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Boosting Parent-Child Math Engagement and Preschool Children's Math Skills: Evidence from an RCT with Low-Income Families☆

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ABSTRACT

Math skill in early childhood is a key predictor of future academic achievement. Parental engagement in math learning contributes to the growth of children's math skills during this period. To help boost parent-child engagement in math activities and children's math skills, we conducted an RCT lasting 12 weeks with 758 low-income preschoolers (3-5 years old) and their primary caregivers. Parents were randomized into five groups: 1) a control group, and groups that received 2) a digital tablet with math apps for children; 3) analog math materials for parents to use with children, 4) analog math materials with weekly text messages to manage parents' present bias; and 5) analog math materials with weekly text messages to increase parents' growth mindset. Relative to the control group, neither the analog math materials alone nor the analog materials with growth mindset messages increased child math skills during the intervention period. However, the analog math materials combined with messaging to manage present bias and the digital tablet with math apps increased child math skills by about 0.20 standard deviations (p=.10) measured six months after the intervention. These two treatments also significantly increased parents' self-reported time engaged in math activities with their children.

1. Introduction

Math skill in early childhood is central to academic achievement later in life (Duncan et al., 2007; Reynolds, 1994). Income-based math achievement gaps often emerge even before the start of formal schooling (Heckman, 2006; Waldfogel & Washbrook, 2011). One reason for differences in children's math skill is differences in how much parents engage their children in math-related learning activities at home. The importance of parental[1] engagement to children's skill development has long been recognized by economists (e.g., Becker, 1965; Hill & Stafford, 1974; Leibowitz, 1974 and 1977) and other social scientists (Coleman, 1966),[2] though much of the existing evidence relies on survey or time diary evidence and statistical techniques to approximate causality (i.e., Bernal, 2008; Bernal and Keane, 2011; Carneiro and Rodriguez, 2010; Del Boca et al., 2012; Del Bono et al., 2016; Fiorini and Keane, 2014; Houtenville and Conway, 2008; Hsin and Felfe, 2014; and

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[1] Throughout this paper we use the term "parent" to refer to any caregiver for a child in our sample. In 2020 4% of caregivers are someone other than the child's mother or father.

[2] Note that there are large literatures on parent's involvement in their children's schools, on the general question of whether parents matter, and on parenting style; these are not addressed here.

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Villena-Roldan and Ríos-Aguilar, 2012). Research shows parents engage their young children in relatively little math learning in the home environment and this is especially true for low-income parents.[3]

In the present study ("Math for Parents and Children Together or "MPACT"), we implement a 12-week RCT with 758 low-income preschoolers (3-5 years old) and their families to boost parent-child engagement in math learning and children's math skills. We aim to test whether high-quality digital apps and analog math materials could increase parental math engagement and child skills and whether the impact is enhanced with text messages aimed at managing parents' behavioral biases. We recruited 30 publicly subsidized preschools throughout the City of Chicago to participate in MPACT. Our randomization was stratified by classroom and students within the classroom were assigned into five groups: 1) a control group, and groups that received 2) a digital tablet with high-quality math apps for children to use with or without parents; 3) high-quality analog math materials (the "MKit") for parents to use with children, 4) the Mkit plus weekly text messages to manage parents' present bias; and 5) the Mkit plus with weekly text messages to increase parents' growth mindset. To capture any changes in parent-child engagement in math learning and children's math skills, we assessed children's math skills using the Woodcock-Johnson test at baseline, the end of the 12-week intervention, and six months after the intervention ended; we also collected a self-reported measure of parental time spent on math engagement at the baseline and the end of the intervention.

Relative to the control group, both the math app treatment and the MKit plus present-bias treatment increased children's math skills by about 0.20 standard deviations (p=.10) six months after the intervention ended while the other treatments did not increase children's math skill. These two treatments that increase math skill also significantly increased parents' reports of the time that they spent engaged in math activities with their children (by a quarter and a third of a standard deviation) while the two treatments that did not increase math skill also did not increase parents' reports of the time they spend in math activities with their children. A plausible hypothesis is that the increase in parent engagement is a mechanism leading to an increase in math skill.

To study why the MKit and growth mindset treatments fail to raise math engagement and test scores, we examine questions in the parent surveys and show that (1) 17% of parents in MKit group reported losing the MKIT and many fewer parents in the MKit group said they completed more than half of the activities compared to other parents with the MKit, and (2) most parents already have a high level of growth mindset at baseline, which substantially reduces the marginal benefits of any intervention that tries to cultivate a growth mindset.

This paper contributes to several literatures. First, we contribute to the literature studying parental engagement and children's skills by providing plausible evidence that an increase in parent engagement is associated with an increase in math skill. This is important because most of the evidence on this relationship focuses on the impact of parent-child reading on children's literacy scores (e.g., see Barone et al., 2019). Math skills appear to be especially important to children's later achievement (Duncan et al., 2007) but the barriers to early math skill development may be different from the barriers to literacy skill development. For example, more parents have math anxiety than anxiety about literacy and evidence suggests that parents with math anxiety can induce this anxiety in their children (Maloney, et al., 2014). Compared to information about how to improve children's literacy skill, information from experts, including scholars, teachers, and other practitioners, about how to improve children's math skill is somewhat abstract. Advice about improving literacy skill mainly includes reading to your child every day. In contrast, advice about improving children's math skill urges parents to engage in "math talk," read stories with math-related content, play board games, and do other activities that develop math skills among preschoolers.[4] There is a vast difference in the cognitive load and the need for prior knowledge to execute the literacy advice (read a book everyday) compared to this math advice. In addition, the materials needed to help a child build literacy skill is clear—books—while it is much less clear what materials are needed to help a child build math skill.

