lines. Nonsignificant paths are represented with dotted lines. Nonsignificant relations with control variables are not shown.
$^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$

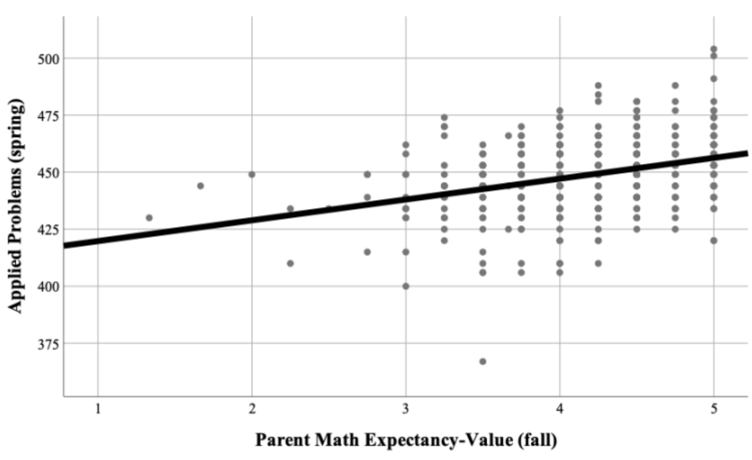


Figure 8. Scatter plot of parent math expectancy-value with child spring math achievement.

Divergent Validity

In Model 3, shown in Figure 9, we examined the divergent validity of our results. The model had good fit: RMSEA = .057, CFI = .734, TLI = .722, and SRMR = .053. Our two key findings reported in Model 1 and Model 2, were that child fall math achievement negatively predicted child spring math anxiety, and parent math expectancy-value positively predicted child spring math achievement. We wanted to examine whether these two key findings held for reading achievement. In Model 3, we included child reading achievement instead of child math achievement. Our key relations were not statistically significant in this parallel model. We see no evidence that child fall reading achievement predicted child spring math anxiety, or that parent math expectancy-value predicted child spring reading achievement.

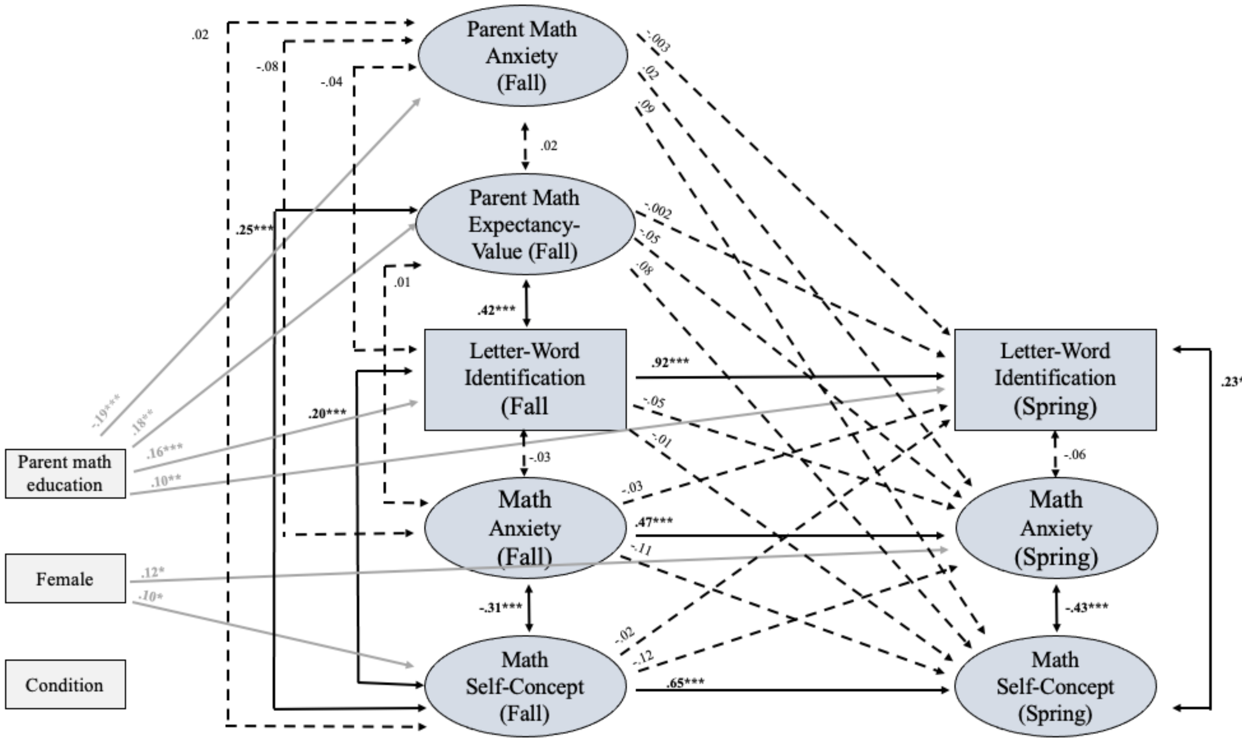


Figure 9. Cross-lagged path analysis for Model 3 showing concurrent and longitudinal relations between reading achievement, math anxiety and math self-concept, as well as parent math anxiety and math expectancy-value. Significant relations at the *p<.05* level are represented with solid lines. Nonsignificant paths are represented with dotted lines. Nonsignificant relations with control variables are not shown.
$^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$

Discussion

The current study was the first to examine longitudinal relations among parent math attitudes, child self-relevant math attitudes, and child math achievement in families from low SES backgrounds. 1st grade is an important time to examine these relations, as children have just begun formal schooling, and are likely beginning to develop positive or negative math attitudes. Because relations between self-relevant math attitudes and math achievement are stronger in high school than in elementary school (Ma & Kishor, 1997; Namkung et al., 2019), breaking the math achievement-attitude link may be easier when children are younger. Although the data reported in the current study are correlational, cross-lagged path analysis brings us one step closer toward improving our understanding of how these important factors are causally related. Nonetheless, experimental work is needed to determine whether the longitudinal relations we observed represent causal relations. Below, we describe conclusions based on our results.

Descriptives

Here, we discuss gender differences and SES differences. Girls had higher math anxiety than boys, in spring of 1st grade, similar to prior studies of elementary school students from higher SES backgrounds (Ching et al., 2020; Gunderson et al., 2018; Sorvo et al., 2017). Further research is needed to understand whether there are gender differences in young children's math self-concept.

In this study of children from lower SES backgrounds, children were behind by about 10 months of math learning (Applied Problems W Score mean = 441 and 448 in fall and spring of 1st grade) compared to Study 1 (Applied Problems W Score mean = 458 and 473 in fall and spring of 1st grade). However, math anxiety and math self-concept levels were very similar to those reported in other studies that used the same measures with 1st graders from diverse (but mostly higher) SES backgrounds (Eccles et al., 1993; Gunderson et al., 2018; see Study 1). Child math self-concept was very high, in line with findings that American children have higher self-esteem (Baumeister et al., 2003). Relatedly, a meta-analysis showed that SES has only a small effect on self-esteem, and the difference is especially small for younger children (Twenge &

Campbell, 2002). Our findings that child math anxiety and math self-concept levels are similar compared to children from higher SES backgrounds may also be explained by the big-fish-little-pond effect (i.e., equally achieving students have a lower self-concept when attending higher-performing schools than lower-performing schools; Marsh, 1987). In other words, children in our sample were in classrooms where their peers' average math achievement was low. However, children with low math achievement that are in classrooms where their peers' average math achievement is much higher than their own, may have more negative math attitudes, as they might compare their math achievement to that of their higher achieving peers.

While our results suggest that child math attitudes are similar regardless of SES backgrounds, in families from low SES backgrounds, parent math attitudes may be more negative. Future research should examine whether child math achievement plays a role in parent math attitudes, and whether in older children, math attitude levels differ based on SES backgrounds. In this study, there was not strong evidence of a relation between parent math anxiety and parent math expectancy-value, in contrast to findings from studies of families from higher SES backgrounds (Schaeffer et al., 2018). Thus, relations among parent math attitudes may differ depending on SES backgrounds.

Child Math Achievement and Self-Relevant Math Attitudes

Math Achievement and Math Anxiety. Our primary hypothesis—that math achievement would predict self-relevant math attitudes more strongly than the reverse—was supported for math anxiety. Fall math achievement significantly negatively predicted spring math anxiety, while the reverse relation was much weaker and not statistically significant. Our results are in line with those from studies of 1st and 2nd grade children from higher SES backgrounds, in which the relation from math achievement to math anxiety was also stronger than the reverse (Cargnelutti et al., 2017; Ching et al., 2020; Gunderson et al., 2018). We compare our findings to those of Gunderson et al. (2018), who used a statistical approach similar to ours, with a sample of 1st and 2nd grade children from homes where the average annual family income was nearly twice as high as that from the current study, and well above the poverty line. The strength of the relation between fall math achievement and spring math anxiety was similar in both studies (ß=-.20 in both studies). While Gunderson et al. (2018) showed a weaker, but significant, relation from fall math anxiety to spring math achievement, the current study showed a very weak relation that was not near statistical significance or in the expected direction. One possibility is that differences in findings can be explained by the children in our sample having lower math achievement and coming from lower SES backgrounds. In sum, math achievement-related experiences likely play an important role in the development of high or low math anxiety in young children, regardless of SES backgrounds. Further research is needed to determine if there are SES differences in the relation of math anxiety to later math achievement in young children.

Math Achievement and Math Self-Concept. We did not observe a significant longitudinal relation between child math achievement and math self-concept. Understanding when and how the relation of math achievement with math self-concept emerges for children from low SES backgrounds is an important question for future research.

Math Anxiety and Math Self-Concept. To our knowledge, our study was the first to examine the longitudinal relation between child math anxiety and math self-concept. We expected that math self-concept and math anxiety would be longitudinally related and were agnostic about the direction of the relation. We did not observe a statistically significant longitudinal relation in our path analysis, but the relation was in the expected direction (negative) and was significant in raw correlations. We did observe a significant concurrent negative relation between math anxiety and math self-concept in the fall and spring, in line with studies showing a concurrent relation in elementary school students from higher SES backgrounds (Jameson et al., 2014; Justicia-Galiano, 2017; Kaskens et al., 2020), suggesting that these self-relevant math attitudes may develop together. Eventually, these two self-relevant variables are likely longitudinally related, and further research should examine when this relation emerges and in what direction.

Parent Math Attitudes, Child Self-Relevant Math Attitudes and Math Achievement

Our second hypothesis—that parent math attitudes would predict child math achievement and child math attitudes—was partially supported. Parent math expectancy-value predicted child spring math achievement, controlling for fall math achievement and self-relevant math attitudes. Our results suggest that parent math expectancy-value is a strong predictor of children's success in math, in line with findings from studies of families from higher SES background (Schaeffer et al., 2018). To our knowledge, this is the first study to control for prior child math achievement, when examining the relation of parent math expectancy-value to young children's future math achievement. Moreover, we found that the relation of parent math expectancy-value to children's future math achievement was domain specific, as parent math expectancy-value did not significantly predict child spring math achievement in our path analysis. Improving parent math expectancy-value may be one way to improve child math achievement in young children from low SES backgrounds.

We did not observe a significant relation of parent math anxiety to child math achievement, in contrast to studies of children from higher SES backgrounds (Berkowitz et al., 2015; Maloney et al., 2015; Schaeffer et al., 2018). These findings are somewhat consistent with those of Berkowitz et al. (2021), who showed that parent math anxiety predicted number talk to toddlers in families from higher SES backgrounds, but not low SES backgrounds. One possibility is that parent math anxiety is not an important predictor of math behaviors or math achievement in families from low SES backgrounds. Another possibility is that in families from low SES backgrounds, parent math anxiety becomes more impactful when children grow older and work on more complex math assignments. Indeed, this is an important question for future research.

Final Thoughts and Future Directions

Social contexts or settings, such as the school and home environment, should be taken into consideration when examining what factors play a role in child math achievement (Eccles & Wigfield, 2020). Most children in our sample were from low SES backgrounds, were African American or Black, and had low math achievement. They attended schools where most of their peers were also from low SES backgrounds and had low math achievement. In contrast, other studies that have included children from diverse SES backgrounds, may have included children from low SES backgrounds that attended schools where many of their peers were from higher SES backgrounds and had higher math achievement (Berkowitz et al., 2015; Eccles et al., 1993; Gunderson et al., 2018; Schaeffer et al., 2018). Thus, the social contexts or settings of students in the current study may be very different from that of students in other samples that have examined similar research questions. It is possible that relations observed in the current study would differ in children from low SES backgrounds that are in different social contexts (e.g., if they attend schools where their peers' math achievement and SES backgrounds are higher). Further, it is unclear whether our results would replicate in samples of families from non-minority backgrounds. Indeed, future research should take social context into consideration when examining relations between parent math attitudes, child math attitudes, and child math achievement.

Our findings suggest that fostering high math achievement and positive math experiences can lead to the development of positive self-relevant math attitudes, regardless of SES backgrounds. Our findings raise important questions for future research. Future research should examine when and to what extent math anxiety begins to predict math achievement in children from low SES backgrounds. More broadly, how might SES differences in the math achievement and math anxiety relation shown in adolescent samples (OECD, 2013) change across development? In addition, more research is needed to understand how children interpret their math achievement and related experiences. For example, how do children feel after receiving a low math score? What messages do parents and teachers convey about their children's math achievement, and how does this make children feel?

