When behavioral barriers are too high or low – How timing matters for text-based parenting interventions[☆,☆☆]

Kalena E. Cortes [a,*], Hans Fricke [b], Susanna Loeb [c], David S. Song [d], Benjamin N. York [e]

[a] The Bush School of Government and Public Service, 4220 TAMU, 2088 Allen Building, Texas A&M University, College Station, TX, 77843, Research Fellow at the Institute for the Study of Labor (IZA), and Research Associate at the National Bureau of Economic Research, USA
[b] Amazon Inc., 320 Westlake Avenue N., Seattle, WA, 98109, and Research Affiliate at the Institute for the Study of Labor (IZA), USA
[c] Stanford University, Stanford, CA 94305, USA
[d] Center for Education Policy Analysis, Stanford University, 520 Galvez Mall, CERAS Building 5th Floor, Stanford, CA, 94305, USA
[e] ParentPowered Public Benefit Corporation, USA

ARTICLE INFO

ABSTRACT

Text-messaged-based parenting curricula can be effective in supporting positive parenting practices. This study compares weekday to weekend delivery of such a program focused on early childhood development. Weekend texts are, on average, more beneficial to children's literacy and math development for initially lower-achieving children. The weekday texts show benefits for higher-achieving children on higher-order skills. These results are consistent with the hypothesis that lower-educated parents and parents of lower-achieving students face such high barriers during weekdays that light-touch supports cannot consistently overcome them, while during weekends barriers to parenting are well-suited to being addressed by these behavioral approaches.

1. Introduction

Parental time investments impact children's development (Del Boca, Flinn & Wiswall, 2014; Del Bono, Francesconi, Kelly & Sacker, 2016; Greenman, Bodovski & Reed, 2011; Thomsen, 2015). These time investments can vary from year-to-year, month-to-month, week-to-week, and day-to-day. Since most parents work away from home, time spent with their children is limited to non-work hours and to their days off (Bianchi, 2000; Fox, Han, Ruhm & Waldfogel, 2013; Stewart, 2010). Responsibilities of work, household, and finances also create cognitive demands that may affect parenting practices and may vary day-to-day. As a result, parenting effectiveness may differ by day, and parents

may systematically benefit more from parenting supports on some days than on others. On some days, parents have greater needs for suggestions of what to do with their children because they are facing greater barriers; yet, on some days, these barriers may be so great that supports are not enough to affect parent-child interactions. In this study, we test and find that the effects of a text-messaged based parenting program differ depending on the days of the week it is offered.

Parents face a number of barriers that hinder their ability to provide beneficial home learning environments for their children. Working during their children's waking hour is clearly one barrier. Information about how to create supportive environments may be another. On top of these difficulties, behavioral barriers can be strong. For instance, the

demands of parenting can create a heavy *cognitive load*. Parents need to make many choices each day about what questions to ask their children, what to feed them, and how to respond to their questions, requests and behaviors. These day-to-day decisions alone can burden parents with a heavy cognitive load, leading parents to interact sub-optimally with their children.[1] Similarly, some of the gratifying aspects of parenting occur in the future, and *delayed gratification* can cause suboptimal behavior such as underinvestment of time and effort in parenting.[2] Moreover, strong cognitive demands can increase self-control problems when individuals make less mindful choices because of distractions (Shiv & Fedorikhin, 1999). Hence, parents may not engage in skill-building activities that are rewarded in the long-term on days with high barriers.[3] Finally, day-to-day tasks may distract parents from more distant goals and cause parents to pay *limited attention* to beneficial parenting practices.[4]

These information, time, and behavioral constraints may be more detrimental for parents from lower socioeconomic backgrounds. Wealthier parents often have jobs with more flexibility, as well as greater access to child care (Galinsky, Sakai & Wigton, 2011), while parents in poorer families often have more restrictive work hours (Bianchi, 2011; Williams & Boushey, 2010). Moreover, the strains of poverty itself limit cognitive capacity for complex tasks (Mani, Mullainathan, Shafir & Zhao, 2013; Shah, Mullainathan & Shafir, 2012). Parents with low incomes may use more of their cognitive capacity on daily financial challenges, aggravating the behavioral barriers to parenting. In part as a result, college-educated parents spend more time with their children (Guryan, Hurst & Kearney, 2008; Sayer, Gauthier & Furstenberg, 2004) and are able to engage more in educational activities (Hsin & Felfe, 2014). Furthermore, the default activities for parents who themselves were raised in highly educated families may be more helpful to children than the default behaviors of other parents, even if both groups face the same behavioral challenges.

Programs aimed at improving parenting practices may be most effective if targeted during times when parents can engage in parenting activities and interact with their children. Most programs have shown only limited success, perhaps because they are not flexible enough to accommodate timing nuances and they focus on information barriers instead of the behavioral barriers of parenting (Aos, Lieb, Mayfield, Miller & Pennucci, 2004; Duncan, Ludwig & Magnuson, 2010; Karoly et al., 1998). More recently, text-messaging interventions have emerged as a promising alternative or supplement to traditional approaches due to their low cost, the widespread use of mobile phones, and their ease of scalability. They have shown to positively influence both student and parent outcomes in a wide array of educational settings.[5] Text

messaging interventions also can readily target particular times and days when advice may be more useful for parents.

This study asks whether the effectiveness of text-message-based parenting support depends on when the support is provided. Specifically, it compares the effects of an early childhood text-messaging program sent during the weekend to those of the same program sent on weekdays. The text-messaging program, which we will call *Tips·by·Texts* (also known as *Ready4K*), has shown positive effects on parental engagement and children's literacy development (Doss, Fahle, Loeb & York, 2019; York, Loeb & Doss, 2019). This intervention breaks down the complexities of parenting by providing a combination of *general information* about important literacy skills and parent-child activities (i.e., "FACT" text messages), *actionable advice* with specific examples of parent-child literacy activities (i.e., "TIP" text messages), and *encouragement/reinforcement* (i.e., "GROWTH" text messages).[6] Starting from the hypothesis that most parents face greater distractions from positive parenting during weeknights than on weekends, if the program is more effective during the week, then the mechanism is likely through reducing barriers that are higher. If it is more effective on the weekend, then the mechanism is likely through reducing barriers that are not so high as to be insurmountable. We expect effect heterogeneity given that some parents may face higher barriers and have less strong defaults.

We randomly assign parents of pre-kindergarten children into three treatment groups. The first group of parents receives the original program, that is, a "FACT" message on Monday, a "TIP" message on Wednesday, and a "GROWTH" message on Friday, henceforth the **Weekday** program. The second group receives the "FACT," "TIP," and "GROWTH" messages on Friday, Saturday, and Sunday, respectively, henceforth **Weekend** program. The **Weekend** program differs from the **Weekday** program in that it sends the text messages on different and consecutive days. Therefore, a third group of parents receives the "FACT," "TIP," and "GROWTH" messages on Tuesday, Wednesday, and Thursday, respectively, in order to parse out the *spacing effect* from the *weekend effect*. This program henceforth will be called the **Midweek** program.

We find that sending text messages with parenting support on weekends is more beneficial to children's development, on average, than sending texts on weekdays. Moreover, we find that the benefits of the weekend program were particularly pronounced for parents with less educational attainment and for children who started pre-Kindergarten in the lower half of the baseline skill distribution on easier subcomponents, such as rapid letter naming, rote counting, and shape naming, whereas the weekday program was somewhat more beneficial for higher educated parents and for the initially higher achieving children. These results are consistent with the hypothesis that the lower-educated parents and parents of lower-achieving students, on average, face higher barriers during weekdays, and supports are not enough to overcome these barriers. For the parents of higher-achieving students and those with more education, weekday texts are more effective because weekdays are not so difficult as to impede positive parenting. These findings suggest that parenting support works best when parents have time, attention, and need.

2. Background of the intervention

To place the current study into context, three prior studies have evaluated the effectiveness of the original **Weekday** texting program in terms of the text messaging content and also frequency of texts. The first study, York et al. (2019), conducted a randomized control trial

---

[1] See Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) for more discussion on cognitive load.

[2] See Michel (1974), Baumeister, Vohs, and Tice, (2007), Thaler and Sunstein (2008), and DellaVigna (2009) for more discussion on delayed gratification.

[3] Mayer, Kalil, Oreopoulos, and Gallegos (2019) show that interventions that regiment reading practices according to a long-term schedule of concrete goals and rewards lead to substantial increases in time parents spend reading with their children.

[4] See Gabraix (2017) and Karlan, McConnell, Mullainathan, and Zinman (2016) for more discussion on limited attention.

[5] Such interventions have been demonstrated to positively affect school and class attendance of students (Bergman and Chan, 2017; Groot, Sander, Rogers, and Bloomenthal, 2017; Robinson, Lee, Dearing, and Rogers, 2018; Rogers and Feller, 2018), the number of course credits earned in high school (Kraft and Rogers, 2015), FASFA completion (Page, Castleman, and Meyer, 2018), chronic absenteeism and parental engagement (Smythe-Leistico and Page, 2018), and college enrollment rates (Castleman and Page, 2015, 2016). Moreover, these interventions have been particularly effective for children and parents from low-income backgrounds (Bergman, 2015; Castleman and Page, 2015, 2016; Bergman and Chan, 2017).

[6] The "GROWTH" text message specifically aims to provide immediate, short-term gratification to parents in the form of written encouragement. Such stimuli represent self-affirmation, which has been found to be effective in the behavioral science literature (Cohen and Sherman, 2014; Hall, Zhao, and Shafir, 2014; Sweeney and Moyer, 2014).

Table 1

Tips·by·Text Preschool Text Messaging Examples - FACT/TIP/GROWTH.

Vocabulary Development:	Letter Knowledge:
FACT: Knowing a lot of words is important for reading, writing & talking to others. Kids who know many words have an advantage in school!	FACT: On the way home is a great time to build your child's knowledge of letter sounds. Children who know many letter sounds are better prepared 4K!
TIP: Say & explain words to your child as you do everyday tasks. Tell your child that you WASH dishes to CLEAN them. To WASH is to CLEAN.	TIP: On the way home ask: What animals start with the "bbb" sound (bee, bear, bobcat, baboon). What letter makes the "bbb" sound? (B) Try other letters.
GROWTH: Keep explaining words. You're preparing your child 4K! Now ask questions about the words: What else do we WASH? (Clothes, hair)	GROWTH: Keep working on letter sounds! Say: Dog starts with the "ddd" sound. What letter makes that sound? (D) What else starts with D? (Duck)
Beginning Word Sounds:	Name Writing:
FACT: To learn to read & write, children need to know that words are made up of sounds. Many children start by learning beginning word sounds.	FACT: Preschool is when many children learn to write their name. Name writing can help kids develop reading, writing & fine motor skills.
TIP: At a meal, say: Let's think about the sound that each food name starts with. What sound does tomato start with? (The ttt sound) Milk? (mmm)	TIP: On a piece of paper, show your child how to write his/her name. Ask your child to try. Help your child & praise her/him for trying!
GROWTH: Keep saying beginning word sounds. You're preparing your child 4K! Now ask your child: What letter makes the ttt sound? The mmm sound?	GROWTH: Keep helping with name writing to prepare your child 4K! Have your child draw a picture with his/her name. Hang it on a wall. S/he will love it!
Rhyming:	Bath Time:
FACT: Rhyming helps children learn that words are made up of sounds, which is an important step in learning to read.	FACT: Bath time is great for teaching your child important skills for K. Start by asking: what are the things we need for bath time? Why?
TIP: Say to your child: Let's play a rhyming game. I'll say a word & you say one that rhymes. Try hat (cat, mat) & bear (care, hair).	TIP: When you're bathing your child, point out the letters on bottles. Ask your child to name them & tell you the sounds that they make.
GROWTH: Keep rhyming. You're preparing your child 4K! When you're out, point to an object like a car. Ask: What rhymes with car? (Far, jar, star)	GROWTH: Keep using bath time to prepare your child 4K! Ask: what rhymes with tub (cub, rub), wet (let, net) or soap (rope, hope)?
Upper- and Lower-Case Letters:	Playing Thinking Games:
FACT: Preschool is when children begin learning the difference between UPPER & lower-case letters, an important skill for reading & writing.	FACT: Playing thinking games with your child that encourage him/her to make connections is a great way to build your child's brain.
TIP: Hunt for UPPER & lower-case letters in a book or magazine. Pick a letter (m). Show your child an m & an M. How many of each can s/he find?	TIP: Pick a word beginning with the "sss" sound like snake. Say: I'm thinking of animal that starts with "sss" & slithers. Can you guess?
GROWTH: Keep working on UPPER & lower-case letters to prepare 4K! In a book, show your child that sentences & names start with UPPER-case letters.	GROWTH: Keep playing thinking games! Now ask your child to think of word that starts with "bbb" & rhymes with snake (bake).