Our results also add to the nascent but growing research showing that parents' present bias is an important impediment to parents' investments in their child's skill development (Mayer et al., 2019; Kalil et al., 2023). Research has shown that disadvantaged parents, such as those in MPACT, face a host of stressors, such as income instability, childcare problems, or transportation issues, that place cognitive and emotional demands on their attention in the present and leave little energy for thinking about the future (Gennetian & Shafir, 2015; Mani et al., 2013; Shah et al. 2022; Spears, 2011) resulting in a greater possibility for the procrastination that results from present bias. Their children also have lower scores on reading comprehension tests.

Mayer et al. (2019) find that a treatment to reduce present bias increased the amount of time that parents spent reading to their child using a digital library on an electronic tablet by 1 standard deviation. This treatment impact was much greater for parents who are more present-biased than for parents who are less present-biased. Kalil et al. (2023) also demonstrated experimentally that present-bias messages designed to boost reading time among low-income parents of young children led to a .32 SD increase in parent reading time over an 11-month period. We contribute to this literature by documenting that our present-bias treatment arm also is effective for boosting parent-child math engagement and child math skills.

Lastly, this study adds to the literature on the role of technology in human capital development (see Escueta, Nickow, Oreopoulos, & Quan, 2020 for a synthesis of experimental evidence). Little research exists on whether technology can increase young children's skills. To the extent that technology is high quality, fun, and engaging it may directly increase skill without increasing parent engagement or it also might reduce frictions to parent effort, thereby increasing the time that parents are engaged and/or the quality of the time they spend in educational activities with their children. The MPACT results highlight the role that technology could play in raising the skills of disadvantaged preschool aged children.

The remainder of this paper proceeds as follows. Section 2 describes the MPACT experiment. Section 3 discusses data and descriptive results. Section 4 discusses the experimental findings, and Section 5 concludes with discussion.

2. The MPACT Experiment

The 12-week MPACT intervention was completed in three rounds, with each round consisting of about a third of the total sample of children. MPACT was done in rounds because of the intensity of the recruitment process and the costs to our research team associated with giving families electronic tablets. The rounds began in fall of 2017, spring of 2018, and fall of 2018. We conducted all aspects of MPACT in Spanish and English.

[3] A large research literature documents the correlation between the home learning environment and children's skills (e.g., Kleemans et al., 2012; Niklas & Schneider, 2014; LeFevre et al., 2009, Marinova et al., 2021).

[4] For example, a National Academy of Sciences report on math learning (National Research Council, 2009) advises that parents should "observe their children carefully, seeing what they do and encouraging and extending their fledgling use of number symbols and processing; Say the number word list. For example, they can count small food items or the number of cups at the table; Ask children to tell them about their problem solving. For example, they can ask 'What did you mean by that?' or 'Why did you do it that way?'; Engage in activities that involve playing with blocks, building things, and board games."

2.1. Design of Treatment Arms

An important potential explanation for why low-income children begin kindergarten with lower math skills than higher income children is that low-income parents lack information about developmentally appropriate ways to engage their children in math learning. Three of the MPACT treatments provided parents with an "MKit." The MKit included a math activity booklet with twenty-two developmentally appropriate math activities for parents and children to do together, a game board and game pieces, a number grid, and a goal tracker.[5] The MKit also included instructions for how to do the activities and tips for how to get the most out of the activities.[6] These materials were given to families in an MKit backpack. [7] The MKit booklet focused on five specific skill areas within the numeracy domain: number recognition, counting, comparing size and quantity, adding and subtracting, and patterns.

The materials in the MKit were developed by our research team and informed by the recommendations in chapters 4 and 5 of a report by the National Academy of Sciences (Cross et al., 2009) and other professional recommendations for how parent can engage with their children to increase child numeracy skills. The content of the MKit was extensively piloted to assure that parents understood the activities and were enthusiastic about doing them. At the follow-up survey of parents (described below), 92% of parents said that they would recommend the MKit to a friend, and over 80% disagreed with the statement, "I did not need the MKit because I already know how to do math activities with my child." When asked how much they would be willing to pay for the MKit, 74% said they were willing to pay $10 and over 40% said they were willing to pay $20. Parents in the MKit treatment group received only the MKit. This treatment was intended to test whether providing materials alone to parents would improve children's math skills.

Because procrastination induced by present bias is another potential reason that low-income parents engage their children less in math learning activities, a second treatment group, the "present bias group," received an MKit plus up to four text messages per week intended to overcome parents' present bias. By reducing parents' procrastination in engaging their child, the present bias treatment was intended to increase children's math skill more than the MKit only treatment.[8] Although the wording of the messages differed each week, all the text messages were meant to either encourage parents to set a goal for how much time they would engage their children in math activities and to stick to the goal. An example of a message in this treatment is, "It's Goal Day. Set a goal for how many days this week you will do math activities with [CHILD_NAME]. Ask [him/her] to help you write it down on the MKit chart."