Further, fostering high math expectancy-value in parents may be a way to improve child math achievement. However, experimental work is needed to understand whether improving parent math expectancy-value in turn improves child math achievement, or whether providing families with math tools reduces the negative effect of low parent math expectancy-value on child math achievement, in families from low SES backgrounds. Prior research shows that interventions are not a one-size-fits-all, as some interventions that reduce the effect of negative parent math attitudes on child math achievement are not successful in families from low SES backgrounds (Herts, 2020).

Given the importance of parent math expectancy-value, which has been shown to predict parent behaviors (Simpkins et al., 2002; Wigfield et al., 2006; 2015), in Study 3 we focus on a specific parent behavior: the talk that they provide to their toddlers.

STUDY 3: EFFECTS OF PARENT TALK DURING TODDLERHOOD ON CHILD CARDINALITY AND CALCULATION SKILLS

The amount of number talk that parents provide to their children during various stages of development positively predicts child math achievement (Casey et al., 2018; Elliott et al., 2017; Glenn et al., 2018; Gunderson & Levine, 2011; Levine et al., 2010; Ramani et al., 2015; Susperreguy & Davis-Kean, 2016; Thippana et al., 2020). However, the *causal effects* of the quantity of both naturalistic parent number talk and other talk (i.e., overall talk that excludes number talk) on children's long-term math skills are less clear. In the current study, we address important questions regarding the effect of parent talk: 1) Does parent number talk impact child math skills, 2) Does parent other talk impact child math skills, and 3) Does the impact of parent talk on child math skills vary based on when in development it is provided (i.e., early at 14 months or later at 38 months)? We address these questions by examining longitudinal, dynamic relations between parent talk and child math skills, using inverse probability of treatment weighting (IPTW). This statistical approach is designed to evaluate whether observed relations between variables could be causal, by accounting for baseline and time-varying covariates (Robins et al., 2000). IPTW allows us to treat observational data of parent talk provided at different time points as if they came from a randomized experiment. By using IPTW with observational data to examine causal effects of parent talk on child math achievement, we can make more informed decisions when developing interventions that target the optimal types of parent talk at the optimal times. In addition, we examine what parent and child characteristics predict parent talk.

Parent Number Talk and Other Talk

Young children likely acquire early math skills through exposure to informal learning opportunities at home (see Geary, 2022). Thus, understanding what aspects of the home environment are important for children's math skills is essential. One aspect of the home environment that might be particularly important is the talk that parents provide to their children. Sociocultural theory posits that language is a tool that supports cognition (Gauvain et al., 2001, 2011). Indeed, language is necessary for communicating math knowledge and for thinking about abstract math concepts (Peng et al., 2020). The current study focuses on two types of parent talk, which we describe below: parent number talk and parent other talk (i.e., overall talk that excludes number talk).

Observational studies show that the amount of number talk parents provide to their toddlers and children positively predicts math skills during preschool and elementary school, including cardinal number knowledge and calculation skills (Casey et al., 2018; Elliott et al., 2017; Glenn et al., 2018; Levine et al., 2010; Ramani et al., 2015; Susperreguy & Davis-Kean, 2016; Thippana et al., 2020). Thus, there is good reason to expect that parent number talk would causally affect child math skills. However, the statistical approaches that have been used in observational studies (e.g., regressions) preclude interpreting these relations as causal. We cannot rule out the possibility that observed relations between parent number talk and child math skills were due to confounders, such as parent characteristics, child math skills, or child talk.

Moreover, experimental studies suggest that parent number talk improves child math skills. For example, providing parents with math tools, such as math books or apps, to engage in with their children, improves child math achievement (Berkowitz et al., 2015; Gibson et al., 2020; Purpura et al., 2021; Schaeffer et al., 2018). Providing parents with these math tools likely changes their behaviors, such as the talk that they provide to their children, suggesting that parent talk has a causal effect on child math skills. For example, Gibson et al. (2020) showed that providing parents with math books to read with their 3-year-olds increased parent number talk in the context of book reading. Whether these math books changed other types of parent talk that might also be important is unclear. Moreover, these math tool interventions likely do not change long-term parent talk, as the effects of short-term interventions often fadeout relatively quickly (Bailey et al., 2016; Espinas & Fuchs, 2022).

In addition to parent number talk, there is good reason to expect that broader (i.e., overall) parent talk, is also important for child math skills for at least two reasons. First, parent language input (e.g., vocabulary and syntax input) affects child language skills (Silvey et al., 2021). Second, child language skills and math skills are linked. Because number skills have a strong language component, it makes sense that language skills would support children's understanding of terminology important for number skills (Espinas & Fuchs, 2022; LeFevre et al., 2010; Purpura et al., 2011, 2021). For example, child language skills are important for math skills, such as cardinal number knowledge, arithmetic and word problem solving (Espinas & Fuchs, 2022; Napoli & Purpura, 2018; Schröder et al., 2018). Moreover, a recent meta-analysis shows that language skills predict cardinal number knowledge and calculation skills, controlling for initial math skills (Peng et al., 2020). Thus, given the link between parent language input and child language skills, and the link between child language skills and math skills, it is possible that parent overall talk affects child math skills. For example, children who have larger vocabularies may be more ready to learn the meanings of number words, which are more challenging than nouns, because they refer to sets rather than to objects.

In sum, while results from previous observational and experimental studies support the *hypothesis* that increased parent number talk and other talk (i.e., overall talk that excludes number words) improve child math achievement, they do not show a long-term causal effect. The current study provides a more stringent test of the causal effects of increased parent number talk and other talk on child math skills. We acknowledge that parent other talk is quite broad. While testing the effects of parent other talk won't reveal if specific aspects of it are particularly effective for children's math skills, we thought it important to examine the effects of parents' overall talkativeness (i.e., other talk) as a first step. One possibility is that parent number talk and other talk have similar effects on child math skills. Another possibility is that the effects of parent number talk and other talk differ depending on the timing.

Timing of Parent Talk

The current study examines whether the effects of parent number talk and other talk differ based on *timing*. Researchers have examined the concurrent relation of parent number talk to child math skills (Elliott et al., 2017; Ramani et al., 2015), or the relation of parent number talk to children's future math skills, without accounting for intermediate parent number talk (Casey et al., 2018; Levine et al., 2010; Thippana et al., 2020). Because most studies examine parent number talk at one time point, the extent to which talk at that particular time point matters is unclear. One possibility is that parent number talk or other talk provided at one time point could influence various other processes that could impact child math learning (e.g., exposure to math materials and positive parent-child math interactions) that could then make parent talk provided at a later time point less important. However, a second possibility is that parents who provide high levels of talk early continue to do so throughout development, and that it is later parent talk that affects child math skills. A third possibility is that the timing of parent talk does not matter, but rather cumulative talk across development is what impacts child math skills. We test these three possibilities in the current study. We focus on parent talk provided earlier (at 14 months) when children's number skills and talk are limited, and later (at 38 months) when children begin to associate small number words with their corresponding set sizes (i.e., "one" through "three"; Carey & Barner, 2019). Understanding the optimal timing of parent number talk, as well as the optimal timing of parent other talk can inform the development of interventions that can aim to increase parent talk during the time points when it is most important.

Predictors of Parent Talk

Understanding what child and parent characteristics predict parent number talk and other talk will provide insight into why children receive differing levels of parent talk. For example, child behaviors such as their gestures or their own talk, could lead parents to provide more talk (Silvey et al., 2021). Math-gender stereotypes may lead adults to treat boys and girls differently (Levine & Pantoja, 2021). Child birth order could also predict parent behaviors (Keller & Zach, 2002). Further, parent characteristics, including income and education, have been shown to predict number talk and other language input (Dailey & Bergelson, 2021; Dearing et al., in prep; Levine et al., 2010; Silvey et al., 2021).

To our knowledge, previous research has not stringently examined how the relation of parent and child characteristics to parent talk may differ based on the type of parent talk (i.e., number talk or other talk) and when it is provided (i.e., earlier or later). For example, income and education have been shown to predict overall talk more strongly than number talk (Dailey & Bergelson, 2021; Geary et al., in prep). Determining what parent and child characteristics are strong predictors of parent number talk and other talk provided earlier and later, is an important part of IPTW, as children who received different sequences of parent talk are up-weighted or down-weighted (i.e., balanced) based on these parent and child characteristics (i.e., confounders). Further, this information will provide insights into what influences parent talk.

The Current Study

The current study aims to improve our understanding of the *causal* effect of naturalistic parent talk on child math skills. Since we cannot run an experiment randomly assigning children to parents that provide different sequences of talk, we use inverse probability of treatment weighting (IPTW), which allows us to treat observational data as if they were from an experiment, conditional on the assumption that we have measured the covariates that confound the association between parent talk and child math achievement. This statistical method was derived from epidemiology (Naimi et al., 2014; Robins et al., 2000), and was recently used to address similar questions regarding parent language input and child language outcomes (Silvey et al., 2021). IPTW allows us to estimate the true effects of parent early, later, and cumulative talk on child math skills under key assumptions, which we discuss in the Analytical Procedure section.

Our primary goal is to compare the impact that the timing of parent talk has on child math skills. We measure parent talk during two distinct stages in child development: earlier when children were 14 months and later when children were 38 months (see Methods for details on why we chose these time points). We test three hypotheses regarding the effects of parent number talk and other talk on child math skills: 1) early parent talk is more important than later parent talk, 2) later parent talk is more important than early parent talk, and 3) cumulative parent talk is key, and timing does not matter. Our secondary goal is to examine what parent and child characteristics (e.g., parent income and child gender) predict parent number and other talk provided earlier and later.

Silvey et al. (2021) examined the impact of time-varying parent language on child language outcomes and found differing effects for vocabulary and syntax. For vocabulary, increased parent talk both early (at 14 months) and later (at 30 months) were key for vocabulary outcomes in kindergarten. However, for syntax, parent later talk was key. Thus, it is possible that the optimal timing of parent talk may differ based on the type of talk (e.g., number talk or other talk) and based on the math skill (e.g., cardinal number knowledge at 46 months or calculation skills in 3rd grade).

Like previous studies, we account for baseline covariates, such as parent income and education. However, examining parent talk at multiple time points introduces another confounder: child talk. Parent early talk is likely to impact child intermediate talk and math skills, which could then influence child later math skills as well as parent later talk. Child talk is a *time-varying confounder*, as it is a potential response to early parent talk that could predict parent later talk and child future math skills. These baseline and time-varying confounders make it important to use IPTW to address our research questions.

We focus on two math outcomes: cardinal number knowledge (i.e., understanding the meanings of number words) at 46 months, before children have begun formal schooling; and calculation skills in 3rd grade, when children have been in a formal school setting for a few years. We focus on cardinal number knowledge, as it is a foundational math skill that is necessary to develop more complex math skills (Geary et al., 2018; Nguyen et al., 2016; Purpura & Lonigan, 2013). We focus on calculation skills because it is a key numerical skill that is acquired in the early elementary school years, and it predicts children's use of advanced mathematics strategies (Throndsen et al., 2011).

Method

Data were collected as part of a large longitudinal study of language development (Levine et al., 2010). The 64 children (31 girls, 36 first-borns) and their primary caregivers (56 mothers, 1 father, 7 both mother and father) who were selected to participate in the study were representative of the Chicago area in terms of race, ethnicity, and income. Families were visited once every 4 months from age 14 to 58 months and their naturalistic interactions were videotaped for 90 minutes. Parents were asked to interact with their child as they normally would. There were no instructions about what activities to engage in. All primary caregiver and child speech and gesture were transcribed, and we calculated measures of baseline and time-varying child speech and gesture, and measures of parent number talk and other talk, as described below.

Time-Points

To examine the effects of parents' early and later talk, we selected time points for "early" and "later" on a theoretical basis. The two time points are distinct stages in children's math development. Early talk was measured when children's math skills and number talk are known to be limited, and later talk was measured when there are changes in children's number knowledge. We chose 14 months as our time point for early talk, as at this age children's math skills and math talk are limited, and thus their number talk is not likely to influence parent number talk. We chose 38 months as our time point for later talk, as at this age, some children are beginning to understand the meanings of number words (LeCorre & Carey, 2007; LeCorre et al., 2006; Spaepen et al., 2018; Wynn, 1990).

Examining parent talk at multiple time points introduces another confounding variable: child number talk that occurs in between parent early and later talk, which could both be influenced by parent early number talk, and influence parent later number talk. For this measure, we used child talk at 34 months, which occurs after the time point when we measured parent early talk at 14 months and just before we measured parent later talk at 38 months.

We examined two different measures of children's math skills, which were assessed at different time points. First, we assessed cardinal number knowledge at 46 months, in line with previous analysis of this data (Gunderson & Levine, 2011; Levine et al., 2010), and to understand the role of parent math talk on a foundational aspect of math knowledge that is predictive of math achievement (Geary et al., 2018). Children also completed this assessment at 42 and 50 months. However, because some children reached ceiling on this task at 50 months, and performance at 46 months was more reliably correlated with important covariates (e.g., parent income and education) than performance at either 42 months or 50 months, we decided to focus on performance at 46 months for this analysis. Second, we assessed children's calculation skills when children were in 3^{rd} grade. Children also completed this assessment in 2^{nd} and 4^{th} grade, however because there was not much growth between 3^{rd} and 4^{th} grade, and performance in 3^{rd} grade was more reliably correlated with parent and child covariates than performance in 2^{nd} or 4^{th} grade, we decided to focus on performance in 3^{rd} grade.