Source: Author's own text messaging program, Tips·by·Text (For more information see: https://annenberg.brown.edu/projects/tipsbytext/overview). These texts are a few examples of the 32-week long preschool parenting text messaging program conducted in partnership with the Dallas Independent School District during the 2016–17 academic school year.

evaluation in which they compared the ***Weekday*** program to a control group, and found substantial improvements of self-reported parent engagement and child literacy assessment scores. The treatment effects were much more pronounced for children who began the school year in the bottom half of the literacy skill distribution. The second study, Doss et al. (2019), tested whether these improvements were driven by the *content* of the text messages or simply by the texts serving as reminders, or nudges. To do so, the authors also conducted a randomized control trial evaluation that compared the original ***Weekday*** program to a ***Weekday*** program in which they tailored the text content to the skill level of the children. The program that targeted skill levels had a greater effect that was driven by children at the bottom and top of the literacy

Table 2

Summary Statistics of the Randomization Sample and the Analytic Sample.

	Randomization		Analytic	
Panel A: Children Characteristics	—		—	
Age (in years)	4.56	(0.29)	4.57	(0.29)
Female	0.50		0.51	
Race and ethnicity				
Black	0.18		0.16	
Hispanic	0.78		0.80	
Asian	0.01		0.01	
White	0.02		0.02	
Other[1]	0.01		0.01	
Low socioeconomic status	0.95		0.96	
Circle-1 Literacy assessment (raw) scores				
Rapid Letter Naming	6.03	(8.49)	6.08	(8.74)
Missing Rapid Letter Naming	0.06		0.00	
Rapid Vocabulary	10.68	(6.97)	10.63	(7.22)
Missing Rapid Vocabulary	0.06		0.00	
Phonetic Awareness	10.39	(5.54)	10.47	(5.65)
Missing Phonetic Awareness	0.07		0.00	
Circle-1 Math assessment (raw) scores				
Rote Counting	0.61	(0.64)	0.61	(0.66)
Missing Rote Counting	0.07		0.00	
Shape Naming	3.12	(1.56)	3.15	(1.61)
Missing Shape Naming	0.06		0.00	
Number Discrimination	1.43	(0.73)	1.44	(0.75)
Missing Number Discrimination	0.06		0.00	
Number Naming	1.43	(0.73)	1.44	(0.75)
Missing Number Naming	0.06		0.00	
Shape Discrimination	4.48	(1.58)	4.51	(1.63)
Missing Shape Discrimination	0.07		0.00	
Counting	2.70	(1.55)	2.73	(1.61)
Missing Counting	0.07		0.00	
Operations	0.91	(1.03)	0.91	(1.08)
Missing Operations	0.08		0.00	
Panel B: Parental Characteristics				
Highest education level[b]				
Less than high school	0.32		0.33	
High school	0.32		0.32	
Some college	0.19		0.19	
Associate degree	0.04		0.04	
Bachelor's degree	0.03		0.03	
Graduate degree	0.02		0.02	
Missing education	0.07		0.08	
Texting language: English	0.57		0.55	
Panel C: Teacher Characteristics				
Average female	0.75		0.75	
Average race and ethnicity				
Hispanic	0.43		0.45	
Black	0.22		0.22	
White	0.32		0.32	
Average missing teacher demographics	0.00		0.00	
Average experience (in years)	7.79	(5.92)	7.85	(6.01)
Average days absent	9.39	(9.16)	9.13	(9.03)
Average days missing	0.08	(0.19)	0.07	(0.17)
Observations	4419		3664	

Notes: Numbers in parentheses are standard deviations for continuous variables. Circle-3 test sample is conditioned on having Circle-1 test scores. [1]Includes American Indian, Native American, Pacific Islander, two or more races, or other race.

distribution, providing evidence that the content of the text messages, not just the reminders, caused the effects in the first study. Having established in the first two studies the effectiveness of the texting content, a third study by Cortes, Fricke, Loeb, Song and York (2021) assessed the frequency of the text messages. The authors randomized parents into three treatment groups in which they compared the original three-times-per-week ***Weekday*** program to a program with only one "TIP" on Wednesday, and to a program that added two additional "TIP" messages on Tuesday and Thursday for a total of five texts per week. The findings demonstrated that text messaging programs can supply too little or too much information. Parents prefer three times per week to the

Table 3

Treatment Effects on Attrition in Circle-3 Test Sample - Whether any Circle-3 Test Outcomes (Literacy and Math) are Missing.

Midweek Program	−0.009
	(0.010)
Weekend Program	−0.003
	(0.010)
P-value: Midweek vs. Weekend	0.519
Adj. R^2	0.56
N	4419

Notes: Only the regression coefficients of the treatment's status are reported. The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include controls for student characteristics (age, gender, race/ethnicity, low-SES status, and Circle-1 test scores), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent, and days missing) and randomization block fixed effects. Standard errors are clustered at the randomization block level. Statistical significance levels: *$p<0.10$; **$p<0.05$; ***$p<0.01$.

other options, and children in the bottom quarter of the pre-intervention literacy distribution benefited from receiving three texts per week compared to one but did not show further improvements when receiving five texts per week.

The aforementioned studies provide evidence that *content* as well as *frequency* matter for the effectiveness of the texting programs. However, we do not know whether the *timing* of the text messages matter. That is, when do parents interact with their children, have the bandwidth to implement advice on beneficial parenting practices, and have the potential to benefit from that advice? Understanding these parenting dynamics can further guide and improve program design of the *Tips·by·Texts* programs.

In general, the text-messaging programs are designed to support parenting practices of parents' four-year-old pre-Kindergarten children over the course of eight months. ParentPowered (parentpowered.com) delivered the text messages that were based on the original **Weekday** program that were first used in the San Francisco Unified School District in the 2013–14 school year (York et al., 2019). The **Weekend**, the **Midweek**, and the **Weekend** programs are identical in text content except for the days of delivery.

We use the "FACT," "TIP," and "GROWTH" text message approach to help parents overcome barriers that might limit parental engagement with their children. First, the "FACT" text messages intend to solve *imperfect information* by providing general information about child literacy development and best parenting practices. Second, activities in the "TIP" and "GROWTH" messages aim at alleviating the *cognitive load* of parenting by presenting concrete examples of parent-child literacy activities that are easy to implement and fun. Third, the "GROWTH" text messages aim to overcome the problem of *delayed gratification* and inconsistent time preferences. With praise and encouragement, such as "*You're doing a good job preparing your child for kindergarten*," the messages provide immediate gratification for skill-building and school readiness activities that otherwise would be only be rewarded in the long-term. Finally, the weekly frequency of the text messages reminds parents to engage in beneficial activities with their child and solves the potential problem of *limited attention.*

Table 1 provides several examples of the texting curriculum.[7] The

Table 4

Language and Literacy Results - Treatment Effects on Overall Score and by Specific Sub-scores of the Circle-3 Skills Assessment Test (standardized) .

	Panel A: Language and Literacy, overall score
Midweek Program	−0.006
	(0.025)
Weekend Program	0.026
	(0.022)
P-value: Midweek vs. Weekend	0.220
Control μ [σ]: Weekday Program	70.57 [23.10]
Adj. R^2	0.52
N	3664

	Panel B: Rapid Letter Naming
Midweek Program	0.023
	(0.032)
Weekend Program	0.112***
	(0.029)
P-value: Midweek vs. Weekend	0.009
Control μ [σ]: Weekday Program	30.19 [14.24]
Adj. R^2	0.44
N	3664

	Panel C: Rapid Vocabulary Naming
Midweek Program	0.003
	(0.034)
Weekend Program	0.023
	(0.032)
P-value: Midweek vs. Weekend	0.572
Control μ [σ]: Weekday Program	20.67 [8.77]
Adj. R^2	0.44
N	3664

	Panel D: Phonological Awareness
Midweek Program	−0.044
	(0.032)
Weekend Program	−0.057*
	(0.030)
P-value: Midweek vs. Weekend	0.700
Control μ [σ]: Weekday Program	19.72 [4.75]
Adj. R^2	0.41
N	3664

Notes: Only the regression coefficients of the treatment's status are reported. The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include controls for student characteristics (age, gender, race/ethnicity, low-SES status, and Circle-1 test scores), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing) and randomization block fixed effects. Standard errors are clustered at the randomization block level. Statistical significance levels: *$p<0.10$; **$p<0.05$; ***$p<0.01$.

text messages cover a broad range of skills, including letter knowledge, beginning word sounds, vocabulary development, rhyming, reading comprehension, parent-child language interactions, and parent-child reading activities to name a few. We structured the texting program as a spiral curriculum, such that the program begins with simple information and advice and becomes progressively more advanced over the eight months of intervention, while also reintroducing topics for reinforcement. As shown in Table 1, the content of the text messages provides enjoyable and easy-to-implement activities for parent-child interactions that would help build literacy skills and more general positive parenting practices. Moreover, most of the activities build on existing family routines and activities, such as mealtime, bath time, or commuting/travel, to minimize the costs of adopting beneficial

---

[7] More examples can be found in York et al. (2019), Doss et al. (2019), and Cortes et al. (2021).

Table 5

Language and Literacy Results - Treatment Effects by Parent's Educational Attainment and Child's Pre-Intervention Circle-1 Test Scores.

| | Panel A: Parent's Educational Attainment (High School Degree or Lower (≤ HS) vs. Greater than a High School Degree (> HS)) | | | | | | | |
| | Language and Literacy, | | Rapid Letter Naming, | | Rapid Vocabulary Naming, | | Phonological Awareness, | |
	≤ HS	> HS	≤ HS	> HS	≤ HS	> HS	≤ HS	> HS
Midweek Program	−0.025	−0.023	−0.000	0.006	−0.015	−0.040	−0.060	−0.034
	(0.034)	(0.053)	(0.043)	(0.071)	(0.046)	(0.073)	(0.042)	(0.068)
Weekend Program	0.020	−0.039	0.105***	0.025	0.008	−0.031	−0.054	−0.111*
	(0.027)	(0.054)	(0.036)	(0.072)	(0.038)	(0.084)	(0.042)	(0.063)
P-value: Midweek vs. Weekend	0.170	0.765	0.013	0.796	0.627	0.904	0.883	0.294
Control μ [σ]: Weekday Program	69.52	74.79	30.01	31.40	20.24	22.22	19.26	21.17
	[22.90]	[23.38]	[14.19]	[14.13]	[8.68]	[9.22]	[4.60]	[4.70]
Adj. R²	0.51	0.55	0.43	0.47	0.42	0.45	0.41	0.43
N	2382	1006	2382	1006	2382	1006	2382	1006
—	—	—	—	—	—	—	—	—

| | Panel B: Child's Pre-Intervention Circle-1 Test Scores (Lower-half vs. Upper-half) | | | | | | | |
| | Language & Literacy Skills, | | Rapid Letter Naming, | | Rapid Vocabulary Naming, | | Phonological Awareness, | |
	Lower	Upper	Lower	Upper	Lower	Upper	Lower	Upper
Midweek Program	−0.037	0.041	−0.009	0.068	−0.046	0.049	−0.055	0.006
	(0.039)	(0.033)	(0.050)	(0.044)	(0.055)	(0.047)	(0.049)	(0.044)
Weekend Program	0.061*	−0.006	0.167***	0.073*	0.072	−0.023	−0.055	−0.069*
	(0.034)	(0.031)	(0.047)	(0.043)	(0.047)	(0.048)	(0.049)	(0.041)
P-value: Midweek vs. Weekend	0.024	0.116	0.002	0.893	0.040	0.114	0.993	0.086
Control μ [σ]: Weekday Program	61.98	79.31	25.80	34.64	18.12	23.27	18.06	21.40
	[22.87]	[19.83]	[14.52]	[12.46]	[8.36]	[8.41]	[4.80]	[4.05]
Adj. R²	0.44	0.51	0.37	0.45	0.38	0.45	0.34	0.37
N	1828	1836	1828	1836	1828	1836	1828	1836

Notes: Only the regression coefficients of the treatment's status are reported. The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. In Panel A, regressions samples were split for students with parents with more than a high school education (> HS) and a high school education and less (≤ HS). All regressions shown in panel A include controls for student characteristics (age, gender, race/ethnicity, low-SES status, and Circle-1 test scores), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing), and randomization block fixed effects. In Panel B, regressions samples were split at the median (lower-half vs. upper-half) based on student's Circle-1 language and literacy test prior to the intervention. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions shown in panel B include controls for student characteristics (age, gender, race/ethnicity, and, low-SES status), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing), and randomization block fixed effects. Standard errors are clustered at the randomization block level. Statistical significance levels: *p<0.10; **p<0.05; ***p<0.01.

Table 6

Math Circle-3 Results - Treatment Effects on Overall Score and for Specific Sub-scores of the Circle-3 Skills Assessment Test (standardized) .