A third treatment group, the "growth mindset group," received an MKit plus up to four text messages per week that were intended to promote a growth mindset within the parent. While individuals with a growth mindset believe that skill can be developed through information and hard work, individuals with a fixed mindset believe that skills are inherent and not changeable. Individuals with a fixed mindset tend to achieve less than those with a growth mindset (Blackwell et al., 2007; Claro et al., 2016; Mangels et al., 2006). Most research on growth mindset has focused on people's mindsets about their own intelligence and many studies have focused on students' own mindset (and not their parents' mindset). Parents with a fixed mindset might invest less in engaging their child in learning activities because they will expect a lower return on that investment than parents with a growth mindset. The few studies that have considered the role of parental mindset on child achievement have found that having parents with growth mindsets is associated with greater child achievement (Haimovitz & Dweck, 2016; Rowe & Leech, 2019; Andersen & Nielsen, 2016) and persistence (Song, Barger, & Bub, 2022). There is some evidence that parents with higher socioeconomic status have higher levels of a growth mindset (Song et al. 2022). An example of a growth mindset message in our study is "Math ability is not fixed. If you talk about math with [CHILD_NAME] every day, [his/her] math ability will grow.[9]

Parents in the fourth treatment group, the "math app group," did not receive an MKit. Instead, they received an electronic tablet preloaded with eight apps intended to teach math skills to three-to-five-year-old children. The math apps could be used by the child alone or by the parent and child together. Thus, this treatment could increase child math skills either because the child learned directly from the apps or because the apps increase the parent-child engagement.[10] We reviewed dozens of math applications for three-to-five-year-old children and selected applications that were available in both Spanish and English; worked on the tablets that we selected, with few problems; and covered roughly the same math skills as the MKit. We piloted the applications to ensure that children could use the tablets and the apps with few technical issues, and that the applications were enjoyable. None of the math applications that we reviewed required parent engagement with the child to use the application, but we did not instruct parents that the apps were exclusively for the child's use. The choice of the math apps is described in the Appendix.

Parents in the control group only received an MPACT backpack with a story book rather than math materials at the beginning of the intervention. They also received text messages reminding them when they would be surveyed and when their child would be assessed. The Appendix includes an excerpt from the MKit.

2.2. Randomization

We recruited 30 publicly subsidized preschools throughout the City of Chicago to participate in MPACT, 17 from Head Start and 13 from Chicago Public Schools. In these preschools eligible parents had a child between the ages of three and four years who was enrolled in the preschool, had a primary language of either English or Spanish, and who was willing to consent to participate. Parents also had to have a mobile phone and be willing to receive text messages.[11] Language restrictions were due to limitations on the languages in which we could produce the MKit and find suitable electronic applications.

In the eleven centers that allowed it, all eligible parents were automatically enrolled in MPACT but were given a chance to opt out of participation.[12] Eighteen preschools did not permit opt-out recruitment. In these centers, research assistants recruited parents in person by approaching them at drop-off and pick-up time to ask if they would be

[5] The goal tracker is a grid in which parents could keep track of the days on which they did math with their children

[6] The MKit also provided suggestions for how to make the activity harder after the child mastered the skill. The booklet families received also included information on the importance of parents spending time in math activities with their child for the child's future success.

[7] An example of an activity in the booklet instructs the parent to ask the child to choose a number between 1 and 10 and then along with the child to make up a dance with that number.

[8] For more literature studying present bias, please see Meier & Sprenger, 2010; Eckel et al., 2005 on overall effects, Chabris et al., 2008 for dieting, exercising, and smoking and Sutter et al., 2013; Castillo et al., 2011 for investments in human capital.

[9] All messages of the present bias and the growth mindset groups are available upon request for replication purposes.

[10] Tablets were set up so that parents could not access the internet from the tablets; therefore, no other apps or materials could be downloaded.

[11] In other studies that we have done with a similar sample of Head Start parents, about 90% of parents reported having a working phone on which they could receive text messages (Kalil et al., 2019)

[12] Only the principal parent or guardian participated in MPACT.

willing to participate. Participation rates were high at both opt-out (99%) and opt-in (71%) preschool centers.[13]

We conducted a stratified randomization in two stages. In the first stage, we randomly assigned preschool classrooms to either a treated classroom or a control classroom. We assigned 15 classrooms (5 in each round) out of 181 classrooms to be a control classroom. All sample children in these classrooms were assigned to the control group. In the second stage, we randomly assigned students in the treatment classrooms to one of the treatment groups or the control group. We implemented this two-stage randomization so that we could detect peer effects, which we did not find evidence of and will not discuss in this paper.

3. Data and Descriptive Results

The sample for estimating treatment effects on child math skill includes children who were assessed at baseline, the end of the 12-week intervention, and six months after the end of the intervention, who had no learning disability identified by either the assessor or the preschool, and who were able to complete the assessments. We assessed children at the end of the intervention to evaluate the efficacy of treatments. We also conducted a follow-up assessment six months after the intervention to explore changes in children's math skills without active interventions.