Measures

Parent Covariates. To account for differences in socioeconomic status (SES) we included annual household income and primary caregiver years of education reported at 14 months as covariates. To account for general intellectual similarities between parents and their children, we included primary caregiver IQ, a composite of the full scale of the Wechsler Abbreviated Scale of Intelligence (WASI-II, Wechsler, 2011), that was administered when children were in 5th grade. For families with joint mother and father primary caregivers, only mothers' IQ was assessed, with the exception that in one joint caregiver primary family only the father chose to take the WASI.

Child Covariates. To account for factors that could influence parent talk and child outcomes, we included child word types (i.e., the number of unique words the child spoke) at 14 months. Additionally, we included child gesture types (i.e., the number of unique meanings the child produced in gesture) at 14 months. Child gesture is predictive of their numerical knowledge, motivating our decision to include this measure as a covariate (Gordon et al., 2021). Additionally, we included child gender and birth order, which we coded as either firstborn/only child or second/later born, in line with Silvey et al. (2021).

Parent Number Talk. Parents' use of number words "one" through "ten" at 14 months and 38 months were included. The number word "one" could have numerical or non-numerical uses, and we only used numerical uses in the current study. Parent number talk was divided into instances using the same procedure as Gunderson & Levine (2011), such that a counting sequence (e.g., counting from 1-10) was coded as one instance, as opposed to ten instances. All other types of number talk were coded as one instance. The target session length was 90 minutes, but the actual length slightly varied due to parent schedules or experimenter error. To account for small variations in session length, we pro-rated parent number talk based on actual session length (i.e., number instances * (90 minutes / actual minutes), in line with the procedure used by Levine et al., (2010). In line with previous analyses of this data (Gunderson et al., 2011; Levine et al., 2010), we used the natural log of parent number instances to ensure a linear association between number instances, and covariates and outcomes. All subsequent analyses use the natural log transformation (after adding 1).

Parent Other Talk. We included a measure of parents' other talk at 14 months and 38 months in order to run a parallel secondary analysis with parent other tokens as a predictor of child math achievement. Parent other word tokens consisted of all parent word tokens that did not include number tokens. To calculate other tokens, we subtracted numerical tokens of "one", as well of tokens of any number greater than "one" from parents' total word tokens. For this measure, we excluded number talk greater than 10, and excluded tokens of 0. Parent other tokens were pro-rated based on session length as well. To match our number talk variables, we used the natural log transformation of parent other tokens (after adding 1).

Time-Varying Child Number Talk and Other Talk. Child number instances and other tokens at 34 months, which were coded the same way as parent number instances and parent other tokens, were also pro-rated to adjust for session length and were natural log transformed.

Cardinal Number Knowledge at 46 Months. Children's understanding of the meanings of number words was assessed at 46 months using the 16-item Point-to-X task (Wynn, 1992). For each of the 16 items, children were shown a sheet of paper with two sets of squares. Children were then asked to point to X, with X being a number between 2 and 6 (e.g., 3 vs. 5 or 5 vs. 6; for the full list of items, see Levine et al., 2010). Children responded by pointing to one of the two sets of squares.

Calculation Skills in 3rd Grade. Calculation skills were assessed in 3rd grade using the Calculation subtest of the Woodcock Johnson III, a nationally normed measure of mathematics ability (Woodcock et al., 2001). This subtest requires children to apply their knowledge of numbers and calculation procedures through a paper-and-pencil test. Items of increasing difficulty range from solving numerical operations (i.e., addition, subtraction, division, multiplication) to geometric and trigonometric operations if appropriate. Subsequent analyses examined students' W scores, a transformation of raw scores into a Rasch-scaled score of equal interval measurements that represents the child' ability and the task difficulty, after establishing floor and ceiling. The W score is recommended to measure individual growth. A one-point W score increase roughly represents a month of learning during a school year. A score of 488 is the approximate average performance of a 9-year-old. The correlation between our measures of cardinal number knowledge and calculation skills were 0.439 ($p=0.001$).

Analytical Procedure

Multiple Imputation

For each variable, we had data for 50 to 64 families (see Table 12). We addressed missing values via multiple imputation with predictive mean matching, using the *mice* library in R (van Buuren & Groothuis-Oudshoorn, 2011). Our procedure was very similar to that used by Silvey et al. (2021) with three exceptions, described in Section S1 of the Appendix.

IPTW

To examine the effect of time-varying parent talk on child math skills in an experiment, children would be randomly assigned to sequences of parent talk (e.g., some might be assigned to receive high levels of parent talk earlier, and lower levels of parent talk later). To examine this question in an observational study, we used IPTW. This approach involves marginal structural models, a class of causal models that allow us to estimate how children would perform under alternative sequences of parent talk (Robins et al., 2000). IPTW allows us to treat observational data as if they came from a randomized experiment, under key assumptions described below, by accounting for baseline confounding variables (e.g., parent and child characteristics), and time-varying confounding variables (i.e., child talk that occurs between earlier and later parent talk and could be influenced by and influence parent talk). In our case, weights are assigned to children based on the inverse of their propensity (i.e., probability) of receiving the amount of parent talk they received (i.e., the treatment). An essential component of IPTW involves achieving balance (i.e., removing the association) between parent talk and important covariates from the past that may have influenced parent talk. In other words, the association between earlier parent talk and child math achievement is adjusted for baseline covariates. Similarly, the association between later parent talk and child math achievement is adjusted for baseline covariates, earlier parent talk, and time-varying child talk. Thus, children who received alternative sequences of parent talk will be balanced on observable baseline and time-varying covariates.

Assumptions. There are two important assumptions that need to be met. The first assumption is that there are no *unobservable* confounders once *observable* confounders are accounted for. This strong assumption needs to be at least partially tested theoretically and empirically through sensitivity analysis. With sensitivity analysis, we examine how large the unobservable confounding would need to be to qualitatively change our key findings. If such unobservable confounding would need to be implausibly large, our conclusions would be robust (i.e., they would be insensitive to unobserved confounders). However, if unobservable confounding would be plausible, our conclusions would not be robust (i.e., they would be sensitive to unobserved confounders). We assess this plausibility and the robustness of our conclusions based on prior research and evidence from the current study. This approach is a very partial specification check and does not provide proof that our results are valid. It is impossible to know the severity of bias resulting from failure to include unobserved confounders. See Section S4 of the Appendix for a detailed description of how we conducted the sensitivity analysis.

The second assumption is that common support is present. We have common support when subsets of children who have a similar probability of receiving a certain amount of parent talk vary substantially on the amount they received (Hong, 2012). We lack common support when subsets of children who have a similar probability of receiving a certain amount of parent talk vary little on the amount they received, implying that they have no information about the impact of parent talk (i.e., no comparison group).

Predicting Quantity of Parent Talk. To understand why some children received more parent talk than others, we ran ordinal models of parent and child characteristics predicting each of our four categories of parent talk: parent number talk and other talk provided at 14 months and at 38 months. In addition to providing insights into what factors predicts parent talk, this information is an important component of constructing the weights.

Constructing the Weights. IPTW *up-weights* children who were *unlikely* to have received the amount of parent talk provided, and *down-weights* children who were *highly likely* to have received the amount of parent talk provided. Typically, IPTW is used with binary predictors. Since our parent talk measures are continuous, we used a quantile binning approach (Naimi et al., 2014), adapted by Silvey et al. (2021) to adjust for confounding in continuous variables. First, we divided each of our four categories of parent talk (i.e., number talk or other talk provided at 14 months or 38 months) into eight quantiles. We then used an ordinal model to predict quantile (of each parent talk category) from covariates. This allowed us to estimate each child's likelihood of receiving the amount of parent talk provided.

Because our sample size was small, we used a stepwise procedure to determine what covariates to account for. For each category of parent talk, we first accounted for the covariate that was the strongest predictor (based on standardized betas). We then accounted for the second strongest covariate that remained associated with parent talk and stopped when there were no covariates that were associated with parent talk based on our criteria of $t>|1.67|$ (i.e., $p<.10$). Finally, we calculated the weights to appropriately up-weight or down-weight children. For a very detailed explanation of the quantile binning method, as well as the method for constructing the weights, see Silvey et al. (2021).

In sum, IPTW allows us to have a better sense of how naturalistic parent talk causally affects child math skills, as long as key assumptions are met. With this approach, we can examine causal effects of parent number talk and other talk provided earlier and later, on child math skills.

Hypotheses to Test

Our primary hypotheses to test regarding the effect of parent number talk and other talk on child math skills were about the timing of parent talk, specifically 1) earlier talk (Z_1) is more important than later talk (Z_2), 2) later talk (Z_2) is more important than earlier talk (Z_1), and 3) cumulative talk (Z_1+Z_2) is key and the timing does not matter. We tested these hypotheses with either parent number talk or other talk as predictors of our outcomes (child cardinal number knowledge at 46 months or calculation skills in 3rd grade). Due to the strong correlation between parent number talk and other talk at 14 months (r=.68, $p<.001$), and at 38 months (r=.72, $p<.001$), it was not possible to include them in the same model as predictors (see Section S2 of the Appendix for correlations among other measures of parent talk). In all four sets of models, we controlled for child gesture types at 14 months (X_g; for our reasoning, see the Achieving

Balance Between Parent Talk and Covariates section in Results). In the two sets of models predicting calculation skills in 3rd grade, we controlled for child age at time of test (X_a), because child age at test varied (see Table 12).

To test hypotheses 1 and 2 separately for parent number talk and other talk, we first examined parent earlier talk and later talk as separate predictors of child cardinal number knowledge or calculation skills with the following statistical model using combined weights (i.e., weights for both Z_1 and Z_2; see Silvey et al., 2021 for a detailed explanation of combined weights) to account for children's propensity to receive the amount of parent talk received:

$$Y_i = \alpha + \delta_1 Z_{1i} + \delta_2 Z_{2i} + \beta_g X_{gi} + \beta_a X_{ai} + e_i, \quad (1)$$

where Y_i is the math outcome (cardinality or calculation) for child *i*; δ_1 is the impact of each additional unit of Z_{1i} (parent earlier number talk or other talk at 14 months), holding Z_{2i} (parent later number talk or other talk at 38 months) and covariates constant. δ_2 represents the impact of each additional unit of Z_{2i}, holding Z_{1i} and covariates constant, β_g represents the impact of each additional unit of covariate X_{gi} (child gesture types at 14 months) holding Z_{1i}, Z_{2i}, and covariates constant, α is the model intercept, and e_i is a random error assumed to be uncorrelated with Z_1 and Z_2 after accounting for baseline covariates through weighting. To account for differences in child age at the time calculation skills were assessed in 3rd grade, an additional covariate, β_a, was only included where calculation was the outcome, and represents the impact of each additional unit of X_{ai} (child age in months when they completed the Calculation subtest), holding Z_{1i}, Z_{2i}, X_{ai}, and covariates constant.

Under hypotheses 3, parent earlier and later talk are equally important, and cumulative talk is key. To test this hypothesis, we examined whether the effects of parent earlier talk differed from those of parent later talk. Then, we ran the following statistical model with combined weights:

$$Y_i = \alpha + \delta\,(Z_{1i} + Z_{2i}) + \beta_g X_{gi} + \beta_a X_{ai} + e_i, \tag{2}$$

where δ represents the impact of each additional unit of $Z_{1i} + Z_{2i}$ (cumulative parent talk), holding covariates constant.

Similar to Silvey et al. (2021), we wanted an additional check of hypotheses 1 and 2, as our confidence in our test of differing effects between parent earlier and later talk was weak for a few reasons, which we describe in the Outcome Models section. Assuming that parent talk has a positive effect both earlier and later, if δ_1>0, while δ_2=0, this could imply that parent talk early is necessary and sufficient for child math skills. However, if δ_2>0, while δ_1=0, this could imply that later parent talk is necessary and sufficient, while early parent talk is unimportant. We tested each of these two hypotheses with a strong model, controlling for the appropriate covariates discussed above.

Before testing hypotheses 1, 2, and 3, we estimated the effect of earlier parent talk without controlling for later parent talk with the following statistical model:

$$Y_i = \alpha + \delta^* Z_{1i} + \beta_g X_{gi} + \beta_a X_{ai} + e_i, \tag{A}$$

where δ^* represents the impact of each additional unit of Z_{1i}. We did this to replicate prior studies that show a positive effect of parent number talk at one time point or across time points, without controlling for parent number talk at a later time point (e.g., Levine et al., 2010; Ramani et al., 2015). With this model, the estimated effect of earlier parent talk is ambiguous, as an apparent effect could result from either parent earlier talk being more important than later parent talk, or from parent earlier talk being correlated with parent later talk, which could either have similar effects to parent earlier talk or be a more important predictor. Indeed, the ambiguity of δ^* in prior studies is an important motivation for the current study.

Results

Descriptive Statistics and Correlations

Descriptive statistics for key variables are shown in Table 12. Correlations between covariates and parent talk are shown in Table 13. Correlations between other key variables are shown in Section S2 of the Appendix. Parent number talk and other talk were strongly correlated at 14 months (r=.68, p<.001), and 38 months (r=.72, p<.001; see Section S2 of the Appendix for correlations among other measures of parent talk), therefore we do not include them in the same models. Instead, we run parallel analyses with either parent number talk or other talk as predictors of child math skills.

Table 12. Descriptive statistics for key variables.