	Panel A: Math, overall score	Panel B: Rote Counting	Panel C: Shape Naming	Panel D: Number Discrimination
Midweek Program	0.001	0.030	0.060*	−0.026
	(0.016)	(0.032)	(0.033)	(0.038)
Weekend Program	0.032**	0.069**	0.102***	−0.006
	(0.015)	(0.033)	(0.034)	(0.041)
P-value: Midweek vs. Weekend	0.071	0.212	0.198	0.600
Control μ [σ]: Weekday Program	24.10 [4.24]	1.70 [0.53]	4.45 [0.97]	1.91 [0.35]
Adj. R²	0.47	0.29	0.31	0.09
N	3664	3664	3664	3664
—	—	—	—	—

	Panel E: Number Naming	Panel F: Shape Discrimination	Panel G: Counting	Panel H: Operations
Midweek Program	0.006	−0.002	−0.005	−0.062*
	(0.030)	(0.040)	(0.032)	(0.033)
Weekend Program	0.047	0.037	0.042	−0.037
	(0.029)	(0.037)	(0.033)	(0.035)
P-value: Midweek vs. Weekend	0.177	0.323	0.174	0.478
Control μ [σ]: Weekday Program	3.65 [1.37]	5.61 [0.91]	4.42 [0.94]	2.36 [0.90]
Adj. R²	0.39	0.17	0.29	0.30
N	3664	3664	3664	3664
—	—	—	—	—

Notes: Only the regression coefficients of the treatment's status are reported. The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include controls for student characteristics (age, gender, race/ethnicity, low-SES status, and Circle-1 test scores), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing) and randomization block fixed effects. Standard errors are clustered at the randomization block level. Statistical significance levels: *p<0.10; **p<0.05; ***p<0.01.

Table A1

Randomization Checks - The Effect of Treatment Status on Pre-Treatment Covariates by the Randomization and Circle-3 Test Samples.

Panel A: Regressions of Child Characteristics on Treatment Status (Weekday program is the omitted category in all regressions)

	Randomization Sample ($N = 4419$)			Circle-3 Test Sample ($N = 3664$)		
	Midweek	Weekend	Midweek vs. Weekend	Midweek	Weekend	Midweek vs. Weekend
	Program	Program	(p-value)	Program	Program	(p-value)
Age (in years)	0.013	0.008	0.642	0.008	0.001	0.568
	(0.012)	(0.011)		(0.013)	(0.012)	
Female	−0.003	−0.012	0.661	−0.000	−0.001	0.971
	(0.019)	(0.020)		(0.021)	(0.022)	
Black	0.017	0.009	0.400	0.021*	0.014	0.505
	(0.010)	(0.011)		(0.011)	(0.011)	
Hispanic	−0.015	−0.002	0.223	−0.019	−0.009	0.373
	(0.012)	(0.012)		(0.013)	(0.013)	
Asian	−0.001	−0.005	0.286	−0.004	−0.005	0.572
	(0.005)	(0.004)		(0.004)	(0.004)	
White	−0.006	−0.005	0.858	−0.006	−0.006	0.918
	(0.005)	(0.005)		(0.006)	(0.006)	
Other**	0.001	0.000	0.510	—	—	—
	(0.001)	(0.001)				
Low socioeconomic status	0.002	0.013	0.243	0.003	0.002	0.976
	(0.008)	(0.009)		(0.007)	(0.009)	
Rapid Letter Naming[a]	0.348	0.021	0.288	0.404	0.108	0.405
	(0.297)	(0.313)		(0.336)	(0.385)	
Rapid Letter Naming missing[b]	−0.003	−0.008	0.542	—	—	—
	(0.008)	(0.008)				
Rapid Vocabulary[a]	0.225	0.056	0.524	0.143	−0.008	0.602
	(0.265)	(0.249)		(0.295)	(0.292)	
Rapid Vocabulary missing[b]	−0.004	0.001	0.543	—	—	—
	(0.009)	(0.009)				
Phonological Awareness[a]	0.037	0.084	0.811	−0.050	0.006	0.795
	(0.199)	(0.194)		(0.218)	(0.220)	
Phonological Awareness missing[b]	−0.001	0.003	0.675	—	—	—
	(0.009)	(0.009)				
Rote Counting[a]	0.002	−0.016	0.407	0.001	−0.006	0.805
	(0.022)	(0.022)		(0.026)	(0.026)	
Rote Counting missing[b]	−0.003	−0.006	0.734	—	—	—
	(0.008)	(0.009)				
Shape Naming[a]	0.117**	0.042	0.192	0.122*	0.073	0.463
	(0.059)	(0.058)		(0.068)	(0.072)	
Shape Naming missing[b]	−0.005	−0.001	0.637	—	—	—
	(0.008)	(0.009)				
Number Discrimination[a]	0.059**	0.025	0.227	0.043	0.009	0.305
	(0.028)	(0.031)		(0.033)	(0.034)	
Number Discrimination missing[b]	−0.006	−0.000	0.546	—	—	—
	(0.008)	(0.009)				
Number Naming[a]	0.098*	0.084	0.792	0.117*	0.089	0.643
	(0.052)	(0.054)		(0.060)	(0.060)	
Number Naming missing[b]	−0.001	−0.003	0.775	—	—	—
	(0.008)	(0.009)				
Shape Discrimination[a]	0.109**	0.042	0.279	0.077	0.042	0.629
	(0.054)	(0.063)		(0.063)	(0.076)	
Shape Discrimination missing[b]	−0.005	0.003	0.433	—	—	—
	(0.008)	(0.009)				
Counting[a]	0.033	0.026	0.909	0.033	0.041	0.911
	(0.054)	(0.054)		(0.063)	(0.064)	
Counting missing[b]	−0.006	0.004	0.294	—	—	—
	(0.009)	(0.010)				
Operations[a]	0.020	−0.010	0.394	0.031	0.001	0.471
	(0.037)	(0.039)		(0.042)	(0.046)	
Operations missing[b]	−0.005	−0.000	0.605	—	—	—
	(0.009)	(0.010)				

Notes: Each row represents a separate regression model (only the coefficients of the treatments status are reported). The omitted reference group in all regressions is the original texting program of Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include randomization block fixed effects. Standard errors are clustered at the randomization block level. Circle-3 test sample is conditioned on having Circle-1 and Circle-3 literacy and math test scores. **Includes American Indian, Native American, Pacific Islander, two or more races, or other race. [a]All Circle-1 test score variables are in standard deviation units. [b]No students included with missing information in Circle-3 test sample. Statistical significance levels: *$p<0.10$; **$p<0.05$; ***$p<0.01$.

Panel B: Regressions of Parental Characteristics on Treatment Status (Weekday program is the omitted category in all regressions)

	Randomization Sample ($N = 4419$)			Circle-3 Test Sample ($N = 3664$)		
	Midweek	Weekend	Midweek vs. Weekend	Midweek	Weekend	Midweek vs. Weekend
	Program	Program	(p-value)	Program	Program	(p-value)
Less than high school	0.007	0.021	0.356	0.003	0.024	0.263

Table A1 (*continued*)

Panel B: Regressions of Parental Characteristics on Treatment Status (Weekday program is the omitted category in all regressions)

	Randomization Sample (*N* = 4419)			Circle-3 Test Sample (*N* = 3664)		
	Midweek	Weekend	Midweek vs. Weekend	Midweek	Weekend	Midweek vs. Weekend
	Program	Program	(p-value)	Program	Program	(p-value)
	(0.016)	(0.017)		(0.018)	(0.018)	
High school	−0.007	−0.006	0.947	0.001	−0.004	0.799
	(0.018)	(0.017)		(0.020)	(0.019)	
Some college	0.006	0.005	0.898	0.000	0.013	0.374
	(0.015)	(0.015)		(0.017)	(0.017)	
Associate degree	0.003	0.002	0.822	0.002	−0.001	0.706
	(0.007)	(0.007)		(0.008)	(0.008)	
Bachelor's degree	−0.007	−0.010	0.590	−0.004	−0.013**	0.177
	(0.007)	(0.006)		(0.007)	(0.007)	
Graduate degree	−0.004	−0.007	0.647	−0.000	−0.008	0.148
	(0.006)	(0.006)		(0.007)	(0.006)	
Missing education	0.002	−0.004	0.562	−0.002	−0.011	0.482
	(0.010)	(0.010)		(0.012)	(0.011)	

Notes: Each row represents a separate regression model (only the coefficients of the treatments status are reported). The omitted reference group in all regressions is the original texting program of Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include randomization block fixed effects. Standard errors are clustered at the randomization block level. Circle-3 test sample is conditioned on having Circle-1 and Circle-3 literacy and math test scores. Statistical significance levels: *$p<0.10$; **$p<0.05$; ***$p<0.01$.

Panel C: Regressions of Teacher Characteristics on Treatment Status (Weekday program is the omitted category in all regressions)

	Randomization Sample (*N* = 4419)			Circle-3 Test Sample (*N* = 3664)		
	Midweek	Weekend	Midweek vs. Weekend	Midweek	Weekend	Midweek vs. Weekend
	Program	Program	(p-value)	Program	Program	(p-value)
Ave. female	−0.001	0.000	0.894	−0.001	0.005	0.370
	(0.005)	(0.005)		(0.006)	(0.006)	
Ave. black	−0.004	−0.007	0.650	−0.003	−0.004	0.916
	(0.006)	(0.006)		(0.006)	(0.008)	
Ave. Hispanic	−0.004	0.003	0.351	−0.006	−0.006	0.931
	(0.008)	(0.009)		(0.009)	(0.010)	
Ave. white	0.008	0.007	0.822	0.009	0.012	0.635
	(0.006)	(0.008)		(0.007)	(0.007)	
Ave. experience	0.121	0.068	0.661	0.182	0.096	0.505
	(0.109)	(0.130)		(0.120)	(0.143)	
Ave. days absent	−0.068	−0.193	0.623	−0.186	−0.362	0.479
	(0.198)	(0.288)		(0.212)	(0.316)	
Ave. missing absence info.	−0.008*	−0.003	0.320	−0.002	−0.003	0.754
	(0.005)	(0.005)		(0.005)	(0.005)	
SUR Joint *t*-test (p-value)[c]	0.918	0.914		0.810	0.740	

Notes: Each row represents a separate regression model (only the coefficients of the treatments status are reported). The omitted reference group in all regressions is the original texting program of Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include randomization block fixed effects. Standard errors are clustered at the randomization block level. Circle-3 test sample is conditioned on having Circle-1 and Circle-3 literacy and math test scores. [c]Seemingly Unrelated Regression *t*-test of joint significance of treatments on student, parent, and teacher characteristics. Statistical significance levels: *$p<0.10$; **$p<0.05$; ***$p<0.01$.

behavior.[8]

3. School district partner

We conducted this randomized control trial with the parents of four-year-old pre-kindergarten students in the Dallas Independent School District (ISD) in Texas during the 2016–17 academic year.[9] We recruited parents through the Dallas ISD's existing registration process for pre-

kindergarten enrollment. As all parents in the Dallas ISD must use this process to enroll in pre-kindergarten, all parents received the invitation to receive the *Tips·by·Text* program, which was available in both English and Spanish. Parents could choose to receive text messages in either English or Spanish and had the option to withdraw from the study at any time during the school year.[10] As such the *Tips·by·Text* parental enrollment process involved parents to *actively consent* to receive the text messages. Parents who chose to participate received one of the three text messaging programs from the end of September through the beginning of June.

Dallas ISD is the second-largest public school district in the state and the 14th-largest public school district in the United States, serving approximately 150,000 students from kindergarten to the twelfth grade across 224 schools. Approximately 10,000 pre-kindergarten students are enrolled in 132 preschools in the district. Dallas ISD pre-kindergarten serves students with limited English proficiency and National School Lunch Program eligibility, a diverse and low-income student population.

---

[8] The text messages draw on research on literacy development (e.g., Lonigan and Shanahan 2009), parenting practices (e.g., Reese, Sparks, and Leyva 2010), and behavior change strategies (e.g., Abraham and Michie 2008). All text messages are couched in positive parenting practices with the goal of making the activities fun and engaging for both parent and child. We consulted Parent Management Training (Patterson, Reid, and Dishion 1992), Incredible Years (Webster-Stratton 1992), Triple P-Positive Parenting Program (Sanders et al. 2000), VIPP-SD intervention (Van Zeijl et al. 2006), and Family Check-Up (Gardner, Burton, and Klimes 2006). For a more detailed description of the original text development process see York, Loeb, and Doss (2019).

[9] We used a blocked randomization design within pre-school site and preferred texting language.

[10] York et al. (2019) shows that 90 percent of economically disadvantaged families had unlimited text messaging plans.

Table A2

Differences in Observables - Active Consent Parents versus Non-Consent Parents .