We initially recruited 1459 children. Eleven children dropped out before randomization. Of the remaining 1448 children, 95 were siblings of enrolled children. We dropped siblings, leaving 1353 children in the sample to randomize. After randomization, 59 children dropped out of the study. These children were mainly enrolled in a preschool that closed after randomization but before we could collect any data on either parents or children. Ten children remained enrolled in the preschool but were chronically absent, so we were not able to collect data on either these parents or children. Another 97 children could either not be assessed reliably because of either cognitive or behavioral problems or because they were reported as having learning abilities by their primary caregivers. Improving these children's math skills is beyond our interventions' scope, so we drop them from the analytical sample as well. This left 1187 children who were assessed at baseline, which is composed of 349 children from the first round, 408 children from the second round, and 430 children from the third round.

The first column in Table A1 shows that children were randomized evenly across the MKit, growth mindset and present bias treatment groups. The control group is larger because all the children in the control group classrooms were assigned to the control group. We assigned fewer children (122) to the math app group because of the high cost of purchasing, distributing, and reclaiming tablets compared to the cost of the MKit, which families kept at the end of the treatment.

Column 2 in Table A1 shows that of the original 1187 children who were assessed at baseline, 758 (63.9%) had data on baseline and two rounds of follow-up assessments. This is the sample that we use in our primary analysis below. The percent of children with complete data in the sample ranged from 59% for the math app group to 67.8% for the growth mindset group.

We also surveyed the parents of these children at both baseline and at the end of the 12-week intervention. Data on demographic characteristics of children and their families come from this survey data as do parents' reports of how much time they spend engaged with their children in math learning activities. All parents of the 1187 children who were initially assessed were asked to take the baseline parent survey. Of this sample, 896 parents participated in the baseline survey. As with the child sample, parents were evenly distributed across the MKit, growth mindset, and present bias treatment groups with more parents in the control group and fewer in the math app group (Table A1, Column 3). Among parents, 721 participated in both the baseline and end of intervention survey (Table A1, Column 4). This represents 60.7% of the original sample of parents and 80.5% of the parents who took the baseline survey. The 721 parents who participated in both surveys are the parent sample used in the analyses below.

To address the potential for selective attrition, we compared attrition rates by treatment group from randomization to six months after the end of the intervention. A joint f-test shows no significant difference of attrition rates across treatment and control groups. Further, we compared several characteristics of the child sample at baseline, the end of the intervention, and six months after the end of the intervention. We also compared several characteristics of the parent samples at baseline and at the end of the intervention. In neither case were any of the characteristics statistically significantly different across samples at $p=.10$. These results are shown in tables A2, A3 and A4.

Our primary outcome is math skills. To measure this, we use the Woodcock-Johnson IV Applied Problems subtest (WJ). As noted above, the MKit focuses primarily on numeracy. This WJ subtest also focuses on numeracy, including counting, subitizing (i.e., ability to recognize the number of objects without counting them), adding and subtracting, sequencing, and matching numbers to their quantities. This contrasts with geometric and spatial competencies, which includes recognizing and naming shapes, construction of shapes, spatial imagery, and measurement. Children were assessed at the baseline, the end of the intervention, and six months after the end of the intervention to determine whether any gains from the treatments were retained over that time.

The WJ percentile indicates a child's rank in math performance compared to a nationally norm-referenced group of children of the same age. A percentile of 50 means that the child performed at the median in the national pool of children at the same age. Fig. 1 shows the distribution of baseline WJ percentile for the 758 children with complete assessment data in the MPACT sample. At baseline the average child in MPACT scored at the 25th percentile. This is consistent with other studies that show that children from low-income families score lower than children from high-income families, even before compulsory schooling begins (Fryer & Levitt, 2006, 2013; Magnuson & Duncan, 2016; Reardon & Bischoff, 2011; Waldfogel & Washbrook, 2011).

We also consider a secondary outcome, parent engagement in math activities with their child, that may be a mechanism for any change in math skill. As with most other studies, we do not have an objective measure of how much time parents spend with their child. We asked parents how many days over the previous week they helped their children count, recognize numbers, recognize shapes, and add or subtract.[14] How many days parents engaged in math activities with their child depended on the activity. Just 2% of parents said they never helped their child count, but 37% said they never do addition and subtraction activities with their child. Parents help their child learn shapes and recognize numbers less than they help their child count but more than they do addition and subtraction. Because exposure to math concepts is a function of both the amount of time and the type of activity, with advanced math activities producing greater math skill (Skwarchuk, 2009, Skwarchuk et al., 2014, Thompson et al., 2017), we create a score that weighs each kind of engagement by the inverse of the reported average frequency of the engagement at baseline meaning that time spent in counting activities gets less weight than time in addition and subtraction. This assumes that parents who engage their child in more difficult tasks have a greater intensity of engagement. The measure of intensity of math engagement measure ranges from 0 to 12 with 0 indicating no math engagement and 12 indicating the highest level of

[13] The number of eligible parents was provided by preschool centers, and it is the number of parents who met the eligibility criteria for opt-out centers or the number of children in the classrooms for opt-in centers.