Variable	Type	Number of Valid Cases	Min	Mean	Median	Max	SD
Child gesture types at 14 months	X_0	64	4.00	21.70	18.50	54.00	12.49
Child word types at 14 months	X_0	64	0.00	14.06	8.50	59.00	14.57
Parent IQ composite	X_0	51	75.00	106.92	107.00	134.00	12.86
Household income (thousands of USD)	X_0	64	7.50	60.20	62.50	100.00	31.42
Parent years of education	X_0	64	10.00	15.66	16.00	18.00	2.24
Child number instances at 34 months (natural log)			0.00	1.58	1.61	3.26	1.05
Child number instances at 34 months (raw)	X_{1N}	62	0.00	6.97	4.01	25.00	6.98
Child other tokens at 34 months (natural log)			5.60	7.39	7.46	8.24	0.54
Child other tokens at 34 months (raw)	X_{1O}	62	270.41	1795.65	1709.24	3792.58	833.98
Parent number instances at 14 months (natural log)			0.00	2.06	2.21	3.96	0.97
Parent number instances at 14 months (raw)	Z_{1N}	64	0.00	10.56	8.08	51.29	10.07
Parent number instances at 38 months (natural log)			0.00	2.75	2.80	4.98	0.97
Parent number instances at 38 months (raw)	Z_{2N}	61	0.00	23.13	15.00	144.92	26.28
Parent number instances cumulative (natural log)			0.70	4.81	5.14	7.62	1.64
Parent number instances cumulative (raw)	$Z_{1N}+Z_{2N}$	61	1.01	33.74	26.34	154.30	30.06
Parent other tokens at 14 months (natural log)			5.27	8.04	8.23	9.06	0.66
Parent other tokens at 14 months (raw)	Z_{1O}	64	194.17	3643.11	3760.16	8641.41	1725.94
Parent other tokens at 38 months (natural log)			6.78	8.20	8.35	9.17	0.60
Parent other tokens at 38 months (raw)	Z_{2O}	61	876.51	4122.89	4153.00	9573.94	2004.06
Parent other tokens cumulative (natural log)			12.13	16.24	16.68	18.23	1.14
Parent other tokens cumulative (raw)	$Z_{1O}+Z_{2O}$	61	1144.17	7783.24	8431.53	18215.34	3439.86
Point-to-X at 46 months	Y_P	59	4.00	12.29	13.00	16.00	3.16
Calculation W Score in 3rd grade	Y_C	50	477.00	504.08	502.50	533.00	14.54
Child Calculation in 3rd grade age at test	X_2	64	102.00	112.06	114.00	119.00	4.55

Table 13. Correlations between covariates (first column) and parent talk (first row).

		Z_1 parent number instances	**Z_2 parent number instances**	**Z_1 parent other tokens**	**Z_2 parent other tokens**	**Cumulative parent number instances**	**Cumulative parent other tokens**
Child gender (girl)	**Pearson Correl.**	-0.193	**-.257***	**-0.233†**	**-0.249†**	**-.277***	**.261***
	Sig.	0.127	**0.046**	**0.064**	**0.053**	**.030**	**.042**
	N	64	**61**	**64**	**61**	**61**	**61**
Child birth order (first-born)	**Pearson Correl.**	-0.14	-0.005	-0.085	-0.096	-.132	-.107
	Sig.	0.269	0.97	0.503	0.463	.312	.414
	N	64	61	64	61	61	61
Child gesture types at 14 months	**Pearson Correl.**	0.115	**.277***	**.274***	**0.238†**	.213	**.288***
	Sig.	0.366	**0.031**	**0.029**	**0.065**	.100	**.025**
	N	64	**61**	**64**	**61**	61	**61**
Child word types at 14 months	**Pearson Correl.**	0.131	0.084	-0.01	0.01	.077	.011
	Sig.	0.304	0.521	0.938	0.942	.554	.932
	N	64	61	64	61	61	61
Parent income	**Pearson Correl.**	**.319***	**.309***	**.386****	**.447****	**.356****	**.451*****
	Sig.	**0.01**	**0.015**	**0.002**	**<0.001**	**.005**	**<.001**
	N	**64**	**61**	**64**	**61**	**61**	**61**
Parent education	**Pearson Correl.**	**0.232†**	**.272***	**.414****	**.307***	**.353****	**.404*****
	Sig.	**0.065**	**0.034**	**0.001**	**0.016**	**.005**	**.001**
	N	64	61	64	61	61	61
Parent IQ	**Pearson Correl.**	0.064	0.155	**.367****	**0.243†**	.186	**.335***
	Sig.	0.653	0.277	**0.008**	**0.085**	.192	**.016**
	N	51	51	**51**	**51**	51	**51**
X_1 child number instances	**Pearson Correl.**	0.148	0.119	0.158	0.202	**.189**	.192
	Sig.	0.25	0.36	0.219	0.119	**.146**	.137
	N	62	61	62	61	**61**	61
X_1 child other tokens	**Pearson Correl.**	0.058	0.067	0.104	0.208	**.084**	.170
	Sig.	0.656	0.61	0.423	0.108	**.518**	.190
	N	62	61	62	61	**61**	61
Child age at time of Calculation in 3rd grade	**Pearson Correl.**	0.128	0.184	0.197	0.182	.246†	.198
	Sig.	0.313	0.156	0.119	0.160	.055	.125
	N	64	61	64	61	61	61

†p<0.1 $^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$

Achieving Balance Between Parent Talk and Covariates

To understand what parent and child characteristics in our analyses are the strongest predictors of parent talk, it is important to first explain how we achieved balance between covariates and each category of parent talk. We first used ordinal models with covariates as predictors of each of our four categories of parent talk: earlier number talk (Z_{1N}), later number talk (Z_{2N}), earlier other talk (Z_{1O}), and later other talk (Z_{2O}). We used this information to determine what covariates to adjust for when constructing the weights to achieve balance between each category of parent talk and each covariate. As stated in our Analytical Procedure section, our stepwise procedure involved adjusting for covariates, until no covariate was significantly related to parent talk based on our cutoff criteria of $t>|1.67|$ (i.e., $p<.10$). To construct the weights, we followed the very detailed procedure described by Silvey et al. (2021), with two exceptions.

First, because it was not possible to achieve balance between child gesture types at 14 months (X_g) and Z_{2N} or Z_{1O}, perhaps due to our small sample size, we controlled for X_g in our outcome models. Therefore, in addition to weighting for X_g, it was necessary to ensure balance was achieved between each category of parent talk and other covariates controlling for X_g. Thus, we controlled for X_g in all ordinal models predicting parent talk. Because we controlled for X_g, we did not check balance between parent talk and X_g.

Second, because child age slightly differed when the Calculation subtest was administered in 3rd grade (see Table 2), we controlled for child age (in months) at time of test (X_a) in our outcome models predicting calculation skills. Therefore, it was necessary to run an additional set of ordinal models predicting parent talk from covariates, in which we controlled for X_a. After controlling for X_a in our ordinal models, we still had balance between parent talk and covariates, therefore it was not necessary to make any changes to the weights. See Section S3 of the Appendix for standardized coefficients and t-ratios of covariates before and after weighting.

Determining What Covariates Most Strongly Predict Parent Talk

Below, we describe the covariates that most strongly predicted parent talk, and which we used to construct the weights to achieve balance between parent talk and all covariates, for each of our four categories of parent talk (parent number talk or other talk provided at 14 months or at

38 months). We describe these as the strongest covariates, because adjusting for the covariates discussed below was sufficient to achieve balance between each category of parent talk and all other covariates.

Number Talk. The strongest predictors of parent early number talk (Z_{1N}) were child gender and household income. In addition, it was necessary to adjust for child gesture types (X_g; see Analytical Procedure section for our reasoning). Adjusting (i.e., weighting) for these three covariates was sufficient to achieve balance between Z_{1N} and baseline covariates. The strongest predictors of parent later number talk (Z_{2N}) were parent earlier number talk (Z_{1N}), child gesture types (X_g), child gender, and household income. Adjusting for these four covariates was sufficient to achieve balance between Z_{2N} and baseline covariates.

Other Talk. The strongest predictors of parent early other talk (Z_{1O}) were child gesture types (X_g), child word types at 14 months, child gender, and parent education. Adjusting for these four covariates was sufficient to achieve balance between Z_{1O} and baseline covariates. The strongest predictors of Z_{2O}, were Z_{1O}, household income and parent education. In addition, it was necessary to adjust for child gesture types (X_g; see Analytical Procedure section for our reasoning). Adjusting for these four covariates was sufficient to achieve balance between Z_{2O} and baseline covariates.

Outcome Models for Cardinal Number Knowledge at 46 Months

Number Talk as a Predictor. The results of the models described in the Analytical Procedure section, with parent number talk predicting cardinal number knowledge at 46 months, are reported in Table 14. First, we estimated Model A (control model) to replicate previous findings showing a relation between parent early number talk and child cardinal number knowledge. Results showed that for each one unit increase in the standard deviation (0.97) of our measure of parent earlier number talk (a natural log transformation of parent number instances at 14 months), we expect a 0.810 point (S.E=0.391, p=0.043) increase (i.e., an increase of about 5%) in our measure of cardinal number knowledge (Point-to-X at 46 months), in which scores ranged from 4 to 13 out of 16, M=12.29; SD=3.16). This result replicates previous findings, but as discussed earlier, the ambiguity of results from this model which does not account for later parent or child number talk, is an important motivation for the current study.

Therefore, we estimated Model 1 (separate predictors model), to test the impact of possible sequences of earlier and later parent talk in a regression that includes weights for parent number talk both earlier and later. As shown in Table 14, results showed that for each one unit increase in the standard deviation of our measure of parent earlier number talk (0.97) and later number talk (0.97; a natural log transformation of parent number instances at 38 months), we expect a 0.582 point (S.E=0.420, p=0.171) and 0.556 point (S.E=0.346, p=0.179) increase in our measure of cardinal number knowledge (i.e., an increase of about 4% and 3%). While we did not observe significant differences between the effects of early and later number talk (p=0.965), our confidence in this result was weak for a few reasons: wide confidence intervals in our test of differing effects [-1.388, 1.336], a small sample size (n=64), and the medium correlation between earlier and later number talk (r=0.418).

As shown in Table 14, Model 2 (constant effects model) shows that for each one unit increase in the standard deviation of our measure of parent cumulative number talk (1.64; parent number talk at 14 months + parent number talk at 38 months), we expect a 0.957 point (S.E.=0.379, p=0.014) increase in our measure of cardinal number knowledge (i.e., an increase of about 6%). Interestingly, the coefficient for cumulative number talk is nearly double that of either earlier or later number talk on their own (see Table 14), suggesting that constant parent number talk across time is key and the timing does not matter for children's cardinal number knowledge at 46 months.

As an additional check of differing effects between earlier and later number talk, we ran two extreme models: Models 3 and 4. In Model 3, we assume that earlier number talk is greater than 0 and later number talk is set to 0. In Model 4, we assume that later number talk is greater than 0 and earlier number talk is set to 0. As shown in Table 14, in these models, the coefficient for earlier number talk (0.806) was similar to that of later number talk (0.776). Based on the standard Akaike information criterion (AIC) method of model comparison, in which a lower AIC suggests a stronger model, neither of these two strong models fit the data as well as the constant effects model (Model 2).

We conclude that high levels of parent number talk both earlier at 14 months and later at 38 months (i.e., constant number talk) have a positive impact on children's cardinal number knowledge at 46 months. Sensitivity analysis, reported in Section S4 of the Appendix, suggests that our conclusions are robust. Common support is reported in Section S5 of the Appendix.

Table 14. Results of weighted outcome models estimating the effect of earlier and later parent number talk on child cardinal number knowledge at 46 months.

Model and predictor	Effect	Standardized coefficient estimate	95% CI	*SE*	*t-ratio*	*Nominal p*	AICc
A. Control model							
Early talk	δ_1	0.810	[0.028, 1.591]	0.391	2.074	0.043	
1. Separate predictors							329.5
Early talk	δ_1	0.582	[-0.258, 1.422]	0.420	1.387	0.171	
Later talk	δ_2	0.556	[-0.262, 1.374]	0.346	1.362	0.179	
	$\delta_2 - \delta_1$	-0.031	[-1.388, 1.336]	0.695	-0.045	0.965	
2. Constant effects							**327.1**
Cumulative talk	$\boldsymbol{\delta}$	**0.957**	**[0.198, 1.716]**	**0.379**	**2.525**	**0.014**	
3. Later talk=0, Early talk>0							329.1
Early talk	δ_1	0.806	[0.031, 1.581]	0.387	2.081	0.042	
4. Early talk=0, Later talk>0							329.2
Later talk	δ_2	0.776	[0.029, 1.532]	0.378	2.054	0.044	

Other Talk as a Predictor. The results of the models with parent other talk predicting cardinal number knowledge at 46 months are reported in Table 15. In Model A (control model), the coefficient for our measure of parent earlier other talk at 14 months was not statistically significant (estimate=0.557, S.E=0.373, p=0.142). In Model 1 (separate predictors model), the effects between earlier other talk at 14 months (estimate=0.291, S.E=0.0.485, p=0.551) and later other talk at 38 months (estimate=0.757, S.E=0.462, p=0.142) did not significantly differ (p=0.107). Our confidence in this result was weak, for reasons similar to those discussed earlier.

As shown in Table 15, Model 2 (constant effects model) shows that for each one unit increase in the standard deviation of our measure of parent cumulative other talk (1.14; parent other talk earlier at 14 months + later at 38 months), we expect a 0.965 point increase in our measure of cardinal number knowledge at 46 months (i.e., an improvement of about 6%). Our AICc for this model is 315.90.