—	Dependent Variables	Consent	s.e.	p-value	constant	s.e.	p-value	N	R²
(1)	Female	0.011	(0.010)	0.269	0.491***	(0.007)	0.000	9604	0.00
(2)	Black	−0.191***	(0.009)	0.000	0.366***	(0.006)	0.000	9604	0.04
(3)	Hisp	0.213***	(0.009)	0.000	0.569***	(0.006)	0.000	9604	0.05
(4)	Asian	−0.003	(0.002)	0.297	0.015***	(0.002)	0.000	9604	0.00
(5)	White	−0.019***	(0.003)	0.000	0.037***	(0.002)	0.000	9604	0.00
(6)	Other	−0.001	(0.002)	0.732	0.013***	(0.002)	0.000	9604	0.00
(7)	Low socioeconomic status	0.042***	(0.005)	0.000	0.904***	(0.004)	0.000	9604	0.01
(8)	Rapid Letter Naming	−0.027	(0.169)	0.872	6.059***	(0.115)	0.000	9604	0.00
(9)	Missing Rapid Letter Naming	−0.138***	(0.007)	0.000	0.197***	(0.005)	0.000	9604	0.04
(10)	Rapid Vocabulary	−1.003***	(0.132)	0.000	11.681***	(0.089)	0.000	9604	0.01
(11)	Missing Rapid Vocabulary	−0.233***	(0.008)	0.000	0.298***	(0.005)	0.000	9604	0.09
(12)	Phonetic Awareness	−0.348***	(0.105)	0.001	10.738***	(0.071)	0.000	9604	0.00
(13)	Missing Phonetic Awareness	−0.252***	(0.008)	0.000	0.320***	(0.005)	0.000	9604	0.10
(14)	Rote Counting	0.013	(0.013)	0.302	0.593***	(0.009)	0.000	9604	0.00
(15)	Missing Rote Counting	−0.141***	(0.007)	0.000	0.206***	(0.005)	0.000	9604	0.04
(16)	Shape Naming	0.115***	(0.030)	0.000	3.007***	(0.020)	0.000	9604	0.00
(17)	Missing Shape Naming	−0.238***	(0.008)	0.000	0.302***	(0.005)	0.000	9604	0.09
(18)	Number Discrimination	0.008	(0.014)	0.579	1.427***	(0.009)	0.000	9604	0.00
(19)	Missing Number Discrimination	−0.246***	(0.008)	0.000	0.310***	(0.005)	0.000	9604	0.09
(20)	Number Naming	0.084***	(0.027)	0.002	1.292***	(0.018)	0.000	9604	0.00
(21)	Missing Number Naming	−0.140***	(0.007)	0.000	0.208***	(0.005)	0.000	9604	0.04
(22)	Shape Discrimination	0.021	(0.030)	0.482	4.464***	(0.020)	0.000	9604	0.00
(23)	Missing Shape Discrimination	−0.251***	(0.008)	0.000	0.318***	(0.005)	0.000	9604	0.10
(24)	Counting	0.021	(0.030)	0.481	2.684***	(0.020)	0.000	9604	0.00
(25)	Missing Counting	−0.253***	(0.008)	0.000	0.326***	(0.005)	0.000	9604	0.10
(26)	Operations	−0.078***	(0.020)	0.000	0.987***	(0.013)	0.000	9604	0.00
(27)	Missing Operations	−0.253***	(0.008)	0.000	0.335***	(0.005)	0.000	9604	0.09
(28)	Average experience (in years)	0.582***	(0.123)	0.000	7.205***	(0.083)	0.000	9604	0.00
(29)	Average female	−0.030***	(0.005)	0.000	0.777***	(0.004)	0.000	9604	0.00
(30)	Average teacher: Hispanic	0.068***	(0.007)	0.000	0.365***	(0.005)	0.000	9604	0.01
(31)	Average teacher: Black	−0.059***	(0.007)	0.000	0.284***	(0.005)	0.000	9604	0.01
(32)	Average teacher: White	−0.001	(0.007)	0.931	0.326***	(0.005)	0.000	9604	0.00
(33)	Average days present	0.939***	(0.298)	0.002	162.827***	(0.202)	0.000	9604	0.00
(34)	Average days absent	−0.513***	(0.191)	0.007	9.905***	(0.130)	0.000	9604	0.00
(35)	Average days missing	−0.027***	(0.005)	0.000	0.103***	(0.003)	0.000	9604	0.00

Notes: Rows (1) - (35) are regressions. Specifically, the dependent variables are the covariates listed in Table 2 (i.e., age, sex, race or ethnicity, etc.) and the independent variable is a binary variable, $Consent_{is}$, that takes the value of 1 for those parents who consented to receive the text messages (i.e., those who are in the randomization sample, $N = 4419$) and takes the value of 0 for those parents that did not consent to receive the text messages (i.e., those who are not in the randomization sample, $N = 5185$). We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include the randomization block fixed effects and the standard errors are clustered at the randomization block level. The district did not provide us the education variable for parents who did not consent to receive the text messages. Statistical significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Twenty-seven percent of the students are identified as black, 68 percent as Hispanic, and two percent as white, with the remaining three percent as Asian, American Indian, Hawaiian, multi-racial, or without a reported ethnicity. Fifty-three percent of these students are classified as limited English proficiency, and 93 percent are classified as economically disadvantaged.[11]

4. Data sources and descriptive statistics

We use information about Dallas ISD pre-kindergarten students, their teachers, and their parents. Student data come from the Dallas ISD administrative student records, which include demographic information such as age, gender, race and ethnicity, and whether the child comes from an economically disadvantaged family. Our main outcomes are literacy and mathematics scores of the Circle Assessment System (henceforth Circle), which is a one-on-one assessment and is available in English- and Spanish-language versions.

Circle evaluates children's early language and literacy skills through three primary components: rapid letter naming, rapid vocabulary, and phonological awareness. Specifically, rapid letter naming is a one-minute timed assessment task that evaluates the child's alphabet

knowledge, rapid vocabulary is a one-minute timed assessment task that evaluates the child's ability to name common objects, and phonological awareness is a five-minute assessment that evaluates the child's understanding of sounds, including rhyming, alliteration, syllabication, and onset-rime. Circle also evaluates children's math skills through seven components: rote counting, shape naming, number discrimination, number naming, shape discrimination, counting, and operations.

Dallas ISD implements the Circle assessments three times per year: at the beginning of the school year (September and October 2016), mid-school year (January and February 2017), and end of the school year (April and May 2017). Hereafter, we refer to the beginning and the end of the school year Circle assessments as Circle-1 and Circle-3, respectively. Because Circle-1 data collection preceded the beginning of the text messaging program, we use its literacy and math measures as covariates in our regression models to increase precision and to check the balance in observed child characteristics. Circle-3 is our primary outcome of interest.

Data about teachers comes from the Dallas ISD administrative records that includes gender, race and ethnicity, years of experience, and days absent from work during the school year. As each child can have multiple teachers, for each student, we use mean characteristics of all their teachers during the school year. Lastly, we obtain data on parents from the enrollment forms, which provide information on their preferred texting language and highest educational attainment. We use data on teacher and parent characteristics as covariates in our regression models to improve precision.

---

[11] Numbers are based on the Dallas ISD enrollment statistics as of June 2018. See https://mydata.dallasisd.org/SL/SD/ENROLLMENT/Enrollment.jsp for more information

Table A3
Differences in Observables - Attrited from the RCT versus Non-Attrited from the RCT.

—	Dependent Variables	Attrite	s.e.	p-value	constant	s.e.	p-value	N	R²
(1)	Female	−0.052**	(0.022)	0.019	0.511***	(0.004)	0.000	4419	0.00
(2)	Age	−0.011	(0.015)	0.435	4.566***	(0.002)	0.000	4419	0.01
(3)	Black	0.045**	(0.018)	0.012	0.168***	(0.003)	0.000	4419	0.50
(4)	Hispanic	−0.055***	(0.020)	0.007	0.791***	(0.003)	0.000	4419	0.47
(5)	Asian	0.003	(0.005)	0.594	0.012***	(0.001)	0.000	4419	0.21
(6)	White	0.002	(0.007)	0.781	0.018***	(0.001)	0.000	4419	0.02
(7)	Other	0.005	(0.006)	0.389	0.011***	(0.001)	0.000	4419	0.02
(8)	Low socioeconomic status	−0.070***	(0.014)	0.000	0.958***	(0.002)	0.000	4419	0.03
(9)	Rapid Letter Naming	−1.192***	(0.345)	0.001	6.236***	(0.059)	0.000	4419	0.14
(10)	Missing Rapid Letter Naming	0.348***	(0.024)	0.000	−0.001	(0.004)	0.873	4419	0.39
(11)	Rapid Vocabulary	−0.565*	(0.331)	0.088	10.774***	(0.057)	0.000	4419	0.19
(12)	Missing Rapid Vocabulary	0.390***	(0.024)	0.000	−0.002	(0.004)	0.646	4419	0.41
(13)	Phonetic Awareness	−0.891***	(0.275)	0.001	10.543***	(0.047)	0.000	4419	0.28
(14)	Missing Phonetic Awareness	0.409***	(0.025)	0.000	−0.002	(0.004)	0.617	4419	0.43
(15)	Rote Counting	−0.079***	(0.030)	0.008	0.620***	(0.005)	0.000	4419	0.12
(16)	Missing Rote Counting	0.392***	(0.026)	0.000	−0.002	(0.004)	0.695	4419	0.42
(17)	Shape Naming	−0.285***	(0.068)	0.000	3.171***	(0.012)	0.000	4419	0.14
(18)	Missing Shape Naming	0.398***	(0.024)	0.000	−0.004	(0.004)	0.340	4419	0.41
(19)	Number Discrimination	−0.090***	(0.031)	0.004	1.450***	(0.005)	0.000	4419	0.04
(20)	Missing Number Discrimination	0.398***	(0.024)	0.000	−0.004	(0.004)	0.367	4419	0.40
(21)	Number Naming	−0.152***	(0.052)	0.004	1.402***	(0.009)	0.000	4419	0.07
(22)	Missing Number Naming	0.408***	(0.027)	0.000	−0.002	(0.005)	0.699	4419	0.44
(23)	Shape Discrimination	−0.239***	(0.077)	0.002	4.526***	(0.013)	0.000	4419	0.12
(24)	Missing Shape Discrimination	0.415***	(0.025)	0.000	−0.004	(0.004)	0.344	4419	0.42
(25)	Counting	−0.288***	(0.069)	0.000	2.754***	(0.012)	0.000	4419	0.13
(26)	Missing Counting	0.438***	(0.026)	0.000	−0.002	(0.004)	0.651	4419	0.46
(27)	Operations	−0.126***	(0.044)	0.005	0.931***	(0.008)	0.000	4419	0.19
(28)	Missing Operations	0.486***	(0.027)	0.000	−0.000	(0.005)	0.954	4419	0.51
(29)	Less than high school	−0.021	(0.016)	0.193	0.321***	(0.003)	0.000	4419	0.23
(30)	High school	−0.010	(0.022)	0.666	0.322***	(0.004)	0.000	4419	0.02
(31)	Some college	0.007	(0.018)	0.724	0.192***	(0.003)	0.000	4419	0.14
(32)	Associate degree	0.003	(0.010)	0.775	0.043***	(0.002)	0.000	4419	0.02
(33)	Bachelor's degree	0.020**	(0.009)	0.024	0.026***	(0.001)	0.000	4419	0.01
(34)	Graduate degree	0.009	(0.008)	0.262	0.021***	(0.001)	0.000	4419	0.02
(35)	Missing education	−0.007	(0.012)	0.554	0.075***	(0.002)	0.000	4419	0.08
(36)	Average experience (in years)	−0.483*	(0.252)	0.056	7.869***	(0.043)	0.000	4419	0.70
(37)	Average female	−0.014	(0.011)	0.183	0.750***	(0.002)	0.000	4419	0.68
(38)	Average teacher: Hispanic	−0.047***	(0.018)	0.009	0.441***	(0.003)	0.000	4419	0.64
(39)	Average teacher: Black	0.006	(0.012)	0.593	0.224***	(0.002)	0.000	4419	0.72
(40)	Average teacher: White	0.033**	(0.016)	0.042	0.319***	(0.003)	0.000	4419	0.67
(41)	Average days present	−1.480**	(0.611)	0.016	164.019***	(0.104)	0.000	4419	0.69
(42)	Average days absent	1.093***	(0.364)	0.003	9.205***	(0.062)	0.000	4419	0.58
(43)	Average days missing	0.034**	(0.013)	0.011	0.071***	(0.002)	0.000	4419	0.55
(44)	Teacher Count	−0.120	(0.221)	0.587	9.720***	(0.038)	0.000	4418	0.48

Notes: Rows (1) - (44) are regressions. Specifically, the dependent variables in each of the regressions are the covariates listed in Table 2 (i.e., age, sex, race or ethnicity, etc.), and the independent variable is a binary variable, Attrite$_{is}$, that takes the value of 1 for those observations not in the analytic sample (i.e., those who attrited from the randomization sample, $N = 755$) and taking the value of 0 for those observations that where analyzed (i.e., those in the analytical sample, $N = 3664$). We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include the randomization block fixed effects and the standard errors are clustered at the randomization block level. Standard errors are clustered at the randomization block level. Statistical significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 2 presents the descriptive statistics based on two samples: the randomization sample ($N = 4419$) and the analytic sample ($N = 3664$), which includes all observations that do not have missing literacy and math outcomes in both Circle-1 and Circle-3 assessments. As shown in Table 2 (panel A), most of the children in the randomization sample are Hispanic (78 percent) followed by black (18 percent), two percent are white, and one percent are Asian. Ninety-five percent of the randomization sample is economically disadvantaged, and the average age among children is 4.56 years. These demographic characteristics are similar to those of the population of DISD pre-kindergarten students, except for Hispanic children are slightly overrepresented, and black and white children are slightly underrepresented.