[14] Responses were never (with a value of 0), 1 or 2 days (assigned a value of 1.5), 3 or 4 days (assigned a value of 3.5), and 5 or more days (assigned a value of 6)

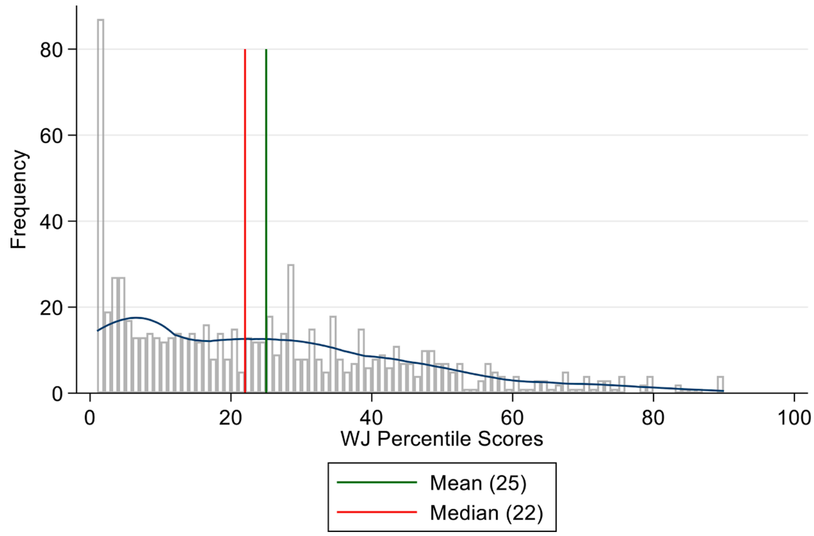

Fig. 1. Distribution of baseline WJ percentile for children participating in MPACT
Notes: Sample is 758 children who participated in all 3 assessments.

intensity of math engagement. The baseline intensity of math engagement has a mean of 6.35 and a standard deviation of 2.79.

Table 1 shows descriptive statistics of characteristics of children and parents in our primary analysis samples by treatment group status. A joint f-test test comparing children or parents in any treatment group with those in the control group for each characteristic. These characteristics are balanced across treatment groups and the control group. Pairwise t-tests show that the WJ percentile for the MKit group is significantly higher than other treatment groups and the control group. We controlled baseline WJ percentile when estimating the treatment effects on math skills. None of these sample characteristics differed significantly by round.

Finally, Table A5 compares the MPACT analytical sample of 758 children who completed the baseline, post-intervention and 6-months post intervention assessments with children in a national Head Start sample and an Illinois state Head Start sample in 2022[15]. Compared to the national and Illinois Head Start samples, MPACT children are slightly younger, and MPACT parents are much less likely to be non-Hispanic White, more likely to be Hispanic and to speak Spanish at home, and are more likely to have less than high school education.

4. Main Results

4.1. Treatment Effects on Math Skills

To estimate the effect of the MPACT treatments on math skill we estimate the following model:

$$Y = \beta_0 + \beta_1 T_1 + \beta_2 T_2 + \beta_3 T_3 + \beta_4 T_4 + \gamma + \varepsilon \qquad (1)$$

where Y is the WJ percentile score measured at the end of the intervention or six months after the intervention ended, T_1 to T_4 are indicators of the assignment to the four MPACT treatment groups, γ are control variables including the baseline measure of the outcome and classroom fixed effects, and ε is an error term. Because the control group is the omitted group, the coefficient for each treatment is the average change in children's WJ percentile score for that treatment compared to the control group.

Table 2 shows the estimated effect of the MPACT treatments on children's WJ age-adjusted percentile. The first column in Table 2 shows that at the end of the 12-week intervention no treatment had a positive effect on math scores at any significance level. Children in the growth mindset group scored slightly lower than the control group (p=.10).

Column 2 shows that at six months after the intervention, the math app treatment increased the WJ percentile scores by 3.96 percentile points compared to the control group, which is equal to an effect size of 0.20 standard deviations (p=.10). The present bias treatment had a treatment effect of 3.47 percentile points, which is equal to an effect size of 0.18 standard deviations (p=.10).

We use randomized inference as an alternative way to estimate significance levels and report p-values in brackets in Table 2 (Athey & Imbens, 2016). This method of estimating p-values may be superior to parametric estimates when the sample has been randomized into treatment groups. To do this we created a set of counterfactual treatment effects by statistically altering treatment assignment across the sample. We then rank these treatment effects to see where our actual estimate lies within the distribution of possible effects. The p-value is the percentile of our estimate compared to counterfactual estimates. Our results did not change using this alternative way to calculate the p-values.

Based on our pre-analysis plan, we initially aimed to analyze heterogeneous treatment effects on children's math skills by child's gender, baseline math skills, and baseline level of parental engagement. Unfortunately, our response rate left us underpowered to conduct these analyses reliably. Point estimates of treatment effects suggest that the math app and the present bias interventions may be more beneficial for children with higher math skills and more parental engagement at baseline. Girls benefited more from the tablet intervention than boys, while boys and girls had similar gains from the present bias interventions. Further research with a larger sample and richer survey data is needed to confirm this pattern and explore the underlying mechanism.