As an additional check of differing effects between earlier and later other talk, we ran two extreme models: Models 3 and 4. In Model 4, where earlier other talk is set to 0 and later other talk is greater than 0, the coefficient for later talk (0.930) is similar to that of cumulative other talk from Model 2 (constant effects model); and the AICc is 315.88, which is about the same as that of Model 2. We cannot rule out the possibility that other talk provided later at 38 months is more important than other talk provided earlier at 14 months for a few reasons: in Model 1 (separate predictors model) the coefficient for later number talk (0.757) is more than double that of earlier number talk (0.291), our test of differing effects shows a wide confidence interval [-1.201, 2.134], we have a small sample size, and the AICc in Model 2 (constant effects model) and Model 4 (an extreme model where parent talk earlier is set to 0 and parent talk later is greater than 0) are similar.

We conclude that high levels of constant parent other talk (i.e., other talk both earlier at 14 months and later at 38 months) positively impacts cardinal number knowledge at 46 months.

However, we cannot rule out the possibility that parent other talk provided later at 38 months is more important than parent other talk provided earlier at 14 months. Sensitivity analysis is reported in Section S4 of the Appendix. Common support is reported in Section S5 of the Appendix.

Table 15. Results of weighted outcome models estimating the effect of earlier and later parent other talk on children's cardinal number knowledge at 46 months.

Model and predictor	Effect	Standardized coefficient estimate	95% CI	*SE*	*t-ratio*	*Nominal p*	AICc
A. Control model							
Early talk	δ_1	0.557	[-0.191, 1.304]	0.373	1.490	0.142	
1. Separate predictors							317.8
Early talk	δ_1	0.291	[-0.680, 1.262]	0.485	0.600	0.551	
Later talk	δ_2	0.757	[-0.168, 1.683]	0.462	1.640	0.107	
	$\delta_2 - \delta_1$	0.492	[-1.201, 2.134]	0.817	0.603	0.549	
2. Constant effects							**315.90**
Cumulative talk	$\boldsymbol{\delta}$	**0.965**	**[0.229, 1.700]**	**0.367**	**2.627**	**0.011**	
3. Later talk=0, Early talk>0							318.4
Early talk	δ_1	0.787	[0.026, 1.548]	0.380	2.072	0.043	
4. Early talk=0, Later talk>0							**315.88**
Later talk	$\boldsymbol{\delta_2}$	**0.930**	**[0.220, 1.641]**	**0.355**	**2.621**	**0.011**	

Outcome Models for Calculation Skills in 3rd Grade

Number Talk as a Predictor. The results of the models with parent number talk predicting calculation skills in 3rd grade are reported in Table 16. Because number talk did not significantly predict calculation in Models 1 to 4, we ran a simple model, Model 5, where only our control variables— child gesture types at 14 months (X_g) and child age at time of test (X_a)— were entered as predictors of calculation. Based on the AIC method of model comparison, Model 5 was the best fitting model.

We caution against strong conclusions of no effect of number talk on calculation skills for a few reasons: the coefficients for number talk—especially cumulative number talk (estimate=2.292, which represents about 2 months of learning during a school year)—are quite large, the confidence intervals are mostly on the positive side—especially for cumulative number talk [-1.345, 5.929]—and we have a small sample. If it were the case that there is a true effect of parent number talk, for each one unit increase in the standard deviation of our measure of cumulative parent number talk, we would expect a 2.292 point increase (S.E.=1.813, p=0.212) in our measure of calculation skills in 3rd grade (i.e., about 2 months of learning during a school year).

While the effects between earlier number talk at 14 months and later number talk at 38 months on calculation skills did not significantly differ, we caution against strong conclusions of no differing effects for a few reasons: in Model 1 the coefficient for earlier number talk (1.802; S.E.=1.997, p=0.371) is nearly twice as large as that of later number talk (0.941; S.E.=1.933, p=0.628), wide confidence intervals in our test of differing effects [-7.151, 5.429], our small sample size, and the medium correlation between earlier and later number talk (r=0.418).

We conclude that although we do not see significant effects of parent number talk on calculation skills in 3rd grade, it is possible that number talk—especially constant number talk—has a positive effect on calculation skills. Additionally, we see some suggestion that parent number talk earlier at 14 months is more important than parent number talk later at 38 months for calculation skills in 3rd grade. Common support is reported in Section S5 of the Appendix.

Table 16. Results of weighted outcome models estimating the effect of earlier and later parent number talk on child calculation skills in 3rd grade.

Model and predictor	Effect	Standardized coefficient estimate	95% CI	*SE*	*t-ratio*	*Nominal p*	AICc
A. Control model							
Early talk	δ_1	1.846	[-1.878, 5.571]	1.858	0.994	0.325	
1. Separate predictors							526.6
Early talk	δ_1	1.802	[-2.205, 5.809]	1.997	0.902	0.371	
Later talk	δ_2	0.941	[-2.938, 4.820]	1.933	0.487	0.628	
	$\delta_2 - \delta_1$	-0.878	[-7.151, 5.429]	3.291	-0.267	0.120	
2. Constant effects							524.3
Cumulative talk	δ	2.292	[-1.345, 5.929]	1.813	1.264	0.212	
3. Later talk=0, Early talk>0							524.5
Early talk	δ_1	2.181	[-1.488, 5.850]	1.829	1.193	0.238	
4. Early talk=0, Later talk>0							525.1
Later talk	δ_2	1.617	[-1.959, 5.193]	1.783	0.907	0.369	
5. Simple model							**523.8**

Other Talk as a Predictor. The results of the models with parent other talk predicting calculation skills in 3rd grade are reported in Table 17. In Model A (control model), the coefficient for our measure of parent earlier other talk at 14 months was statistically significant (estimate=6.187, S.E=2.046, p=0.004). Model 1 (separate predictors model) shows that the effects between parent earlier other talk at 14 months (estimate=2.378, S.E.=2.633, p=0.371) and later talk at 38 months (estimate=4.051, S.E.=2.428, p=0.102) do not significantly differ (p=0.665). Our confidence in this result is weak for reasons similar to those discussed earlier. Model 2 (constant effects model) shows that for each a one unit increase in the standard deviation of our measure of cumulative parent other talk (1.14), we expect a 5.933 point increase in our measure of calculation skills in 3rd grade, which represents about 6 months of learning during a school year. The AICc for this model is 525.1.

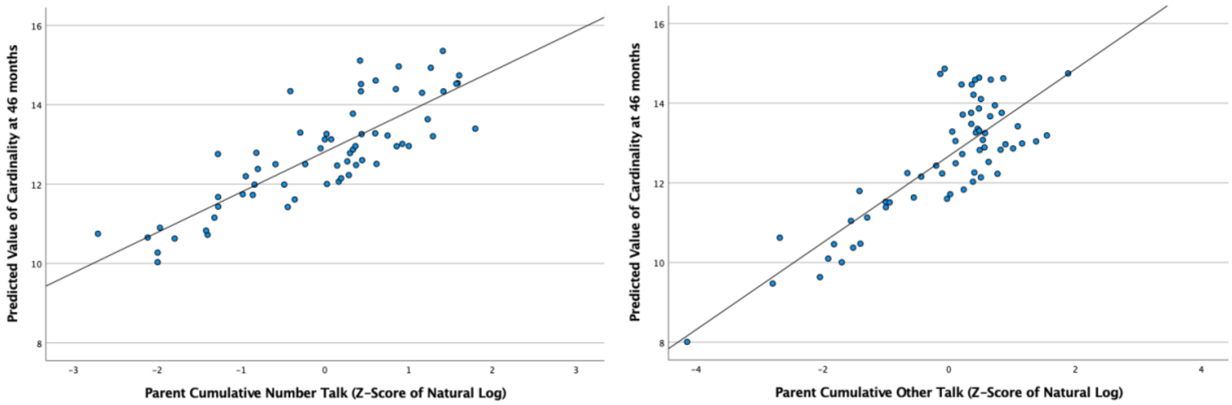
As an additional check of differing effects between parent earlier and later other talk, we ran two extreme models: Models 3 and 4. In Model 4, where earlier other talk is set to 0 and later other talk is greater than 0, the coefficient for later talk is 5.360 (i.e., about 5 months of learning during a school year) and the AICc is 525.9. We cannot rule out the possibility that other talk later is more important than earlier for a few reasons: in Model 1 (separate predictors model) the coefficient for later number talk (4.051) is nearly double that of earlier number talk (2.378), our test of differing effects shows a wide confidence interval [-6.873, 10.220], we have a small sample size, and the AICc in Models 2 (constant effects model) and 4 (an extreme model where parent talk earlier is set to 0 and parent talk later is greater than 0) are similar.

We conclude that high levels of constant parent other talk (i.e., other talk both earlier at 14 months and later at 38 months) positively impacts child calculation skills in 3rd grade. However, similar to our conclusions for the effect of parent other talk on cardinal number knowledge at 46 months, we cannot rule out the possibility that parent other talk provided later at 38 months is more important than parent other talk provided earlier at 14 months for calculation skills in 3rd grade. Sensitivity analysis is reported in Section S4 of the Appendix. Common support is reported in Section S5 of the Appendix.

Table 17. Results of weighted outcome models estimating the effect of earlier and later parent other talk on children's calculation skills in 3rd grade.

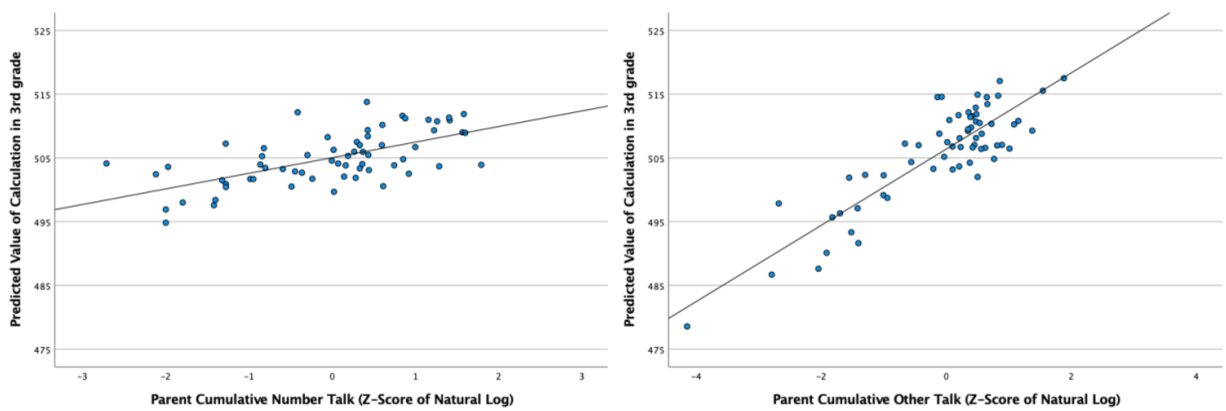
Model and predictor	Effect	Standardized coefficient estimate	95% CI	*SE*	*t-ratio*	*Nominal p*	AICc
A. Control model							
Early talk	δ_1	6.187	[2.083, 10.291]	2.046	3.023	0.004	
1. Separate predictors							527.2
Early talk	δ_1	2.378	[-2.913, 7.668]	2.633	0.903	0.371	
Later talk	δ_2	4.051	[-0.827, 8.929]	2.428	1.668	0.102	
	$\delta_2 - \delta_1$	1.885	[-6.873, 10.220]	4.332	0.435	0.665	
2. Constant effects							**525.1**
Cumulative talk	$\boldsymbol{\delta}$	**5.933**	**[1.867, 10.000]**	**2.027**	**2.927**	**0.005**	
3. Earlier talk is sufficient							528.2
Early talk	δ_1	4.996	[0.743, 9.250]	2.120	2.356	0.022	
4. Later talk is sufficient							525.9
Later talk	δ_2	5.360	[1.503, 9.217]	1.923	2.788	0.007	

Comparing Number and Other Talk Effects. In four sets of models, we tested the effects of parent number talk or other talk on child cardinal number knowledge at 46 months or calculation skills in 3rd grade. In all four sets of models, the constant effects model was either the best fitting or one of the best fitting models. Figures 10 and 11 show a visual representation of the effects of cumulative parent number and other talk on child math skills. Notice how the effect of cumulative number talk and cumulative other talk on cardinal number knowledge appear similar. However, the effect of cumulative other talk on calculation skills appears stronger than that of cumulative number talk. As mentioned earlier, we caution against strong conclusions against of no effect of parent number talk on calculation skills.



a. b.

Figure 10. Scatter plots with Y axes showing predicted values of child cardinal number knowledge at 46 months from weighted regressions of Model 2 (constant effects model) reported in Tables 14 and 15. X axes show the Z-score of the natural log transformation of (a) parent cumulative number talk and b) parent cumulative other talk.



a. b.

Figure 11. Scatter plots with Y axes showing predicted values of child calculation skills in 3rd grade from weighted regressions of Model 2 (constant effects model) reported in Tables 16 and 17. X axes show the Z-score of the natural log transformation of (a) parent cumulative number talk and b) parent cumulative other talk.

Discussion

For the first time, we show that the quantity of parent talk provided to toddlers causally impacts math skills years later, emphasizing the critical importance of parent talk provided during the first years of life for future math development. Specifically, our findings show a causal effect of both parent number talk and parent other talk on child cardinal number knowledge at 46 months. Further, our findings show a causal effect of parent other talk on child calculation skills in 3rd grade. Our results provide some suggestion that the optimal timing of parent talk may differ based on the type of parent talk (number talk or other talk) and based on the math skill assessed (cardinal number knowledge at 46 months or calculation skills in 3rd grade).