Table 2 also shows the raw Circle-1 literacy and math test scores, which we standardize within assessment language for the analysis. On average, children name 6.03 letters in one minute of the rapid letter naming assessment, identify 10.68 objects in one minute of the rapid vocabulary assessment, and correctly answer 10.39 items in the phonological awareness assessment. For reference, the maximum score in the rapid letter naming assessment is 52 (26 lower-case and 26 upper-

case letters in the alphabet); in the rapid vocabulary assessment the maximum score is 55 objects named with vocabulary; and in the phonological awareness assessment the maximum score is 28 points. The children in our sample had rapid letter naming and rapid vocabulary scores lower than age-appropriate standards, and phonological awareness scores higher than age-appropriate standards. According to the Circle user guide of 2016–17, the benchmarks for English-speaking children between ages four and 4.5 are 8 letters for rapid letter naming, 16 items for rapid vocabulary, and 8 points for phonological awareness.[12] For the math test scores, on average, children in our sample score: 0.61 points on rote counting, 3.12 points on shape naming, 1.43 points on number discrimination, 1.43 points on number

---

[12] The benchmarks for their Spanish-speaking counterparts are 6 letters, 12 items, and 5 points, respectively. See Children's Learning Institute. (2016). *CIRCLE progress monitoring: User guide 2016-17.* Houston, TX: Children's Learning Institute. Retrieved from: https://cliengage.org/user-guides/ CIRCLE_Progress_Monitoring_User_Guide_91416.pdf.

Table A4
Alternative Regression Specifications - Treatment Effects on Language/Literacy and Math Circle-3 Results .

	Panel A: Language/Literacy Circle-3 Assessment Test				Panel B: Math Circle-3 Assessment Test							
	Overall Literacy (1)	Rapid Letter Naming (2)	Rapid Vocabulary (3)	Phonological Awareness (4)	Overall Math (5)	Rote Counting (6)	Shape Naming (7)	Number Discrimination (8)	Number Naming (9)	Shape Discrimination (10)	Counting (11)	Operations (12)
Midweek Program, T1	−0.036	−0.003	−0.043	−0.063	0.019	0.059	0.127**	−0.022	0.021	−0.017	0.022	−0.032
	(0.037)	(0.048)	(0.051)	(0.046)	(0.027)	(0.051)	(0.056)	(0.066)	(0.048)	(0.063)	(0.054)	(0.053)
Weekend Program, T2	0.062*	0.157***	0.071	−0.042	0.062**	0.102**	0.158***	−0.010	0.106**	0.037	0.094*	−0.010
	(0.032)	(0.044)	(0.044)	(0.045)	(0.025)	(0.049)	(0.058)	(0.070)	(0.046)	(0.058)	(0.052)	(0.052)
Upper-half of Circle-1, covariate (cv)	0.052	0.115*	−0.001	0.040	−0.019	0.015	0.041	−0.035	0.092	−0.134*	−0.080	−0.106*
	(0.045)	(0.061)	(0.065)	(0.055)	(0.033)	(0.065)	(0.068)	(0.071)	(0.061)	(0.077)	(0.072)	(0.063)
T1 * covariate	0.062	0.056	0.092	0.039	−0.040	−0.064	−0.148**	−0.010	−0.037	0.033	−0.060	−0.066
	(0.046)	(0.061)	(0.066)	(0.061)	(0.032)	(0.064)	(0.064)	(0.078)	(0.063)	(0.080)	(0.070)	(0.071)
T2 * covariate	−0.072*	−0.092	−0.094	−0.030	−0.064**	−0.072	−0.123*	0.009	−0.130**	0.004	−0.110*	−0.054
	(0.043)	(0.062)	(0.063)	(0.057)	(0.030)	(0.060)	(0.069)	(0.077)	(0.058)	(0.075)	(0.065)	(0.067)
P-value: test btw. T1 and T2	0.018	0.002	0.042	0.683	0.136	0.384	0.569	0.860	0.070	0.430	0.184	0.707
P-value: test btw. T1*cv and T2*cv	0.005	0.017	0.007	0.298	0.463	0.895	0.697	0.807	0.095	0.730	0.436	0.876
Adj. R²	0.523	0.441	0.437	0.412	0.472	0.288	0.314	0.084	0.390	0.172	0.289	0.305
N	3664	3664	3664	3664	3664	3664	3664	3664	3664	3664	3664	3664

Notes: Only the regression coefficients of the treatment's status and the interactions with the treatment status are reported. The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include controls for student characteristics (age, gender, race/ethnicity, and low-SES status), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing) and randomization block fixed effects. Standard errors are clustered at the randomization block level. Statistical significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

naming, 4.48 points on shape discrimination, 2.70 points on counting, and 0.91 points on operations. These math scores are also consistently lower than age-appropriate standards.[13]

Panels B and C of Table 2 show parent and teacher characteristics, respectively. On average, parents tend to have relatively low levels of educational attainment. In the randomization sample, 32 percent of parents have less than a high school degree, 32 percent have a high school degree, and only 28 percent have completed at least some college. Fifty-seven percent of parents chose to receive text messages in English, and the remaining 43 percent chose to receive them in Spanish. Three-quarters of the teachers are female, with an average teaching experience in the district of 7.79 years. The racial and ethnic demographics of teachers differ from their students: 43 percent are black, 22 percent are Hispanic, and 32 percent are non-Hispanic white.

V. Empirical strategy

We estimate the treatment effect of the **Midweek** and **Weekend** programs compared to the control **Weekday** program using the following model specification:

$$y_{is} = \alpha + \beta_1 \cdot Midweek_{is} + \beta_2 \cdot Weekend_{is} + \delta \cdot X_{is} + \gamma_s + \varepsilon_{is} \tag{1}$$

where y_{is} is the outcome of interest of child i in randomization block s. We used a blocked randomization design within the pre-school site and preferred texting language. The outcomes are Circle-3 literacy and mathematics outcomes. We standardize both overall scores and individual components (e.g., rapid letter naming, shape discrimination, etc.) to have a standard deviation of one and a mean of zero. We standardize the literacy assessments within assessment language because of language-specific assessment differences.[14] The variables $MidWeek_{is}$ and $Weekend_{is}$ are binary indicators for whether a parent received the **Midweek** or the **Weekend** programs, respectively, in comparison to the original **Weekday** program. X_{is} is a vector of covariates that includes the child, parent, and teacher characteristics. These covariates include the child's age, gender, race and ethnicity, whether they are economically disadvantaged, and their Circle-1 literacy and mathematics outcomes, which were measured before the text messaging programs began; the parents' highest educational attainment; and the averaged characteristics of a child's teachers, including their gender, race and ethnicity, and days absent from work during the school year. γ_s indicates randomization block fixed effects, and ε_{is} is a child-level error-term. We cluster standard errors at the randomization block level.

The coefficients of interest are β_1 and β_2 as they measure the causal effects of receiving the **Midweek** or the **Weekend** programs compared to the original **Weekday** program. To explore heterogeneity of these treatment effects, we also estimate Eq. (1) by several subgroups: parent's educational attainment (i.e., high school degree or lower versus greater than a high school degree) and child's pre-intervention literacy skill level (i.e., lower half versus upper half of the respective Circle-1 literacy distribution). Specifically, for the heterogeneity analysis by the child's skill level, for literacy outcomes, we split the sample based on the distribution of overall literacy score. For math outcomes, we split the sample based on the distribution of the overall literacy score as well. We split the math outcomes based on the distribution of overall literacy

---

[13] According to the CIRCLE user guide of 2016-17, the benchmarks for both English and Spanish-speaking children between ages four and 4.5 are 2 points for rote counting, 4 points for shape naming, 2 points for number discrimination, 3 points for number naming, 5 points for shape discrimination, 4 points for counting, and 3 points for operations.

[14] We standardized within testing language (English or Spanish) because as noted earlier the benchmarks for the Spanish version of the test differs from those of the English version because they correspond to the 16th percentile of the version-specific score distribution. Moreover, the Spanish alphabet has twenty-seven letters compared with twenty-six letters in the English alphabet.

Table A5
Treatment Effects by Non-Hispanic and Hispanic Students.

	Panel A: Language & Literacy Skills,		Panel B: Rapid Letter Naming,		Panel C: Rapid Vocabulary Naming,		Panel D: Phonological Awareness,	
	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic
Midweek Program	−0.066	−0.008	−0.027	0.029	−0.111	0.008	−0.060	−0.061*
	(0.051)	(0.029)	(0.062)	(0.038)	(0.076)	(0.040)	(0.077)	(0.037)
Weekend Program	−0.079	0.037	−0.038	0.136***	−0.110	0.039	−0.089	−0.063*
	(0.062)	(0.025)	(0.082)	(0.032)	(0.103)	(0.035)	(0.069)	(0.036)
P-value: Midweek vs. Weekend	0.835	0.129	0.888	0.006	0.989	0.441	0.714	0.942
Adj. R^2	0.65	0.51	0.60	0.40	0.43	0.44	0.56	0.40
N	719	2945	719	2945	719	2945	719	2945
	Panel E: Mathematics,		Panel F: Rote Counting,		Panel G: Shape Naming,		Panel H: Num. Discrimination,	
	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic
Midweek Program	−0.039	0.007	−0.041	0.041	−0.042	0.084**	−0.056	−0.007
	(0.047)	(0.018)	(0.097)	(0.035)	(0.093)	(0.038)	(0.111)	(0.043)
Weekend Program	−0.010	0.040**	0.059	0.075**	−0.022	0.119***	0.012	−0.013
	(0.047)	(0.018)	(0.076)	(0.036)	(0.094)	(0.038)	(0.117)	(0.048)
P-value: Midweek vs. Weekend	0.460	0.098	0.254	0.333	0.803	0.339	0.458	0.882
Adj. R^2	0.55	0.47	0.32	0.29	0.36	0.32	0.07	0.09
N	719	2945	719	2945	719	2945	719	2945
	Panel: I Number Naming,		Panel: J Shape Discrimination,		Panel: K Counting,		Panel: L Operations,	
	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic
Midweek Program	−0.041	0.007	0.016	−0.004	−0.079	0.009	−0.102	−0.067*
	(0.076)	(0.034)	(0.124)	(0.043)	(0.091)	(0.037)	(0.096)	(0.038)
Weekend Program	−0.018	0.063*	0.033	0.037	−0.057	0.055	−0.046	−0.036
	(0.087)	(0.033)	(0.113)	(0.041)	(0.109)	(0.038)	(0.094)	(0.038)
P-value: Midweek vs. Weekend	0.735	0.111	0.887	0.337	0.827	0.234	0.550	0.439
Adj. R^2	0.50	0.37	0.27	0.17	0.36	0.28	0.32	0.30
N	719	2945	719	2945	719	2945	719	2945

Notes: Only the regression coefficients of the treatment's status are reported. The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include controls for student characteristics (age, gender, and low-SES status), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing) and randomization block fixed effects. Standard errors are clustered at the randomization block level. Statistical significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

score because this would allow us to further explore the potential *spill-over* or *crowding out* effects of the texting program on the child's math development.