[15] Ideally, we would also compare our sample with a Chicago Head Start sample or a sample of Chicago low-income families with preschool-aged children to understand the representativeness of the sample in the city, however, neither Head Start nor CPS have such publicly available data for comparison. Since around 50% of Head Start programs in Illinois locate in Chicago, we can still get a sense of the city-wise representativeness of our sample by comparing our sample with the Illinois Head Start sample, even though it is not ideal. Another piece of suggestive evidence of the city-wise representativeness is our diversification of preschool locations in Chicago. See Figure A6.

Table 1
Descriptive Statistics at baseline and Balance Tests

Child variables (N=758)	Control (N=220)	Math app (N=72)	MKit (N=153)	Growth mindset (N=162)	Present bias (N=151)	F Statistic p-value
Female	55.9%	48.6%	56.9%	56.8%	51.7%	.68
Age (month)	46.93	45.86	46.38	46.37	46.86	.64
	(5.98)	(6.06)	(6.15)	(6.04)	(5.63)	
Intensity of math engagement	6.01	6.81	6.49	6.17	6.32	.25
	(2.72)	(2.36)	(2.87)	(2.81)	(2.7)	
WJ percentile	23.34	23.73	28.58	23.97	24.45	.17
	(20.96)	(19.2)	(21.27)	(19.37)	(20.91)	
Parent variables (N=721)	Control (N=202)	Math app (N=87)	MKit (N=136)	Growth mindset (N=143)	Present bias (N=153)	F Statistic p-value
Female	92.5%	95.3%	93.4%	95.1%	93.5%	.83
Age	31.31	31.95	32.16	31.36	31.52	.82
	(7.14)	(7.44)	(7.1)	(6.78)	(6.6)	
Non-Hispanic Black	31.2%	31%	29.4%	27.3%	34%	.79
Hispanic	62.9%	63.2%	66.9%	68.5%	61.4%	.68
Percent with less than HS degree	26.7%	20.7%	28.7%	23.8%	30.1%	.47
Percent with HS degree	24.3%	24.1%	27.9%	30%	25.5%	.76
Percent with BA or higher degree	9.4%	6.9%	5.9%	7%	9.2%	.72
Percent who speak Spanish at home	56.9%	54%	56.6%	61.5%	53.6%	.69
Household income in previous year	$20436	$20862	$18205	$18768	$18396	.51
	(15295)	(15709)	(14200)	(15372)	(15200)	

Note: The data are from baseline math assessment and parent survey. The child characteristic data are limited to the sample of children who have baseline and two follow-up assessments. The parent characteristic data are limited to the sample of parents who responded to both baseline and follow-up parent surveys. The number of respondents to each question varies because not all parents answered all questions. The F-Statistic p-value column represents the p-value on a joint hypothesis test with a null hypothesis of equal means across treatment conditions. *** p<.01, ** p<.05, * p<.10.

Table 2
Effect of MPACT treatments on children's WJ percentile

	(1) End of intervention	(2) 6 months post intervention
Math app	-.1	3.96*
	(2.8)	(2.37)
	[.97]	[.09]
MKit	-2.89	1.12
	(2.25)	(2.08)
	[.16]	[.56]
Growth mindset	-3.78*	0.06
	(2.11)	(1.99)
	[.06]	[.97]
Present bias	1.79	3.47*
	(2.24)	(1.96)
	[.38]	[.07]
Constant	10.52***	9.12***
	(1.62)	(1.52)
Control Mean	27.89	22.02
Control SD	23.96	19.38
N	758	758

Notes: The control group is omitted. Regressions include baseline measures of the outcome and classroom fixed effects. Parametric standard errors are reported in parentheses. The significance level is calculated by parametric standard errors. *p<0.1, **p<0.05, ***p<0.01. P-values calculated by randomization inference are included in the bracket.

Table 3
Effect of MPACT treatments on intensity of parental math engagement

	(1) Intensity of parental math engagement
Math app	.96***
	(.34)
	[.01]
MKit	.35
	(.33)
	[.27]
Growth mindset	.45
	(.32)
	[.14]
Present bias	.73**
	(.31)
	[.01]
Constant	3.36***
	(.32)
Control Mean	6.16
Control SD	2.76
N	665

Notes: The control group is omitted. Regressions include baseline measures of the outcome and classroom fixed effects. Parametric standard errors are reported in parentheses. The significance level is calculated by parametric standard errors. *p<0.1, **p<0.05, ***p<0.01. P-values calculated by randomization inference are included in the bracket.

4.2. Treatment Effects on the Intensity of Math Engagement

Parental engagement in math-related learning activities is essential in developing children's math skills, so one potential channel through which our interventions changed children's math skills is the change in parental engagement. We use the same model as equation 1 to estimate the impact of the treatments on the intensity of parent's math engagement with their child. Recall that this is measured at the end of the intervention.

Table 3 reports the point estimates of treatment effects on the intensity of parent's math engagement. It shows that the math app and present bias treatments increased the intensity of parents' math engagement. We also calculate the effect sizes of each treatment by diving the coefficients by the standard deviation of the control group. Parents in the math app group increased the intensity of their math engagement by a third of a standard deviation more than the increase for the control group (p=.01). Parents in the present bias treatment group increased the intensity of their math engagement by .26 standard deviation more than the increase for the control group (p=.05). Parents in the MKit and growth mindset treatments increased the intensity of their engagement in math activities with their children by a modest but insignificant amount, .13 and .16 standard deviations of the control group. These results are consistent with the argument that increasing parent engagement in math activities increased math skill.