Effects of Parent Talk on Cardinal Number Knowledge

Our results provide evidence that the quantity of parent number talk and parent other talk provided both earlier at 14 months and later at 38 months positively impact child cardinal number knowledge at 46 months.

There was not strong evidence of differing effects between parent number talk provided earlier and later. Children learn the meanings of number words in a series of lengthy stages—starting with reciting the count list around age 2, followed by learning the meanings of number words "one" through "three" around age 3, and finally understanding the meanings of larger numbers, thus understanding the cardinal principle around age 4 (Carey & Barner, 2019; Sarnecka & Carey, 2008; Sarnecka & Lee, 2009; Wynn, 1990, 1992). Thus, it is possible that parent number talk provided earlier at 14 months supports children with the early stages, while parent number talk provided later at 38 months supports children with the later stages of cardinal number knowledge. It is also possible that the type of parent number talk provided earlier and later may differ. For example, parents might focus on smaller number words earlier and on larger number words later.

There was some suggestion that parent other talk provided later at 38 months, might be more important than parent other talk provided earlier at 14 months. This suggests that parents' overall talk, especially when provided later at 38 months, also helps children understand the meanings of number words. Whether certain aspects of parents' overall talk (e.g., a diverse vocabulary or the use of certain math terms) are particularly important is an open question for future research. Further, the type of parent other talk provided earlier and later may differ.

Effects of Parent Talk on Calculation Skills in 3rd grade

Our results provide evidence that parent other talk provided both earlier at 14 months and later at 38 months impacts child calculation skills in 3rd grade. While we did not see a statistically significant effect of parent number talk on calculation skills, we caution against strong conclusions of no effect, as our confidence in this result is weak, for reasons described in our Results section.

One possibility is that parent number talk, at 14 months and 38 months, is less impactful for child calculation skills in 3rd grade, than it is for child cardinal number knowledge at 46 months. Calculation is a more advanced math skill that is more formally taught in school, and children know the meanings of number words well before 3rd grade. Thus, parent number talk may no longer be important, but instead other language input captured by our measure of parent other talk, such as their explanation of what needs to be done to solve calculations might be more important.

A second possibility is that we simply did not have a large enough sample to show significant effects of parent number talk on child calculation skills in 3rd grade. A third possibility is that our measure of parent number talk was too broad, and that in fact certain types of parent number talk are particularly important for child calculation skills in 3rd grade, such as number words greater than 10 (which we did not include in our analyses) or number words used to describe complex math concepts. In sum, we cannot rule out the possibility that parent number talk, or specific types of parent number talk, provided at 14 and 38 months, is important for calculation skills in 3rd grade.

While our results provide evidence that parent other talk impacts child calculation skills in 3rd grade, it is possible that our measure of parent other talk is correlated with a more specific type of parent other talk that is particularly key. For example, parents' overall talkativeness might be linked to vocabulary input, syntax input, or the use of math terms. Indeed, future research should further breakdown parent other talk to determine whether parents' overall talkativeness is key, or whether certain aspects of parent other talk are key.

In terms of the timing of parent talk, for both number talk and other talk, our test of differing effects did not show significant differences between talk provided earlier at 14 months and later at 38 months. However, we see some suggestion that parent number talk provided earlier at 14 months may be more important than parent number talk provided later at 38 months. In contrast, we see some suggestion that parent other talk provided later may be more important than parent other talk provided earlier. At 38 months, parent other talk is likely more complex, and may be qualitatively different and more important than parent other talk at 14 months. Findings by Silvey et al. (2021) support this possibility, as parent syntax input was more important at 30 months than at 14 months for children's syntax skills in kindergarten. Moreover, child syntax skills are important for math skills, such as for understanding the operations involved in calculations (Espinas & Fuchs, 2022). Thus, for children's calculation skills, parent number talk may be more optimal when provided earlier at 14 months, while parent other talk may be more optimal when provided later at 38 months, and these two types of parent talk (number talk and other talk) may be supporting children through different stages important for their calculation skills.

Predictors of Parent Talk

Our findings contribute to our understanding of why some children receive more talk from their parents. We determined what parent and child characteristics were the most robust predictors of parent talk, based on the covariates that we weighted for to achieve balance between each of our four categories of parent talk (parent number talk and other talk at 14 months and 38 months) and each covariate. We described our stepwise procedure for this process in our Analytical Procedure section. The most robust predictors of parent number talk and other talk were household income, child gender, and child gesture types at 14 months. For parent other talk, child word types at 14 months and parent education were also important.

Household income positively predicted all four categories of parent talk, in line with findings from previous meta-analyses, suggesting that toddlers from higher SES backgrounds receive more talk than their peers from lower SES backgrounds. Boys received more number talk at 14 months and 38 months, and more other talk at 14 months, suggesting that perhaps gender stereotypes influence parents talk to their toddlers. Toddlers that produced more gesture types at 14 months received more parent other talk at 14 months and more number talk at 38 months. These findings suggest that toddlers' gestures encourage parents to provide more talk. Indeed, gesture is thought to be a mechanism through which children share thoughts they cannot yet express verbally to parents, and parents can in turn use this information to tailor their behaviors (see Goldin-Meadow, 2009). For example, parents have been shown to translate child gestures into words (Goldin-Meadow et al., 2007).

In addition, child word types at 14 months and parent education positively predicted parent other talk at 14 months and at 38 months, respectively. This further suggests that toddlers' early behaviors encourage parents to provide more talk, and children from higher SES backgrounds receive more parent talk than their peers from lower SES backgrounds.

In sum, as early as 14 months and 38 months, differences in the amount of parent talk provided are already present. Household income, child gender, and child gesture are particularly predictive of the amount of talk parents provide to their toddlers.

Limitations and Future Directions

Our findings are not without limitations. First, we could not include parent number talk and parent other talk in the same models due to their strong correlation in our data, which prevented us from directly comparing effects of parent number talk and parent other talk. In other words, our outcome models with parent number talk predicting child math skills did not account for parent other talk, and our outcome models with parent other talk predicting child math skills did not account for parent number talk. Second, our sample size was small, which may have limited our ability to detect true effects of parent number talk on calculation skills in 3^{rd} grade, as well as differing effects between earlier and later parent number talk and other talk on math skills. Third, while we accounted for important confounders, and our sensitivity analysis increases our confidence that our results are robust, with observational data, it is impossible to know the severity of bias resulting from failure to include unobserved confounders, such as executive functioning.

We encourage researchers to examine the effects of parent number talk and other talk provided at other time points. Our decision of examining parent talk at 14 months and 38 months was based on theoretical reasoning. However, examining parent talk that occurs after 38 months, during the preschool or elementary school years for example, is important. It is possible that for math skills assessed in elementary school, parent talk that occurs when children have begun formal schooling is more important than parent talk that occurs during the toddler years. We cannot rule out the possibility that parent talk that occurs at 38 months predicts parent talk that occurs even later, sometime before 3rd grade, and that it is talk that occurs after 38 months which affects calculation skills in 3rd grade. Under key assumptions, IPTW provides a way to tackle these questions, and improves our understanding of the optimal timing of parent talk.

While we examined effects of the quantity of parent talk, we did not examine effects of the quality of parent talk. We encourage researchers to use IPTW to further examine the types of parent number talk and other talk that are most beneficial for children's math learning. Evidence suggests that parent number talk that includes larger numbers or more advanced number functions (e.g., cardinality, ordinality) is more predictive of child math skills, such as cardinal number knowledge, numerical magnitude knowledge and word problem solving in preschool (Casey et al., 2018; Gunderson & Levine, 2011; Ramani et al., 2015). Because our measure of parent number talk did not include number words greater than 10, we may be obscuring a larger effect of parent number talk on child math skills, which may be particularly important for complex math skills, such as calculation. Our measure of parent other talk (overall talk excluding number talk) may have been too broad. Examining whether specific aspects of parent overall talk are particularly impactful (e.g., complex vocabulary) is important.

Our findings suggests that interventions that aim to improve child math achievement may be particularly effective if they focus in increasing naturalistic talk that parents provide to their toddlers. However, it is essential to ensure that interventions are culturally appropriate, as they are not a one-size-fits all. For instance, while a math app intervention improved the math achievement of children from higher SES backgrounds (Berkowitz et al., 2015; Schaeffer et al., 2018), it did not improve the math achievement of children from lower SES backgrounds, likely because families rarely used the app (Herts, 2020). Thus, it is essential to develop interventions that support parent math engagement in culturally appropriate ways that fit into the lives of families from diverse backgrounds. As Hunter and Civil (2021) suggest, strength-based approaches that build on parents' practices are important in this endeavor.

In sum, our findings suggest that there is a need to apply novel statistical approaches, such as IPTW to examine the effects of parent talk on child math skills, as these statistical approaches may improve our understanding of causal effects of parent talk and the optimal timing of parent talk, which could lead to the development of more effective interventions. Our study makes a unique contribution to the literature on child math development, as we applied a statistical method from epidemiology to show a causal effect of parent number talk and parent other talk to their toddlers on their math skills years later.

GENERAL DISCUSSION

General Summary

Together, the three studies in this dissertation expanded on links represented in the EMAA model in important ways.

In Study 1, results showed that child math anxiety from 1st grade predicted math achievement through 3rd grade, controlling for a foundational math skill (number line estimation). This finding suggests that math self-relevant math attitudes relate to math achievement starting at a young age. Thus, fostering positive math attitudes, in addition to ensuring children have the foundational math skills necessary for more complex math, is important for children's math achievement, at least in children from middle and higher SES backgrounds. Further, math anxiety was most predictive of math performance when math task difficulty was at the cusp of children's learning level. This finding has important implications for math achievement in school, as children typically work on math skills that are at the cusp of their math ability. Thus, math anxiety may be particularly impactful in school settings when children are learning and working on math that is at the cusp of their learning level. Because results from Study 1 provided some evidence that math anxiety may be quite impactful starting at a young age, it is important to understand how the link between self-relevant math attitudes and math achievement emerges to begin with.

In Study 2, we focused on families from low SES backgrounds and examined longitudinal relations among 1st grade children's self-relevant math attitudes, math achievement, and their parents' math attitudes. Results showed that child math achievement was a stronger predictor of math anxiety, than the reverse, in line with findings from other studies of elementary school children from middle and higher SES backgrounds (Cargnelutti et al., 2017; Ching et al., 2020; Gunderson et al., 2018). Thus, math achievement-related experiences may influence the development of positive or negative self-relevant math attitudes in young children, regardless of SES backgrounds. There was some suggestion that math anxiety might be less predictive of math achievement for young children from lower SES backgrounds, compared to children from higher SES backgrounds, however future research should test this more carefully in samples of children from a wide range of SES backgrounds. In addition, while average math achievement was very low for children in Study 2, math anxiety and math self-concept levels were similar to those reported in studies of 1st graders from higher SES backgrounds (Eccles et al., 1993; Gunderson et al., 2018; Pantoja et al., 2020). The big-fish-little-pond effect (i.e., equally achieving students have a lower self-concept when attending higher-performing schools than lower-performing schools; Marsh, 1987) could potentially explain why math attitude levels were similar.

Because young children spend a lot of time at home, in Study 2, we also examined the role of parent math attitudes. We observed a significant positive relation from parent math expectancy-value to child math achievement, even after accounting for prior child math achievement and self-relevant math attitudes. To our knowledge, this was the first study to control for prior child math achievement, when examining the relation of parent math expectancy-value to child future math achievement. Thus, fostering positive child-specific math attitudes in parents, may be one way to improve math achievement in children, regardless of SES backgrounds. Parent math anxiety did not significantly predict child math achievement, which is contrary to prior research showing that parent math anxiety predicts child math achievement in families from higher SES backgrounds (Berkowitz et al., 2015; Maloney et al., 2015; Schaeffer et al., 2018). However, this finding is in line with a study showing that parent math anxiety is less predictive of math behaviors in families from lower SES backgrounds (Berkowitz et al., 2021). In sum, our findings suggest that parent self-relevant math attitudes may be less consequential for children's math achievement in families from lower SES backgrounds, while parent child-specific math attitudes may be important predictors of child math achievement, regardless of SES backgrounds.

Given that Study 2 showed the importance of parents' math expectancy-value, which has been shown to predict parent behaviors (Simpkins et al., 2012; Wigfield et al., 2006, 2015), in Study 3, we focus on a specific parent behavior: the talk that they provide to their toddlers. We focused on families from diverse SES backgrounds and used inverse probability of treatment weighting (IPTW) to examine whether there were causal effects of parent number talk and parent other talk on two different math skills. We examined parent number talk and other talk earlier when toddlers were 14 months old, and later when toddlers were 38 months old. Results suggested that both parent number talk and other talk impact child cardinal number knowledge at 46 months. While parent number talk both earlier and later appeared to be of equal importance, there was some suggestion that parent other talk provided later at 38 months might be more important than parent other talk provide earlier at 14 months. Results also suggest that parent other talk impacts child calculation skills in 3rd grade, with some suggestion that parent other talk provided later might be more important than parent other talk provided earlier. While parent number talk did not significantly impact child calculation skills in 3rd grade, our confidence in this result is weak and we caution against strong conclusions of no effect. Further, there was some suggestion that for calculation skills in 3rd grade, parent number talk provided earlier may be more important than parent number talk provided later. In sum, parent talk causally effects child math achievement, but the optimal timing of parent talk may differ based on the type of parent talk provided (e.g., number talk or other talk) and based on the child math skill (e.g., cardinal number knowledge at 46 months or calculation skills in 3rd grade) assessed.