VI. Randomization checks and attrition analysis

In a randomized control trial, the characteristics of the treatment groups should not differ systematically. Systematic differences among the treatment groups could potentially bias the estimated treatment effects. Therefore, in this section, we conduct a series of randomization checks for all covariates to evaluate covariate balance. That is, we estimate the following randomization block fixed effect models for each of our child, parent, and teacher covariates:

$$X_{is} = \alpha + \beta_1 \cdot MidWeek_{is} + \beta_2 \cdot Weekend_{is} + \gamma_s + \varepsilon_{is} \qquad (2)$$

Small and statistically insignificant values for the coefficients of the treatment indicators β_1 and β_2 would indicate a successful randomization. We present results of the randomization checks in the online appendix table (see Table A1) separately for the randomization sample and the analytic sample. The results provide evidence that the randomization was successful. Of the 84 point estimates using the randomization sample, only three coefficients are significant at the five percent level or less, and two coefficients are significant at the ten percent level or less. Of the 64 point estimates using the analytic sample, one coefficient is significant at the five percent level or less, and three coefficients are significant at the ten percent level or less. This number of statistically significant point estimates can be expected to be produced by chance. Moreover, tests for joint significance using seemingly unrelated regressions, as shown in the bottom of Panel C of Table A1, fail to reject the

null hypothesis that the coefficients of the treatments across the covariates are jointly equal to zero.[15]

Because we restrict our analytic sample to include only the children that have all Circle-1 and Circle-3 literacy and math components, one might worry about selective attrition. That is, if attrition is related to the treatment conditions, the estimated effects would be biased. For example, if assessment completion is higher for low-performing children whose parents received the **Weekend** program, compared to children whose parents received the **Weekday** program, then the estimated effect of the **Weekend** program compared to the **Weekday** program could be biased upward. Thus, to assess systematic attrition differences between the programs, we use the following a randomization block fixed effects regression model:

$$A_{is} = \alpha + \beta_1 \cdot Midweek_{is} + \beta_2 \cdot Weekend_{is} + \delta \cdot X_{is} + \gamma_s + \varepsilon_{is} \qquad (3)$$

---

[15] Though our randomization was successful (see balancing checks in appendix Table A1), we do observe statistical differences in sample characteristics from those parents who consented to receive text messages versus those parents who did not consent to receive texts. As noted earlier all Dallas ISD parents who had a preschooler were invited to receive the text messages but only those parents who gave us their consent (i.e., active consent) to receive the texts are in the randomization sample. As shown in appendix Table A2, parents of Black and white students were less likely to consent to receive texts, while parents of Hispanic and low socioeconomic status students were more likely to consent to receive text messages. Thus, our randomization sample is internally valid, however, in terms of external validity our treatment effects are more applicable to school districts that serve a heavily Hispanic and lower SES student population.

Table A6

Math Results - Treatment Effects by Parent's Educational Attainment and Child's Pre-Intervention Circle-1 Test Scores.

	Panel A: Math (overall score)				Panel E: Number Naming			
	≤ HS	> HS	Lower	Upper	≤ HS	> HS	Lower	Upper
Midweek Program	−0.025	0.009	0.015	−0.009	−0.010	0.010	0.005	0.024
	(0.021)	(0.031)	(0.028)	(0.019)	(0.039)	(0.065)	(0.054)	(0.040)
Weekend Program	0.021	0.010	0.053*	−0.002	0.051	−0.015	0.059	0.032
	(0.021)	(0.034)	(0.028)	(0.019)	(0.039)	(0.063)	(0.052)	(0.043)
P-value: Midweek vs. Weekend	0.049	0.956	0.183	0.737	0.132	0.686	0.277	0.865
Adj. R^2	0.465	0.517	0.45	0.46	0.394	0.416	0.37	0.37
N	2382	1006	1828	1836	2382	1006	1828	1836

	Panel B: Rote Counting				Panel F: Shape Discrimination			
	≤ HS	> HS	Lower	Upper	≤ HS	> HS	Lower	Upper
Midweek Program	0.005	0.004	−0.015	0.078*	−0.066	0.015	0.013	−0.028
	(0.040)	(0.064)	(0.054)	(0.041)	(0.048)	(0.090)	(0.070)	(0.042)
Weekend Program	0.055	−0.011	0.053	0.057	−0.005	0.030	0.051	−0.010
	(0.042)	(0.072)	(0.055)	(0.057)	(0.047)	(0.088)	(0.070)	(0.040)
P-value: Midweek vs. Weekend	0.214	0.823	0.173	0.611	0.218	0.832	0.557	0.680
Adj. R^2	0.293	0.280	0.29	0.27	0.175	0.233	0.17	0.15
N	2382	1006	1828	1836	2382	1006	1828	1836

	Panel C: Shape Naming				Panel G: Counting			
	≤ HS	> HS	Lower	Upper	≤ HS	> HS	Lower	Upper
Midweek Program	0.050	0.116*	0.143**	−0.015	−0.032	−0.070	0.015	−0.025
	(0.047)	(0.068)	(0.058)	(0.037)	(0.046)	(0.063)	(0.058)	(0.036)
Weekend Program	0.098**	0.105	0.202***	−0.011	0.038	−0.004	0.058	0.021
	(0.045)	(0.084)	(0.058)	(0.037)	(0.043)	(0.066)	(0.061)	(0.044)
P-value: Midweek vs. Weekend	0.251	0.883	0.267	0.919	0.119	0.345	0.443	0.329
Adj. R^2	0.335	0.287	0.34	0.27	0.290	0.300	0.33	0.25
N	2382	1006	1828	1836	2382	1006	1828	1836

	Panel D: Number Discrimination				Panel H: Operations			
	≤ HS	> HS	Lower	Upper	≤ HS	> HS	Lower	Upper
Midweek Program	−0.090*	0.061	−0.033	−0.024	−0.111**	−0.026	−0.022	−0.096*
	(0.054)	(0.094)	(0.081)	(0.041)	(0.044)	(0.060)	(0.054)	(0.049)
Weekend Program	−0.063	0.111	−0.043	−0.014	−0.063	−0.059	0.013	−0.110**
	(0.054)	(0.083)	(0.083)	(0.042)	(0.044)	(0.069)	(0.057)	(0.049)
P-value: Midweek vs. Weekend	0.571	0.585	0.893	0.801	0.293	0.660	0.566	0.785
Adj. R^2	0.109	0.035	0.09	0.01	0.282	0.310	0.28	0.28
N	2382	1006	1828	1836	2382	1006	1828	1836

Notes: Only the regression coefficients of the treatment's status are reported. The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. Regression samples were split for students with parents with more than a high school education (> HS) and a high school education and less (≤ HS). All regressions split by parental education control for student characteristics (age, gender, race/ethnicity, low-SES status, and Circle-1 test scores), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing), and randomization block fixed effects. Regression samples were split at the median (lower-half vs. upper-half) based on student's Circle-1 language and literacy test prior to the intervention. All regressions split by pre-intervention Circle-1 test control for student characteristics (age, gender, race/ethnicity, and, low-SES status), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing), and randomization block fixed effects. Standard errors are clustered at the randomization block level. Statistical significance levels: *$p<0.10$; **$p<0.05$; ***$p<0.01$.

where A_{is} is a binary variable, which equals 1 if an observation is not included in the analytic sample and 0 otherwise. Statistically insignificant values for the coefficients β_1 and β_2 would indicate that neither treatment group is more likely to experience attrition. We also estimate this model in the upper and lower halves of the Circle-1 literacy and mathematics distribution. Table 3 shows the estimates for this analysis. The results provide evidence that selective attrition is not a problem in our study. None of the estimated coefficients of the **Midweek** and

Weekend programs are statistically significant, nor are these coefficients significantly different from each other. Though we do not find any selective attrition by treatment conditions, we do, observe statistical differences in sample characteristics from those parents (or children) who attritted from the randomization sample versus those who were included in the analytical sample.[16] Specifically, female, Hispanic, lower SES status students were less likely to attrite, but Black students were more likely to attrite. Regarding the pre-intervention Circle-1 literacy and math assessment scores, we see that, on average, lower-skilled students

[16] We estimated several of the following bivariate regressions, $X_{is} = \alpha + \beta_1 \cdot Attrite_{is} + \gamma_s + \varepsilon_{is}$ where the dependent variables, X_{is}, in each of the regressions are the covariates listed in Table 2 (i.e., age, sex, race or ethnicity, etc.) and the independent variable is a binary variable, $Attrite_{is}$, that takes the value of 1 for those observations not in the analytic sample (i.e., those who attritted from the randomization sample, N = 755) and taking the value of 0 for those observations that where analyzed (i.e., those in the analytical sample, N = 3,664). γ_s indicates randomization block fixed effects. ε_{is} is a child-level error-term. We cluster standard errors at the randomization block level. These results are reported in appendix Table A3.

Table A7
Circle-2 Results - Language and Literacy Treatment Effects (standardized).

—	Midweek Program	Weekend Program	Midweek vs. Weekend (p-value)	Adj. R^2	N
Panel A: Language and Literacy Skills (overall score)					
Entire sample	0.015	0.025	0.652	0.555	3606
	(0.023)	(0.021)			
Lower half	−0.002	0.072**	0.029	0.401	1826
	(0.034)	(0.032)			
Upper half	0.059*	−0.007	0.045	0.480	1780
	(0.033)	(0.031)			
Panel B: Rapid Letter Naming					
Entire sample	−0.010	0.046*	0.059	0.515	3606
	(0.031)	(0.027)			
Lower half	−0.033	0.104**	0.003	0.390	1826
	(0.046)	(0.045)			
Upper half	0.057	0.018	0.380	0.495	1780
	(0.046)	(0.041)			
Panel C: Rapid Vocabulary Naming					
Entire sample	0.022	0.005	0.642	0.464	3606
	(0.031)	(0.033)			
Lower half	0.001	0.054	0.307	0.367	1826
	(0.046)	(0.046)			
Upper half	0.068	−0.035	0.040	0.418	1780
	(0.046)	(0.050)			
Panel D: Phonological Awareness					
Entire sample	0.032	0.023	0.766	0.395	3606
	(0.033)	(0.031)			
Lower half	0.028	0.059	0.552	0.318	1826
	(0.055)	(0.048)			
Upper half	0.052	−0.004	0.209	0.331	1780
	(0.046)	(0.045)			

Notes: Each row represents a separate regression model (only the coefficients of the treatments status are reported). The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include controls for student characteristics (age, gender, race/ethnicity, low-SES status, and Circle-1 test scores), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing) and randomization block fixed effects. Standard errors are clustered at the randomization block level. Regressions in samples split at medians are based on student's Circle-1 language and literacy test prior to the intervention. Statistical significance levels: *p<0.10; **p<0.05; ***p<0.01.

were more likely to attrite. Also, more educated parents were more likely to attrite.

VII. Results

A Language and literacy results

Table 4 presents the language and literacy results for the aggregate score and the sub-scores of the Circle-3 skills assessment test, and Table 5 reports on the same literacy outcomes by several subgroups of interest.[17] Only the coefficients for the Midweek and Weekend programs are reported, and the omitted reference group in all regressions is the Weekday program. Depending on the regression specification, we also include controls for student, parent, and teacher characteristics. All regressions include randomization block fixed effects (i.e., pre-school site and texting language). Standard errors are clustered at the randomization block level. Moreover, to differentiate between the *spacing effect* and the *weekend effect* between the texting programs, we formally conduct a test between the **Midweek** program coefficient versus the **Weekend** program coefficient (i.e., H$_0$: $\beta_1 = \beta_2$; H$_1$: $\beta_1 \neq \beta_2$), the p-value on this test is reported in each panel.

Panel A of Table 4 shows the results for the aggregate literacy and language score. Although we do not find statistically significant differences in treatment status for the overall literacy score, we do, however, find treatment effects for the literacy assessment components. Panels B-D of Table 4 report the results for the three components: rapid letter naming, rapid vocabulary, and phonological awareness. We find that children whose parents received the **Weekend** program scored, on average, 0.112 standard deviations (SDs) higher in rapid letter naming (least advanced skill) than children whose parents received the **Weekday** program. A 0.112 SD increase in rapid letter naming is an increase of about 1.59 alphabet letters (or a 5 percent increase relative to the control mean of 30.19). Moreover, the coefficient of the **Weekend** program is significantly larger in magnitude than of the **Midweek** program, and we are able to reject the null hypothesis (p-value: 0.009) that both programs are the same. This result shows that sending texts on the weekend rather than on consecutive days is more beneficial to children in developing letter knowledge. Interestingly, for the phonological awareness score, which is considered a mega-cognitive skill in child development (i.e., the child's understanding of sounds, including rhyming, alliteration, syllabication, and onset-rime), we find a marginally significant negative program effect (−0.057 SDs) for children whose parents received the **Weekend** program relative to children whose parents received the **Weekday** program.[18]

Next, Table 5 presents the literacy treatment effects by two subgroups: parent's educational attainment (panel A) and the child's pre-

---

[17] The Circle assessment is administered three times during the school year: Circle-1 is carried out in the beginning of the year (September/October 2016), Circle-2 is carried out in the middle of the year (January/February 2017), and Circle-3 is carried out at the end of the year (April/May 2017). Because the first assessment of Circle occurred before the intervention started, we use Circle-1 results as covariates in all regression specifications. Our main set of child outcomes comes from the third assessment (Circle-3), as parents and children had the most exposure to the program. The analysis on Circle-2 results for both literacy and math assessments are weaker than the Circle-3 literacy and math results. Circle-2 results are reported in Tables A7 and A8 of the appendix.