To check the robustness of our results, we also computed empirical p-values from randomization inference and reported them in the brackets, which did not change our results. Further, Table A7 reports the same regression as in Table 3 but changes the weighted baseline control to four baseline measures of parent-child math learning activities, which include counting, recognizing numbers, adding or subtracting, and recognizing shapes. We also estimated the treatment effects individually on how many days over the previous week parents helped their children count, recognize numbers, recognize shapes, and add or subtract. Results in Table A8 show that the math app and present bias interventions significantly increased the frequency of three math activities, while the MKit and growth mindset interventions only significantly increased the frequency of one math activity each. It confirms our conclusion that the math app and present bias treatments are more powerful in increasing parents' math engagement than the other two treatments.

Because this study relies on parents' self-reports of how much time they spend in math activities with their children, a potential concern is that parents differentially exaggerate their engagement due to differential priming from the treatments. Research has demonstrated that socially desirable behaviors are overstated in surveys (e.g., Paulhus, 2002; Touraneau & Yan, 2007; Krumpal, 2013). Research comparing objective measures of parents' time to parents' reports of their time use show that parents on average overestimate the time they spend in educational activities with their children (Mayer et al., 2019) but also that parents who report spending more time generally do spend more time in educational activities with their children than parents who report spending less time. For MPACT the question is whether the treatments result in different levels of self-reports of engagement. For differential priming to be an issue for our results it would have to be the case that the math app and present bias treatments provide greater priming than the MKit and growth mindset treatments. However, neither the math app treatment or the MKit treatment included any messaging about math engagement. Yet engagement increased in the math app but not the MKit group. In addition, the two treatments that increased reported engagement were also the treatments that increased math test scores, suggesting that engagement really did increase in these groups.

The three treatments in which parents received the MKit materials were intended to increase parent engagement by providing materials and information about how to engage children in math activities. We surveyed parents about their MKit usage in the first round of the experiment. Among 125 eligible parents who were in the first round of the survey sample and were in one of the three groups receiving the MKit, 106 parents reported their MKit usage at the end of the intervention. These 106 parents were evenly distributed across three treatment groups that received the MKit. In the MKit only group 17% of parents reported that they misplaced or lost the MKit at times versus only 6% of parents in the growth mindset group and 11% of parents in the present bias group. Only 37% of parents in the MKit only group finished half of the MKit activities compared to 47% of parents in the growth mindset group and 54% of parents in the present bias group. Though we did not survey parents on their MKit usage in the following rounds, the results from the first round provide suggestive evidence that only providing the MKit is not sufficient to substantially increase parent-child math learning.

The growth mindset treatment was intended to increase children's WJ scores by increasing the extent to which parents believed that engaging their child in math activities would improve their child's math skill. However, at baseline our survey data show the median growth mindset value was 4.6 on a 6-point scale with 6 being a very strong growth mindset, and fewer than 11% of parents reported a growth mindset value of less than 3. This finding was unexpected given results of other research with similar types of parents (ie Rowe & Leech, 2019) and suggests that almost all parents believed that children could learn new things if they were taught those things. This left little room for a treatment intended to increase growth mindset to change parent engagement.

5. Discussion and Conclusion

5.1. "Fade-in" Treatment hypothesis

A puzzle in our findings is that for both the math apps and present bias interventions, we find no treatment effect at the end of the intervention, but significant positive treatment effects six months later. This pattern seems contrary to many interventions in which treatment effects are the highest at the end of the intervention, and then "fade out" over time.

One potential explanation for this anomaly is that some behavior changes produce treatment effects only after a long intervention. For instance, someone trying to lose weight by jogging might not lose weight after a few days, or even a few weeks of running. However, after several months of running, they will lose weight. This treatment effect has a "fade-in" pattern over time because the effect of the behavior change must accumulate over time. This pattern is consistent with the results for MPACT.

In MPACT this would happen if math engagement in the math app group and the present bias group accumulated during the 12-week intervention but was unable to increase children's math skills during that period compared to the control group but if participants in these groups math engagement over the next six months that additional engagement may sufficed to increase children's math skills. This is possible because parents kept the MKit once the intervention ended, so parents could have continued to use it. This scenario is most likely to happen in the present bias group, where parents already showed an increase in math engagement at the end of the intervention. Similarly, the math apps that were on the tablets were available at little or no cost so parents could have downloaded these for their children to use after the intervention ended. Use of the math apps beyond the 12-week intervention could also have resulted in greater math skill. We conduct Chow tests[16] to verify if the "fade-in" hypothesis creates significant increase of the treatment effects for our interventions between the end of the intervention and six months after the intervention, however, we are underpowered to detect any significant changes in the treatment effects within treatment arm, even though we observe a large increase in treatment effects for all groups from the end of the intervention to six months post.