Future Directions

Findings from the three studies in this dissertation raise important questions for future research. For example, future research should further examine how the links examined in this dissertation differ based on a) social contexts or settings, such as the home and school environment, b) different types of parent talk (e.g., certain types of number talk) provided at different times (e.g., after 38 months), c) a broad range of SES backgrounds and d) change across development.

Study 1 suggests that math anxiety may be impactful as early as 1st grade, at in children from middle and higher SES backgrounds. However, research shows that self-relevant math attitudes may be less consequential for math achievement in adolescents from lower SES backgrounds and adolescents with lower math achievement (OECD, 2013). Consistent with findings from OECD (2013), in Study 2 there is some suggestion that math anxiety may be less consequential for young children from lower SES backgrounds. It will be important for future research to examine when math anxiety becomes at least somewhat impactful for children from a broad range of SES backgrounds, and when this relationship becomes more bidirectional. Further, additional research is needed to determine whether these relations differ depending on social context (e.g., home and school settings). Further, in Study 2, we did not observe a significant longitudinal relation between math self-concept and math achievement, and future research should examine when and how this relation emerges for children from diverse SES backgrounds, and how it changes across development.

Study 3 suggests that, taking parent and child characteristics into account (e.g., household income, child gender, and child gesture) the effects of parent talk may differ based on the type of parent talk provided and the type of child math skill assessed. For instance, there was some suggestion that parent other talk was more impactful than parent number talk for child calculation skills. Future work can further examine whether certain types of parent number talk (e.g., more complex number talk) or parent other talk (e.g., spatial talk, quantifiers, complex math conversations) are particularly important for children's math achievement, as well as the optimal timing of different types of parent talk. In other words, we examined the quantity of two broad categories of parent talk (number talk and other talk) but did not examine the quality of parent talk. Further, our findings are limited to two types of child math skills: cardinal number knowledge at 46 months and calculation skills in 3rd grade. Examining effects of parent talk on other math skills, such as word problem solving is also important.

We chose the timing of parent earlier talk and later talk based on theory. Specifically, parent talk earlier, was measured at a time when child talk and math skills are limited (14 months) and parent talk later was measured at a time when some children are beginning to understand the meanings of number words (38 months). Examining the effect of parent talk after 38 months on math skills assessed years later, is important. For calculation skills in 3rd grade, it is possible that parent talk provided when children have begun formal schooling is more important than parent talk provided during the toddler years. Future research can use IPTW to examine how the effects of parent talk observed in Study 3 may differ when provided during other stages of child math development.

The studies in this dissertation focused on parents, because toddlers and young children spend a lot of time with their parents. However, we did not examine the role of teachers. Future research should examine how both the home and school environment together play a role in early child math achievement. While evidence suggests that non-school factors (e.g., socioeconomic status) widens SES math achievement gaps, schools are thought to serve as important equalizers (Downey et al., 2004). One important question that future research could address regarding the role of parents and teachers is: if one key socializer (e.g., a parent or a teacher) has positive math attitudes, and the other has negative math attitudes, how does that predict child math achievement? If children receive high quality math instruction in school would this counteract the effect of decreased levels of parent talk at home?

Conclusions

In conclusion, the three studies in this dissertation expand on links represented in the EMAA model in important ways. We now have a better understanding of how impactful math anxiety might be for young children's math performance on tasks at the cusp of their math ability. However, future research should examine how the relation of math anxiety to math achievement may differ for young children from diverse, particularly low, SES backgrounds. Further, we have a better understanding of the emergence of the math achievement-attitude link, particularly in children from low SES background, whose math achievement is typically lower than that of their peers from higher SES backgrounds. Early math achievement likely plays an important role in the development of positive or negative math attitudes across SES levels. Parents' child-specific math attitudes likely play an important role in young children's math achievement across SES levels. Finally, the amount of parent talk provided to toddlers causally affects child math achievement years later. Children's early math achievement may provide insights into their future math attitudes, and their early experiences, such as their parents' math attitudes and behaviors. An important area for future research involves examining how links observed in this dissertation may differ based on social context (e.g., differences in the home and school environment). Another critical direction for future research is testing our findings in intervention studies, to determine whether changing factors such as child math attitudes or parent number talk can in turn change child math achievement.

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APPENDIX: SUPPLEMENTARY MATERIALS FOR STUDY 3

Section S1. Multiple Imputation

We addressed missing values via multiple imputation with predictive mean matching, using the *mice* library in R (van Buuren & Groothuis-Oudshoorn, 2011). Our procedure was very similar to that used by Silvey et al. (2021) with three exceptions. 1) We did not use auxiliary variables, which are used when there is a need to increase the robustness of imputation, as we had many variables in our dataset (see Table 12 in Study 3) and knew from previous research that the variables in our dataset with missing values were correlated with other variables in our dataset that were not missing those values (Dailey & Bergelson, 2021; Dearing et al., in prep; Silvey et al., 2021). 2) We used 20 datasets (i.e., iterations) as opposed to 5 datasets. While just 5-10 iterations can yield satisfactory performance (Brand 1999; van Buuren et al. 2006), it does not hurt to calculate extra iterations (Van Buuren & Groothuis-Oudshoorn, 2011). Using 20 iterations for less than 10–15% missing data is ample (van Buuren et al., 2006). 3) Because we wanted all missing data to be predicted based on all existing data, we did not use the "quickpred" function in R, in which missing data is predicted only based on existing data from variables with a correlation of $p<.10$.

Section S2. Correlations Among Key Variables

Table S1. Correlations among predictors

		Z1 Number Instances	**Z2 Number Instances**	**Z1 Other Tokens**	**Z2 Other Tokens**	**Cumulative Number Instances**
Z2 Number Instances	**Pearson Correl.** **Sig.** **N**	**.418**** **0.001** **61**				
Z1 Other Tokens	**Pearson Correl.** **Sig.** **N**	**.678**** **<0.001** **64**	**.334**** **0.009** **61**			
Z2 Other Tokens	**Pearson Correl.** **Sig.** **N**	**.554**** **<0.001** **61**	**.717**** **<0.001** **61**	**.672**** **<0.001** **61**		
Cumulative Number Instances	**Pearson Correl.** **Sig.** **N**			**.599**** **<0.001** **61**	**.761***** **<0.001** **61**	
Cumulative Other Instances	**Pearson Correl.** **Sig.** **N**	**.678***** **<0.001** **61**	**.549***** **<0.001** **61**			**.735***** **<.001** **61**

†p<0.1 $^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$

Table S2. Correlations among covariates

		Child gender (girl)	Child birth order	Child gesture types 14 months	Child word types 14 months	Parent income	Parent education	Parent IQ	X1 child number instances	X_1 child other tokens
Child birth order	**Pearson Correl.**	**-0.217†**								
	Sig.	**0.086**								
	N	**64**								
Child gesture types 14 months	**Pearson Correl.**	0.026	0.131							
	Sig.	0.84	0.301							
	N	64	64							
Child word types 14 months	**Pearson Correl.**	0.145	0.163	**0.579****						
	Sig.	0.253	0.198	**<0.001**						
	N	64	64	**64**						
Parent income	**Pearson Correl.**	-0.049	-0.068	0.161	0.041					
	Sig.	0.702	0.595	0.203	0.749					
	N	64	64	64	64					
Parent education	**Pearson Correl.**	**-0.216**	0.09	**0.305***	0.028	**0.493****				
	Sig.	**0.087**	0.478	**0.014**	0.827	**<0.001**				
	N	**64**	64	**64**	64	**64**				
Parent IQ	**Pearson Correl.**	-0.001	0.066	0.12	-0.224	**0.241**	**.492****			
	Sig.	0.997	0.645	0.402	0.113	**0.089**	**<0.001**			
	N	51	51	51	51	**51**	**51**			
X_1 child number instances	**Pearson Correl.**	-0.06	-0.09	0.161	0.029	**0.404****	**0.273***	0.084		
	Sig.	0.641	0.487	0.211	0.825	**0.001**	**0.032**	0.556		
	N	62	63	62	62	**62**	**62**	51		
X_1 child other tokens	**Pearson Correl.**	0.121	0.127	0.154	**0.216**	**0.355****	0.15	0.004	**0.549****	
	Sig.	0.351	0.327	0.233	**0.092**	**0.005**	0.245	0.975	**<0.001**	
	N	62	62	62	**62**	**62**	62	51	**62**	
Child age at time of 3rd grade calculation	**Pearson Correl.**	**-0.284***	0.005	0.033	-0.137	0.030	0.133	0.138	-0.034	**-0.216†**
	Sig.	**0.023**	0.967	0.794	0.281	0.815	0.295	0.335	0.791	**0.091**
	N	**64**	64	64	64	64	64	51	62	**62**

†p<0.1 $^{*}p < 0.05$ $^{**}p < 0.01$ $^{***}p < 0.001$

Table S3. Correlations between covariates and outcomes.

		Point-to-X (46 months)	WJ Calculation W-Score (3rd grade)
Child gender (girl)	**Pearson Correl.**	-0.022	**-0.364****
	Sig.	0.866	**0.009**
	N	59	**50**
Child birth order	**Pearson Correl.**	0.123	0.171
	Sig.	0.355	0.236
	N	59	50
Child gesture types at 14 months	**Pearson Correl.**	**0.319***	0.222
	Sig.	**0.014**	0.121
	N	**59**	50
Child word types at 14 months	**Pearson Correl.**	0.186	0.17
	Sig.	0.158	0.238
	N	59	50
Parent income	**Pearson Correl.**	**0.423****	**0.411****
	Sig.	**0.001**	**0.003**
	N	**59**	**50**
Parent education	**Pearson Correl.**	**0.380****	0.179
	Sig.	**0.003**	0.214
	N	59	50
Parent IQ	**Pearson Correl.**	**0.335***	0.184
	Sig.	**0.016**	0.214
	N	**51**	47
X_1 child number instances	**Pearson Correl.**	**0.537****	**0.440****
	Sig.	**<0.001**	**0.001**
	N	**59**	**50**
X_1 child other tokens	**Pearson Correl.**	**0.365****	**0.258†**
	Sig.	**0.004**	**0.071**
	N	**59**	**50**
Child age at time of Calculation in 3rd grade	**Pearson Correl.**	-0.016	-0.051
	Sig.	0.903	0.723
	N	59	50

†p<0.1 $\quad ^{*}p < 0.05 \quad ^{**}p < 0.01 \quad ^{***}p < 0.001$

Section S3. Checking Balance

Our procedure for checking balance was described in the Analytical Procedure section (Constructing the Weights section), and Results section (Achieving Balance Between Parent Talk and Covariates Section) of Study 3. The tables below show standardized coefficients and t-ratios of covariates before and after weighting.

Table S4. Bivariate relations between parent earlier number talk at 14 months(Z_{1N}) and baseline covariates (X_0), before and after weighting.[a, b]

Variable	Before weighting (controlling for child gesture types at 14 months)		After weighting for child gesture types at 14 months, household income, and child gender (controlling for child gesture types at 14 months)		After weighting for child gesture types at 14 months, household income, and child gender (controlling for child gesture types at 14 months and age at administration of Calculation in 3rd grade)	
	Standardized coefficient	***t*-ratio**	**Standardized coefficient**	***t*-ratio**	**Standardized coefficient**	***t*-ratio**
X_0, Child birth order	-0.157	-1.22	-0.114	-0.768	-0.122	-0.839
X_0, Child gender	-0.199	-1.712	-0.092	-0.664	-0.054	-0.446
X_0, Child word types at 14 months	0.065	-0.587	0.120	1.070	0.138	1.197
X_0, Parent IQ	0.039	0.300	0.012	0.093	-0.033	-0.233
X_0, Parent education	0.2	1.372	0.011	0.080	-0.002	-0.016
X_0, Household income	0.304	2.417	0.021	0.143	0.018	0.124

[a.] Standardized coefficients and *t*-ratios are reported from regressions predicting each covariate X_0 from Z_{1N} controlling for gesture, *without weighting* (columns 2 and 3) and *with weighting* (for gesture, household income, and child gender; columns 4, 5, 6, and 7).

[b.] Standardized coefficients and *t*-ratios are reported *not controlling for age at administration of Calculation in 3rd grade* (columns 2, 3, 4, and 5) and *controlling for age at administration of Calculation in 3rd grade* (columns 6 and 7)

Table S5. Bivariate relations between parent later number talk at 38 months (Z_{2N}), baseline covariates (X_0), and time-varying covariate (X_{1N}) before and after weighting.[a, b]

Variable	Before weighting (controlling for Z_{1N} and child gesture types at 14 months)		After weighting for Z_{1N}, child gesture types at 14 months, child gender, and household income (controlling for Z_{1N} and child gesture types at 14 months)		After weighting for Z_{1N}, child gesture types at 14 months, child gender, and household income (controlling for Z_{1N}, child gesture types at 14 months, and age at administration of Calculation in 3rd grade)	
	Standardized coefficient	***t*-ratio**	**Standardized coefficient**	***t*-ratio**	**Standardized coefficient**	***t*-ratio**
X_{1N}, Child number instances at 34 months	0.032	0.183	-0.180	-1.038	-0.173	-0.984
X_0, Child birth order	1.546	0.113	-0.091	-0.592	-0.100	-0.672
X_0, Child gender	-0.251	-1.898	0.009	0.051	0.028	0.174
X_0, Child word types at 14 months	-0.135	-1.133	0.011	0.089	0.023	0.185
X_0, Parent IQ	0.060	0.358	-0.041	-0.216	-0.057	-0.315
X_0, Parent education	0.140	1.107	-0.003	-0.022	-0.010	-0.071
X_0, Household income	0.188	1.440	0.028	0.189	0.028	0.187

[a.] Standardized coefficients and *t*-ratios are reported from regressions predicting each covariate X_0 from Z_{2N} controlling for Z_{1N} and gesture, *without weighting* (columns 2 and 3) and *with weighting* (for Z_{1N}, gesture, and household income; columns 4, 5, 6, and 7).