[18] Not surprisingly, given that our district partner's student population is primarily Hispanic, our results reported in Table 4 are largely driving by Hispanic children in the district. Again, we find significant positive **Weekend** program effects for Hispanic children's rapid letter naming score (0.136 SDs) relative to Hispanic children whose parents receive the **Weekday** program, but marginally negative **Weekend** program effects on the phonological awareness score (−0.06 SDs) for Hispanic children relative to those children whose parents received the Weekday texting program. These results are shown in appendix Table A5.

Table A8

Circle-2 Results - Math Treatment Effects (standardized).

	Midweek Program	Weekend Program	Midweek vs. Weekend (p-value)	Adj. R²	N	Midweek Program	Weekend Program	Midweek vs. Weekend (p-value)	Adj. R²	N
			Panel A: Mathematics Skills Assessment (overall score)					Panel E: Number Naming		
Entire sample	−0.013	0.028	0.035	0.568	3606	−0.011	0.018	0.304	0.497	3606
	(0.020)	(0.017)				(0.031)	(0.029)			
Lower half	−0.017	0.058*	0.019	0.501	1826	−0.043	0.003	0.410	1826	1826
	(0.033)	(0.032)				(0.045)	(0.049)			
Upper half	−0.000	0.007	0.773	0.543	1780	0.024	0.037	0.494	1780	1780
	(0.025)	(0.023)				(0.045)	(0.044)			
		Panel B: Rote Counting						Panel F: Shape Discrimination		
Entire sample	−0.030	0.059*	0.010	0.361	3606	−0.023	0.000	0.541	0.215	3606
	(0.034)	(0.033)				(0.038)	(0.036)			
Lower half	−0.035	0.089	0.028	0.333	1826	−0.020	0.061	0.230	0.200	1826
	(0.055)	(0.058)				(0.066)	(0.064)			
Upper half	−0.001	0.022	0.643	0.313	1780	−0.027	−0.039	0.811	0.199	1780
	(0.046)	(0.043)				(0.044)	(0.050)			
		Panel C: Shape Naming						Panel G: Counting		
Entire sample	0.003	0.069**	0.047	0.391	3606	−0.005	0.030	0.307	0.345	3606
	(0.034)	(0.032)				(0.033)	(0.035)			
Lower half	0.025	0.134**	0.051	0.363	1826	−0.018	0.080	0.092	0.322	1826
	(0.061)	(0.056)				(0.052)	(0.062)			
Upper half	−0.019	0.019	0.383	0.356	1780	0.042	0.011	0.494	0.313	1780
	(0.045)	(0.043)				(0.044)	(0.045)			
		Panel D: Number Discrimination						Panel H: Operations		
Entire sample	−0.093**	−0.034	0.120	0.161	3606	0.014	0.037	0.509	0.360	3606
	(0.041)	(0.036)				(0.034)	(0.031)			
Lower half	−0.084	−0.014	0.296	0.151	1826	0.023	0.053	0.565	0.322	1826
	(0.078)	(0.067)				(0.058)	(0.053)			
Upper half	−0.064	−0.014	0.259	0.129	1780	−0.006	−0.002	0.950	0.315	1780
—	(0.043)	(0.044)	—	—	—	—	(0.052)	(0.055)	—	—	—

Notes: Each row represents a separate regression model (only the coefficients of the treatments status are reported). The Midweek program sends text messages on Tuesday, Wednesday, and Thursday. The Weekend program sends text messages on Friday, Saturday, and Sunday. The omitted reference group in all regressions is the Weekday program, which sends text messages on Monday, Wednesday, and Friday. We used a blocked randomization design within the pre-school site and preferred texting language. All regressions include controls for student characteristics (age, gender, race/ethnicity, low-SES status, and Circle-1 test scores), parental characteristics (highest educational attainment), teacher characteristics (gender, race/ethnicity, years of experience, days absent and days missing) and randomization block fixed effects. Standard errors are clustered at the randomization block level. Regressions in samples split at medians are based on student's Circle-1 language and literacy test prior to the intervention. Statistical significance levels: *p<0.10; **p<0.05; ***p<0.01.

intervention literacy score (panel B) as proxies for competing demands on parent's time and attention. This heterogeneity analysis aims to shed light at whether parents with more constraints can take advantage of the program differently when it is sent during the weekend rather than the week, either because they have more time or because they face fewer distractions during the weekend. Specifically, panel A reports on the heterogeneity analysis by whether a parent has a high school degree or lower (i.e., proxy for low-SES) versus whether a parent has greater than a high school degree (i.e., proxy for high-SES). We find statistically significant positive **Weekend** program effects on rapid letter naming (0.105 SDs) for children whose parents are low-SES relative to children whose parents are also low-SES but received the **Weekday** program instead. Again, the coefficient of the **Weekend** program is significantly larger in magnitude than of the **Midweek** program, and we are able to reject the null hypothesis (p-value: 0.013) that both programs are the same. In contrast, we find a marginally negative **Weekend** program effect on the phonological awareness score (−0.111 SDs) for children whose parents are high-SES in comparison to children whose parents are also high-SES but received instead the **Weekday** program. To the extent that parental educational attainment is a good proxy to one's job flexibly then these results provide suggestive evidence that the **Weekend** program benefits lower-SES children's literacy development more than the **Weekday** program, but the **Weekend** program might not be as beneficial for higher-SES children.

The gains in literacy are more pronounced for children in the lower half of Circle-1 literacy score distribution. As shown in panel B of Table 5, we find that children who start the school year in the lower half of literacy distribution benefit more from the **Weekend** program.

Initially, lower-achieving children whose parents received the text messages over the weekend scored, on average, 0.061 SDs higher than similar children whose parents received the texts over the weekday. Lower-skilled children's rapid letter naming scores, on average, increased by 0.167 SDs, while the scores of children in the upper half only marginally increased by 0.073 SDs compared to children in the **Weekday** program. A 0.167 SD increase in rapid letter naming translates to an increase of about 2.49 alphabet letters for lower-skilled children (or a 9 percent increase relative to the control mean of 26.38). Again, the test across the **Midweek** vs. **Weekend** programs (p-value: 0.002) also confirms that offering texts over the weekend rather than on consecutive days is more beneficial in increasing the alphabet skills of lower-skilled children. Also, initially higher-performing children whose parents received the **Weekend** program scored lower in phonological awareness (−0.069 SDs), which is the more difficult skill, than children whose parents received the **Weekday** program, again emphasizing the heterogeneous effects of the two programs.[19] These results are consistent with the findings of previous studies (York et al., 2019; and Cortes et al., 2021) that our texting program does well in increasing the literacy development of lower-skilled children.

A Spillover or crowding out effects on math development

---

[19] Appendix Table A4 provides an alternative model specification where we interact the Circle-1 literacy scores (or math scores) with the treatment programs. As shown in these alternative results our main findings do not change.

While we would expect our literacy text-based program to mostly affect the literacy attainment of children, we also looked at potential "spillover" or "crowding out" effects of our texting program on children's math development. Previous research has shown that literacy skills support the acquisition of numeracy skills (Purpura, Hume, Sims & Lonigan, 2011). Moreover, parents are unlikely to reduce math activities at home in response to the program, because few parents focus on math learning with their young children even in the absence of parenting programs. Most math learning occurs at school. If children come to preschool with more early literacy skills, they may be able to benefit more from opportunities to learn math there. Some math activities, such as counting, are also close to the literacy activities that the text-messaging program encouraged. Thus, it is reasonable to hypothesize that the texting program could positively affect math achievement, though the alternative is also possible, that the texting program crowds out time spent on children's math development.

Table 6 provides the results for the math assessment. Panel A shows the results for the overall math score. For math, the **Weekend** program is significantly more effective than the original **Weekday** program. Children whose parents received text messages on Friday, Saturday, and Sunday scored, on average, 0.032 SDs higher on the math assessment than children whose parents received texts during the week. Panels B-H of Table 6 presents results for the math components. The results provide evidence that rote counting and shape naming drive the overall math effects with children whose parents received the **Weekend** program, surpassing those who received the **Weekday** program by 0.069 SDs and 0.102 SDs. In other words, children had an increase of 2 percent in rote counting (relative to the control mean of 1.70), and also an increase of 2 percent in shape naming (relative to the control mean of 4.45). The **Midweek** program also had a significant positive effect on shape naming (0.060 SDs). However, we do find a negative **Midweek** program effect on children's operations scores (−0.062 SDs) relative to children whose parents received the **Weekday** program.

We conducted a heterogeneity analysis by parent's educational attainment and child's pre-intervention literacy score for the math analysis.[20] Again, in this sub-analysis we find some suggestive findings that parallel the results in the literacy sub-analysis. In particular, children who were initially in the lower half of the literacy skill distribution showed a differential gain of 0.053 SDs in their overall math score if their parents received texts on weekends, while children in the upper half of the distribution showed no such differential improvements. Moreover, these lower-skilled children whose parents received the **Weekend** program had math gains of 0.202 SDs in shape naming. The positive **Midweek** program effect on shape naming (0.060 SDs), is again largely driven by children in the lower half of the math baseline distribution (0.143 SDs). Finally, the **Midweek** and the **Weekend** programs both appear to reduce math scores on operations, which is considered a higher math skill, for children who started the school year in the upper half of the literacy skill baseline distribution by 0.096 and 0.110 SDs, respectively, compared to the **Weekday** program.

The math results might, at first, seem surprising because they are meaningfully large. However, these results have plausible explanations. First, math may be easier to affect than early literacy – we often see larger effects of interventions on math than on English Language Arts (Hanushek & Rivkin, 2010; Loeb & Figlio, 2011). These differences could come from differential measurement of math vs. literacy or because many young students get far more supports in literacy than in math so that any effect on math may look larger relative to the distribution of math learning. Moreover, the results for this study show that the positive math effects are concentrated in lower math skills (i.e., rote counting) which are closer to literacy skills than are the higher order math skills (i.e., operations). The texts provided on Table 1, show that the texting program could have encouraged parents to cover some of

these math concepts with their children.

Taken together, our analysis of both the literary and math scores suggests that sending text messages to parents during the weekend is more beneficial to the skill development of their children than sending text messages during the week, especially for initially lower-performing children in building both base-line literacy and math skills, such as rapid letter naming, rote counting, and shape naming. However, our results also suggest that weekend texts may not be as effective as weekday texts for children in the upper half of the baseline skill distribution and for some higher-order skills, such as phonological awareness and operations.

8. Concluding remarks

This study addresses the question of when light-touch parenting supports work best. We answer this question using a text-message-based parenting program for parents of preschool children that has demonstrated positive effects on child development in prior research (York et al., 2019). We compare three identical programs sent on different days of the week to parents in three randomly assigned treatment groups. The goal is to assess whether the messages provide better supports on more challenging days, such as weekdays, or on days that parents are more likely to be free, such as weekends.

Our results show that sending the program on weekends increases children's literacy and math development more, on average, than sending the program during the week. While we do not have direct evidence for why the **Weekend** program is more effective, the benefits could be driven by most parents having more time during the weekends to interact with their children. If many parents do not have time with their children during weekdays, for example, then supports for how to interact may not be as helpful. Some parents may remember the content of the texts and use them on the weekend, but this delay may reduce the benefits of the **Weekday** program.

Our results also show that parents differ in optimal timing of parent supports, and, thus programs may want to consider flexibility in their timing. In particular, the relative positive effects of the **Weekend** program are driven by differential effects for parents of initially lower-performing students and for parents with lesser educational attainment. In fact, we find some weaker evidence that the **Weekend** program may be less effective than the **Weekday** program for parents of initially higher-performing students and for parents with greater educational attainment.

This heterogeneity in program effects may arise because some parents have more free time on weekends while others have more time during the weekday. However, given the characteristics of the students whose parents benefit from the different programs, we hypothesize that mechanism is more complicated. In particular, on some days parents might have greater needs for suggestions from this program of what to do with their children because they are facing some barriers; yet, on other days, these same barriers may be so great that the suggestions are not enough to affect parent-child interactions.

We were not able to categorize parents by their work hours, competing demands, and other factors that limit parents' ability to engage with their children (e.g., single parenthood). Thus, we cannot directly test whether these factors drive the heterogeneity in effects. However, parents' educational attainment and students' initial assessment score are likely to be correlated with these demands. Parents with less educational attainment, on average, have lower wages, more financial stresses, and more likely to work evenings (Bianchi, 2011; Galinsky et al., 2011; Mani et al., 2013; Shah et al., 2012; Williams & Boushey, 2010). While low-scoring students come from families across the range of demand, low scoring students are more likely than other students to live in families with financial and other constraints. As a result, variation by education and score may be proxying for variation in demands.