5.2. Conclusion

We estimated the effect of four separate treatments that were part of the MPACT intervention. Three of these treatments were intended to increase parents' engagement in math activities with their children as a way to increase children's math skill. The fourth treatment, the math app treatment, was intended to increase children's exposure to math concepts through digital apps that did not necessarily require (but did not preclude) parental engagement.

We found that neither the MKit treatment nor the growth mindset treatment increased the intensity of parental math engagement with their child or their child's math skills. On the other hand, the math app

[16] Chow test examines whether the coefficients estimated over one group of data are equal to those over another group of data. We conducted Chow test to compare the treatment effects on math skills between the end of the intervention and six months after the intervention. We pooled assessment data at the end of the intervention and six months after the intervention and generated a dummy variable to distinguish these two assessments. We then fit the pooled data into regression model (1) and interact the dummy variable with every regressor. The interaction term between the dummy variable and the indicator of a given treatment group shows the difference in treatment effects of this group between the two follow-up assessments. Despite large point estimates of these interaction terms, we are underpowered to detect a significant difference for any treatment group.

treatment and the present bias treatment both increased the intensity of parent's math engagement with their child and their child's math skill by a meaningful although marginally statistically significant amount. The estimated effect for these treatments is large compared to those found in other studies. For example, in the Head Start Impact Study the average treatment effect of being randomly assigned to Head Start was 17.9% of a standard deviation (p=.01) on parents' reports of engaging in math activities with their child after one year of being enrolled in Head Start (Padilla 2020; see also Bloom & Weiland 2015) while the effect of the present bias treatment in MPACT was .26 standard deviations and the effect of the app treatment was .35 standard deviations.

These findings confirm results from other papers studying parental engagement. Only providing materials for engagement is unlikely to change parent behavior because many parents who received materials did not use them. Our recent research with a similar sample shows that low-income parents experiencing psychological stress or financial scarcity do not attend to information that they receive (Kalil, Mayer, & Shah, 2023), so it may not be surprising that in this low income sample many parents did not attend to the materials that they received without additional behavioral messaging. When comparing the results from this paper to other studies, we find that our effects are sizable but reasonable. For instance, Doss et al. (2022) used two text message-based interventions to boost the math skills of preschool-age children. One intervention's messages focused solely on math development, and the other targeted math, literacy, and social-emotional skills. They show that no intervention increased children's math skills on average, but the intervention focusing on a combination of math, literacy, and social-emotional skills increased girls' math skills by .16 standard deviations (p=.1). The present bias intervention in our study combines learning materials MKit and behaviorally informed text messages to tackle present bias. We found that the effect size of the present bias intervention increased math skills by .18 standard deviations on average and boys and girls have similar gains.

These results also indicate that in this low-income sample, few parents have a fixed mindset (although not all studies have produced this result; see Rowe & Leech, 2019). Therefore, interventions that focus on fostering parents' growth mindset may be unlikely to change either parent engagement or children's test scores.

The MPACT results also indicate that present bias is a potential barrier for parents to use math related materials when they are available. When parents received the MKit plus behaviorally informed messages to manage present bias they reported greater engagement in math activities with their child compared to the growth mindset and MKit treatments. This is consistent with our previous research (Mayer et al., 2019; Kalil et al., 2023) showing that an intervention to reduce the procrastination associated with present bias increased the amount of time that parents read to their children using an electronic app and that the effects are the greatest for parents who at baseline were the most present biased.

Finally, the math app treatment produced surprisingly large effects on parent engagement at the end of the intervention (p=.01) and on test scores six months after the intervention (p=.10). The math apps selected in our intervention do not require parental engagement because they have features to direct children to learn by themselves. However, we observed a large treatment effect on math engagement in this group suggesting that the math apps may reduce the unit cost for parents to engage in math learning activities with their children. Using math apps may change parents' roles from a teacher to a moderator during the child's math learning process, which may relieve the stress and burden of parent engagement, thus requiring less effort from parents and increasing enjoyment for both parents and children.

All our interventions are built on low-cost elements and can be scaled up to serve a larger population. Nearly 90% of families in our sample reported having a digital device (either a tablet or a smartphone) to download apps at baseline. Scaling up the math app treatment should not require providing additional digital devices to most families. Five of the seven math apps included in the math app treatment were free and the other two charged $2 and $3 one-time fees, respectively. Additionally, a texting program that sends behaviorally informed messages to parents to reduce their present bias can be added to the routine texting programs already used by many preschools at little additional cost. Compared to other interventions, a program using math apps and behavioral messages to increase children's math skills may be relatively inexpensive. This result thus points to new avenues for efficient policy intervention to improve children's math skills at home.

CRediT authorship contribution statement

Susan E. Mayer: Conceptualization, Methodology, Writing – original draft, Supervision, Funding acquisition. **Ariel Kalil:** Conceptualization, Methodology, Writing – review & editing, Supervision, Funding acquisition. **William Delgado:** Conceptualization, Methodology, Formal analysis, Writing – original draft, Visualization. **Haoxuan Liu:** Formal analysis, Writing – original draft, Writing – review & editing, Visualization. **Derek Rury:** Writing – review & editing. **Rohen Shah:** Writing – review & editing.

Data availability

The data used in this article are available online at ICPSR (https://www.openicpsr.org/openicpsr/project/176341/version/V2/view).

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2023.102436.

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