[b.] Standardized coefficients and *t*-ratios are reported *not controlling for age at administration of Calculation in 3rd grade* (columns 2, 3, 4, and 5) and *controlling for age at administration of Calculation in 3rd grade* (columns 6 and 7).

Table S6. Bivariate relations between parent earlier other talk at 14 months (Z_{1O}) and baseline covariates (X_0), before and after weighting.[a, b]

Variable	**Before weighting (controlling for child gesture types at 14 months)**		**After weighting for child gesture types at 14 months, child word types at 14 months, child gender, and parent education (controlling for child gesture types at 14 months)**		**After weighting for child gesture types at 14 months, child word types at 14 months, child gender, and parent education (controlling for child gesture types at 14 months and age at administration of Calculation in 3rd grade)**	
	Standardized coefficient	***t*-ratio**	**Standardized coefficient**	***t*-ratio**	**Standardized coefficient**	***t*-ratio**
X_0, Child birth order	-0.131	-1.115	-0.012	-0.074	0.006	0.032
X_0, Child gender	-0.259	-2.782	-0.205	-1.471	-0.123	-0.880
X_0, Child word types at 14 months	-0.182	-2.183	-0.059	-0.812	-0.019	-0.224
X_0, Parent IQ	0.331	2.432	0.224	1.404	0.193	1.163
X_0, Parent education	0.358	2.693	-0.135	-0.727	-0.178	-0.882
X_0, Household income	0.370	2.923	0.220	0.976	0.227	0.968

[a.] Standardized coefficients and *t*-ratios are reported from regressions predicting each covariate X_0 from Z_{1O}, *without weighting* (columns 2 and 3) and *with weighting* (for parent education, child gender, and child gesture types at 14 months; columns 4, 5, 6, and 7).
[b.] Standardized coefficients and *t*-ratios are reported *not controlling for age at administration of Calculation in 3rd grade* (columns 2, 3, 4, and 5) and *controlling for age at administration of Calculation in 3rd grade* (columns 6 and 7).

Table S7. Bivariate relations between parent later other talk at 38 months (Z_{2O}), baseline covariates (X_0), and time-varying covariate (X_{1O}) before and after weighting.[a, b]

Variable	**Before weighting (controlling for Z_{1O} and child gesture types at 14 months)**		**After weighting for Z_{1O}, child gesture types at 14 months, household income and parent education (controlling for Z_{1O} and child gesture types at 14 months)**		**After weighting for Z_{1O}, child gesture types at 14 months, household income and parent education (controlling for Z_{1O}, child gesture types at 14 months and age at administration of Calculation in 3rd grade)**	
	Standardized coefficient	***t*-ratio**	**Standardized coefficient**	***t*-ratio**	**Standardized coefficient**	***t*-ratio**
X_{1O}, Child other tokens (34 months)	0.232	1.490	0.106	0.623	0.117	0.805
X_0, Child birth order	-0.094	-0.550	-0.132	-0.700	-0.129	-0.695
X_0, Child gender	-0.190	-1.257	0.028	0.174	0.028	0.174
X_0, Child word types at 14 months	-0.054	-0.457	-0.002	-0.018	-0.001	-0.006
X_0, Parent IQ	-0.014	-0.087	-0.140	-0.701	-0.145	-0.698
X_0, Parent education	0.036	0.192	-0.103	-0.515	-0.107	-0.530
X_0, Household income	0.349	2.371	0.129	0.628	0.130	0.634

[a.] Standardized coefficients and *t*-ratios are reported from regressions predicting each covariate X_0 from Z_{2N} controlling for Z_{1N}, *without weighting* (columns 2 and 3) and *with weighting* (for Z_{1O} and household income; columns 4, 5, 6, and 7).
[b.] Standardized coefficients and *t*-ratios are reported *not controlling for age at administration of Calculation in 3rd grade* (columns 2, 3, 4, and 5) and *controlling for age at administration of Calculation in 3rd grade* (columns 6 and 7).

Section S4. Sensitivity Analysis

To assess the sensitivity of our results, we checked the level of bias we would expect if there were confounders we did not measure, in line with Silvey et al. (2021). As described in our Analytical Procedure section, we emphasize that this approach is a very partial specification check and does not provide proof that our results are valid. It is impossible to know the severity of bias resulting from failure to include unobserved confounders.

The most important confounders for parent talk and child math skills that have been reported are confounders we observe: parent income and education (Dailey & Bergelson, 2021; Dearing et al., in prep; Levine et al., 2010). While we do not expect to have unobserved confounders stronger than parent income and education, there may be weaker unobserved confounders. Would failing to control for covariates that prior research has not controlled for disguise the fact that our results may not be robust to unobserved confounding? The goal of the sensitivity analysis is to understand whether our results are robust (i.e., insensitive), or whether they are not robust (i.e., sensitive) to failure of including unobserved covariates.

Sensitivity analysis in observational studies is important. In an experiment, all confounding would be controlled for through random assignment. There likely are unobserved confounders that we did not control for in the current study (e.g., executive functioning), therefore it is important to understand whether failure to control for these unobserved confounders would qualitatively change our conclusions. We want to know how robust (i.e., insensitive) our results are to the failure of including unobserved confounders. Rather than using simulated covariates variables, we use existing baseline covariates in our dataset (parent income, education and IQ, and child gender, birth order, word types and gesture types). Based on the literature, it is unlikely that there are covariates stronger than these that we did not observe, as these covariates have been found to be strong predictors of parent talk (Dailey & Bergelson, 2021; Dearing et al., in prep; Levine et al., 2010; Silvey et al., 2021).

We examined the range of possible biases that would stem from the existence of unobserved covariates with confounding effects similar to those of the covariates we did observe, once the effects of parent income and education are accounted for. If controlling for income and education alone is enough to remove confounding, then we consider our results to be robust (i.e., insensitive to unobservable confounders). Of note, our approach is a partial specification test of sensitivity and does not serve as proof that our results are valid, as we cannot know the severity of bias resulting from excluding unobserved covariates.

For our sensitivity analysis, we assume our best-fitting model—the constant effects model—in each of the three cases where we observed a statistically significant effect of parent talk: the effect of number talk cardinality, the effect of other talk on cardinality and the effect of other talk on calculation.

First, we want to know the level of expected bias from unobserved confounders. We calculate the estimated bias of each of our observed covariates—which we are treating as unobserved covariates—except parent income and education. For details on how to derive this information, see Silvey et al. (2021). Then, we look at the unobserved confounder with the largest bias. For the effects of both cumulative number talk and cumulative other talk on cardinality, this was child gesture types at 14 months. For the effect of cumulative other talk on calculation, this was child gender.

Second, we want to see whether the effect of parent talk adjusted for bias, is smaller than the original effect in our constant effects models. To examine this, we simply take the original coefficient from our best-fitting outcome models and divide it by the coefficient corrected for the bias of the strongest unobserved confounder. For details on how to derive the coefficient corrected for the bias, see Silvey et al. (2021).

For the effect of number talk on cardinal number knowledge at 46 months, the original coefficient of constant number talk is 0.957 (see Table 14), while the estimate corrected for the bias of child gesture types at 14 months is 0.904. Excluding a confounder with bias similar to child gesture types would not exaggerate the estimate of the effect of number talk by more than about 6%. This would not qualitatively change our conclusions, and the effect of number talk on cardinality would still be statistically significant.

For the effect of parent other talk on cardinal number knowledge at 46 months, the original coefficient of constant other talk is 0.965 (see Table 15), while the estimate corrected for the bias of child gesture types at 14 months is 0.859. Excluding a confounder with bias similar to child gesture types would not exaggerate the estimate of the effect of other talk by more than about 12%. This would not qualitatively change our conclusions, and the effect of other talk on cardinality would still be statistically significant.

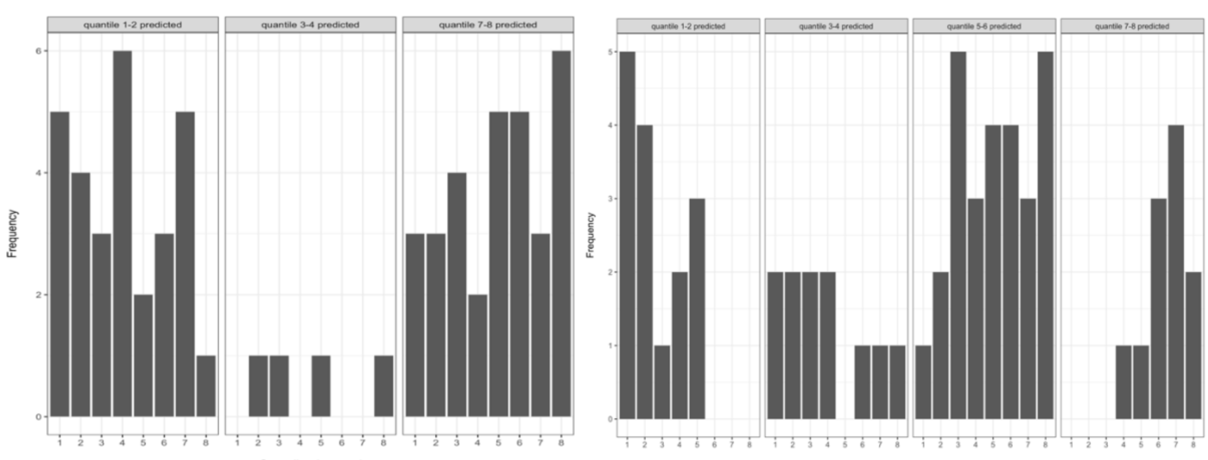
Finally, for the effect of parent other talk on calculation skills in 3rd grade, the original coefficient of constant other talk is 5.933 (see Table 17), while the estimate corrected for the bias of child gender is 5.076. Excluding a confounder with bias similar to child gender would exaggerate the estimate of the effect of other talk by more than about 17%. This would not qualitatively change our conclusions, and the effect of other talk on calculation would still be statistically significant.

In sum, based on our sensitivity analyses, our conclusions appear to be robust (i.e., insensitive to unobserved confounders), and increase our confidence in our results. To reiterate, we cannot know with certainty the true effect of unobserved confounders, as it is impossible to measure this.

Section S5. Common Support

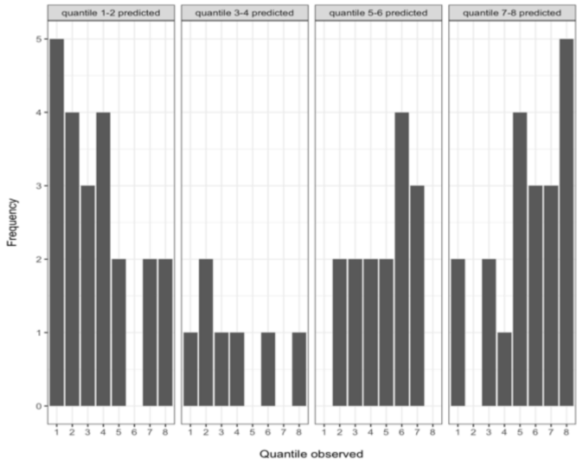
For this analysis, we chose 1 dataset at random to graph results. The procedure described here was done for each of our four categories of parent talk: parent number talk and other talk provided earlier at 14 months and later at 38 months. Figures S2 (a and b) and S3 (a and b; a figure for each category of parent talk) show the distribution of observed parent talk (8 quantiles) for each of four categories of children: those predicted, based on their covariates to have a) low talk (quantiles 1-2 predicted), b) low-mid talk (quantiles 3-4 predicted), high-mid talk (quantiles 5-6 predicted), and high talk (quantiles 7-8 predicted). We have common support when there is variability in the range of observed talk for children in each category of predicted talk. In other words, when subsets of children who have a similar probability of a certain amount of talk, vary in their observed talk (see Hong, 2012; Silvey et al., 2021). Lack of common support would mean that some subsets of children have no information about the impact of different types of talk. For example, those with low talk might have no comparison group.

Figure S1. Common support for estimating the effect of a. earlier number talk Z_{1N} (parent number talk at 14 months), and b. later number talk Z_{2N} (parent number talk at 38 months).

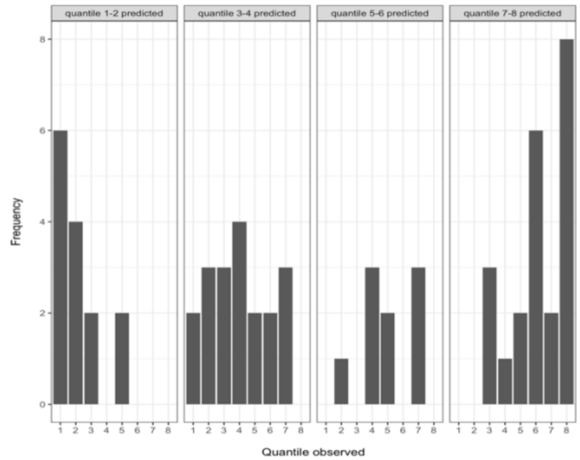


a. b.

Figure S2. Common support for estimating the effect of a. earlier other talk Z_{1O} (parent other talk at 14 months), and b. later other talk Z_{2O} (parent other talk at 38 months).



a.



b.