A possible explanation for the positive effects of the weekend

---

[20] See appendix Table A6 for full set of results.

program for more constrained parents is that these parents, on average, do not have the time or focus during the weekdays to make use of the text messages, while they are less constrained on the weekends and benefit more from the text messages. These parents may need substantially more intensive supports for weekdays to be effective timing for parenting interventions. On the other hand, less constrained, more educated parents, may benefit from a light-touch program on weekdays to make their interactions with their children more educational during those times. On the weekends, when they have less distractions, they may not need these supports because they are able to interact effectively without them. Overall, parents may benefit more from supports first when they have the time and second when other distractions are not so high that they are insurmountable but are high enough that programs aimed to reduce the barriers have leverage.

These proposed mechanisms are not the only feasible explanations for the observed effects and heterogeneity. For instance, some parent's marginal utility of engaging with their children could be higher during weekends than during the week. Hence, the text could reduce cognitive load or remind parents similarly independent of the day, but still have a stronger effect on the weekends because parents gain more value from interacting with their children then. Similarly, the effects of the text message on cognitive load could be the same across the week, but the nudging could be more effective on weekends. For example, parents of low-achieving students may be reminded that their children need additional help on weekends and, conversely, parents of high-achieving may dismiss the text messages over the weekend in favor of more fun and less educational activities because they believe their children are doing well in school. However, generally, the effects of the program do not seem to be driven by the reminders alone. Doss et al. (2019) shows that content tailored to students' skills increases the effectiveness of the texts and, as such, that nudges alone cannot explain the effects of the texting program. While our experimental design does not allow us to examine the relative contributions of reminders and reducing cognitive load directly, we expect this finding to be true in our context, too. Finally, texting fatigue (i.e., parents stop paying attention) (Cortes et al., 2021) could be more pronounced during the week than during the weekend as parents have fewer competing demands. If this was the main mechanism driving the results, the **Midweek** program, in which texts arrive at a faster pace during the week, should increase the fatigue and hence show the worst results. Yet, it shows no discernable difference from the **Weekday** program. As a result, while these alternative explanations are feasible, they do not seem as likely as differential behavioral barriers.

Perhaps it is not surprising either that weekends are, on average, more effective than weekdays or that parents vary in the optimal timing for their program. Most parents have more time with their children on weekends. However, as time-use patterns depend on work and non-work demands, the optimal timing of program delivery differs across parents. The findings highlight the benefits of differentiating programs, even beyond individualizing the activities suggested by the texts to match the development needs of students (Doss et al., 2019). An easy first step would be to allow parents to choose when they receive the program. Such an addition to the program would add very little cost and may lead to meaningful benefits, as long as parents choose the timing that best matches their needs. The heterogeneity in these results may also indicate that other forms of heterogeneity are salient, as well. For example, parents may vary in how much they benefit from suggested activities that are more or less difficult to implement, or require more or less prior preparation; and students may vary in how much they benefit from activities that reference different content areas of interests – such as animals, trucks or household chores. One of the promises of technology-driven interventions is their ability to adjust to needs and preferences, and this study is just the first step in identifying dimension that are salient for these adjustments.

Data availability

The data that has been used is confidential.

References

Abraham, C., & Michie, S. (2008). A taxonomy of behavior change techniques used in interventions. *Health Psychology, 27*(3), 379–387.

Aos, S., Lieb, R., Mayfield, J., Miller, M., & Pennucci, A. (2004). *Benefits and costs of prevention and early intervention programs for youth*. Olympia, WA: Washington State Institute for Public Policy.

Baumeister, R. F., Vohs, K. D., & Tice, D. M. (2007). The strength model of self-control. *Current Directions in Psychological Science, 16*(6), 351–355.

Bettinger, E. P., Long, B. T., Oreopoulos, P., & Sanbonmatsu, L. (2012). The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment. *Quarterly Journal of Economics, 127*(3), 1205–1242.

Bianchi, S. M. (2000). Maternal employment and time with children: Dramatic change or surprising continuity? *Demography, 37*(4), 401–414.

Bianchi, S. M. (2011). Changing families, changing workplaces. the future of children. *The Future of children / Center for the Future of Children, the David and Lucile Packard Foundation, 21*(2), 15–36.

Castleman, B. L., & Page, L. C. (2015). Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? *Journal of Economic Behavior & Organization, 115*, 144–160.

Castleman, B. L., & Page, L. C. (2016). Freshman Year Financial Aid Nudges. *The Journal of Human Resources, 51*(2), 389–415.

Cohen, G. L., & Sherman, D. K. (2014). The psychology of change: Self-affirmation and social psychological intervention. *Annual Review of Psychology, 65*, 333–371.

Cortes, K. E., Fricke, H., Loeb, S., Song, D. S., & York, B. N. (2021). Too little or too much? Actionable advice in an early-childhood text messaging experiment. *Education Finance and Policy, 16*(2), 209–232.

Del Boca, D., Flinn, C., & Wiswall, M. (2014). Household Choices and Child Development. *The Review of Economic Studies, 81*(1), 137–185.

Del Bono, E., Francesconi, M., Kelly, Y., & Sacker, A. (2016). Early maternal time investment and early child outcomes. *The Economic Journal, 126*(596), F96–F135.

DellaVigna, S. (2009). Psychology and economics: Evidence from the field. *Journal of Economic Literature, 47*(2), 315–372.

Doss, C., Fahle, E. M., Loeb, S., & York, B. N. (2019). More Than Just a Nudge: Supporting Kindergarten Parents with Differentiated and Personalized Text Messages. *The Journal of Human Resources, 54*, 567–603.

Duncan, G. J., Ludwig, J., & Magnuson, K. A. (2010). Child development. In Phillip B. Levine, & David J. Zimmerman (Eds.), *Targeting investments in children: Fighting poverty when resources are limited* (pp. 27–58). Chicago, IL: The University of Chicago Press.

Fox, L., Han, W.-. J., Ruhm, C., & Waldfogel, J. (2013). Time for children: Trends in the employment patterns of parents, 1967–2009. *Demography, 50*(1), 25–49.

Galinsky, E., Sakai, K., & Wigton, T. (2011). Workplace flexibility: From research to action. *The Future of Children, 21*(2), 141–161.

Gardner, Frances, Burton, Jennifer, & Klimes, Ivana (2006). Randomized controlled trial of par-enting intervention in the voluntary sector for reducing child conduct problems: Outcomes and mechanisms of change. *Journal of Child Psychology and Psychiatry, 47*(11), 1123–1132.

Greenman, E., Bodovski, K., & Reed, K. (2011). Neighborhood characteristics, parental practices and children's math achievement in elementary school. *Social Science Research, 40*(5), 1434–1444.

Groot, B., Sanders, M., Rogers, T., & Bloomenthal, E. (2017). I get by with a little help from my friends: Two field experiments on social support and attendance in further education colleges in the UK. *The Behavioral Insights Team*. Retrieved from http://38r8om2xjhhl25mw24492dir.wpengine.netdna-cdn.com/wp-content/uploads/2017/04/Study-Supporter-WP_April-2017.pdf.

Guryan, J., Hurst, E., & Kearney, M. (2008). Parental education and parental time with children. *Journal of Economic Perspectives, 22*(3), 23–46.

Hall, C. C., Zhao, J., & Shafir, E. (2014). Self-affirmation among the poor: Cognitive and behavioral implications. *Psychological Science, 25*(2), 619–625.

Hanushek, Eric A., & Rivkin, Steven G. (2010). Generalizations about Using Value-Added Measures of Teacher Quality. *The American Economic Review, 100*(2), 267–271.

Hsin, A., & Felfe, C. (2014). When does time matter? Maternal employment, children's time with parents, and child development. *Demography, 51*(5), 1867–1894.

Karlan, D., McConnell, M., Mullainathan, S., & Zinman, J. (2016). Getting to the Top of Mind: How Reminders Increase Saving. *Management Science, 62*(12), 3393–3672.

Karoly, L. A., Greenwood, P. W., Everingham, S. S., Houbé, J., Rydell, C. P., & Chiesa, J. (1998). *Investing in our children: What we know and don't know about the costs and benefits of early childhood interventions*. Santa Monica, CA: RAND.

Kraft, M. A., & Rogers, T. (2015). The underutilized potential of teacher-to-parent communication: Evidence from a field experiment. *Economics of Education Review, 47* (2015), 49–63.

Loeb, S., & Figlio, D. (2011). School accountability. In E. A. Hanushek, S. Machin, & L. Woessmann (Eds.), Handbook of the economics of education, Vol. 3 (383–423). San Diego, CA: North Holland.

Lonigan, Christopher J., & Shanahan, Timothy (2009). *Developing early literacy: Report of the national early literacy panel*. Washington, DC: National Institute for Literacy.

Mani, A., Mullainathan, S., Shafir, E., & Zhao, J. (2013). Poverty Impedes Cognitive Function. *Science (New York, N.Y.), 341*(6149), 976–980.

Mayer, S. E., Kalil, A., Oreopoulos, P., & Gallegos, S. (2019). Using behavioral insights to increase parental engagement: The parents and children together intervention. *The Journal of Human Resources, 54, 900-925*.

Mischel, W. (1974). Processes in Delay of Gratification. *Advances in Experimental Social Psychology, 7*, 249–292.

Page, L. C., Castleman, B., & Meyer, K. (2018). Customized Nudging to Improve FAFSA Completion and Income Verification. *Educational Evaluation and Policy, 42*(1), 3–21.

Patterson, Gerald R., Reid, John B., & Dishion, Thomas J. (1992). *Antisocial boys: A social interactional approach*. Eugene, OR: Castalia Publishing Co.

Purpura, D. J., Hume, L. E., Sims, D. M., & Lonigan, C. J. (2011). Early literacy and early numeracy: The value of including early literacy skills in the prediction of numeracy development. *Journal of experimental child psychology, 110*(4), 647–658. https://doi.org/10.1016/j.jecp.2011.07.004

Reese, Elaine, Sparks, Alison, & Leyva, Diana (2010). A review of parent interventions for preschool children's language and emergent literacy. *Journal of Early Childhood Literacy, 10*(1), 97–117.

Robinson, C. D., Lee, M. G., Dearing, E., & Rogers, T. (2018). Reducing student absenteeism in the early grades by targeting parental beliefs. *American Educational Research Journal*. Retrieved from http://journals.sagepub.com/toc/aer/0/0.

Rogers, T., & Feller, A. (2018). Reducing student absences at scale by targeting parents' misbeliefs. *Nature: Human Behavior, 2*(2018), 335–342.

Sanders, Matthew R., Markie-Dadds, Carol, Tully, Lucy A., & Bor, William (2000). The triple P-positive parenting program: A comparison of enhanced, standard, and self-directed behavioral family intervention for parents of children with early onset conduct problems. *Journal of Consulting and Clinical Psychology, 68*(4), 624–640.

Sayer, L. C., Gauthier, A. H., & Furstenberg, F. F., J.r. (2004). Educational differences in parents' time with children: Cross-national variations. *Journal of Marriage and Family, 66*(5), 1152–1169.

Shah, A., Mullainathan, S., & Shafir, E. (2012). The Consequences of Having Too Little. *Science (New York, N.Y.), 338*(6107), 682–685.

Shiv, B., & Fedorikhin, A. (1999). Heart and Mind in Conflict: The Interplay of Affect and Cognition in Consumer Decision Making. *Journal of Consumer Research, 26*(3), 278–292.

Smythe-Leistico, K., & Page, L. C. (2018). Connect-text: Leveraging text-message communication to mitigate chronic absenteeism and improve parental engagement in the earliest years of schooling. *Journal of Education for Students Placed at Risk, 23* (1–2), 139–152.

Stewart, J. (2010). The timing of maternal work and time with children. *Industrial and Labor Relations Review, 64*(1), 181–200.

Sweeney, A. M., & Moyer, A. (2014). Self-affirmation and responses to health messages: A meta-analysis on intentions and behavior. *Health Psychology, 34*(2), 149–159.

Thaler, R. H., & Sunstein, C. R. (2008). *Nudge: Improving decisions about health, wealth, and happiness*. New Haven, CT: Yale University Press.

Thomsen, M. K. (2015). Parental time investments in children: Evidence from Denmark. *Acta Sociologica, 58*(3), 249–263.

Van Zeijl, Jantien, Mesman, Judi, IJzendoorn, Marinus H. Van, Bakermans-Kranenburg, Marian J., Juffer, Femmie, Stolk, Mirjam N., et al. (2006). Attachment-based intervention for enhancing sensitive discipline in mothers of 1- to 3-year-old children at risk for externalizing behavior problems: A randomized controlled trial. *Journal of Consulting and Clinical Psychology, 74*(6), 994–1005.

Webster-Stratton, Carolyn (1992). *The incredible years: Basic program manual*. Seattle, WA: The Incredible Years.

Williams, J., & Boushey, H. (2010). The three faces of work-family conflict: The poor, the professionals, and the missing middle. *Working Paper*. Retrieved from https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2126314.

York, B. N., Loeb, S., & Doss, C. (2019). One step at a time: The effects of an early literacy text messaging program for parents of preschoolers. *The Journal of Human Resources, 54*, 537–566.