Using Behavioral Insights to Increase Parental Engagement: The Parents and Children Together Intervention

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Parental engagement plays a key role in children's future success. We implemented a behaviorally informed field experiment designed to increase the time parents spend using a digital library on an electronic tablet to read to their children. Behavioral tools—reminders, goal-setting, and social rewards—more than doubled the amount of time parents spent reading using the electronic application (one standard deviation effect size) after the six-week intervention. The largest gains were for the most present-oriented parents. Our findings suggest substantial promise for the application of behavioral tools to parenting activities that promote investment in children's human capital.

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I. Introduction

A large research literature documents the substantial differences in parental engagement between advantaged and disadvantaged families and the importance of parental engagement to children's future economic outcomes. However, research also documents that existing programs intended to increase engagement among disadvantaged parents have at best met with little success, sometimes at a high cost per child. This paper reports results from a randomized field experiment designed to increase one important form of parental engagement, namely the time that parents spend reading to their children.

Below we describe several possible explanations for why disadvantaged parents are less likely than advantaged parents to engage their children in educational activities. Prior research suggests that cognitive biases and in particular discounting the future are promising explanations. Consequently, our experiment is based on principles from behavioral science that have been successful in addressing cognitive biases in financial savings (Meier and Sprenger 2010), smoking cessation (Rodgers et al. 2005), children's literacy scores (York and Loeb 2014), college attendance (Castleman and Page 2014), and weight loss (Chabris et al. 2008). We also administered a time preference assessment to examine directly its potential importance.

Notably, almost all papers on the topic[i] (Kalb and van Ours 2014; York and Loeb 2014) rely on self-reported measures of parental reading, which are subject to important issues of measurement error. We use an electronic reading application that automatically audio and video records parents as they read to their child. This objective measure of how much time parents spend reading to their children, combined with survey data from the parents, allows us to assess the potential importance of several other possible explanations for differences in the amount of time that parents read to their children. Although we cannot definitively rule out that reading as

measured on the tablet is not substituting for nontablet reading, survey data about parental reading habits in our target population suggests that there is not much scope for such substitution.

Our main results indicate that the treatment increased the number of minutes that parents read to their children using an electronic application by one standard deviation, which equals twice as much reading time compared to the control group mean. The increase was greater for parents with a high discount rate for the future, as measured by the time preference task that we administered. Our findings suggest substantial promise for the application of behavioral tools to parenting activities that promote investments in children's human capital. However, our findings also leave much room for future research. In particular, future research should more rigorously address the persistence of changes in parental behavior beyond the intervention period and the effect of changes on parental behavior on children's academic outcomes. We see our evidence as part of a nascent body of literature that suggests behaviorally informed interventions can be a cost effective way to help parents engage their children more often and more productively.

The remainder of this paper proceeds as follows. Section II provides background on differences between advantaged and disadvantaged parents in both children's skills and parental engagement. Section III provides details about the experiment. Section IV describes the results, while Section V concludes the article.

II. Differences Between Advantaged and Disadvantaged Children

A. *The Influence of Parenting on Children's Skills*

Substantial differences in the skills of economically advantaged and disadvantaged children emerge well before the start of formal schooling and remain fairly constant throughout the school years (Cunha et al 2006; Heckman 2008). For example, preschool-age

children from families in the poorest income quintile score on average at the 34th percentile in a test of literacy, compared to children in the richest quintile who score at the 69th percentile (Waldfogel and Washbrook 2011). Conventional measures of school quality (such as teacher/pupil ratios and teacher salaries) have small effects on creating or eliminating gaps after the first few years of schooling (Carneiro and Heckman 2003; Cunha and Heckman 2007). The importance of family influences relative to schooling is perhaps not surprising, considering children in the United States will spend only about 13–15 percent of their waking hours in school between birth and age 18.[ii] Finally, school outcomes reflect many child characteristics such as orientation toward the future, sense of personal efficacy, work ethic, and other characteristics sometimes referred to as "noncognitive skills" (see, for example, Cunha and Heckman 2007; Carneiro and Heckman 2003; Heckman and Kautz 2012). These skills are largely shaped by family influences and not by schools.

Across disciplines, dozens of studies have demonstrated differences in the ways advantaged and disadvantaged parents engage their children and these differences are likely to help explain differences in children's cognitive and noncognitive outcomes. Among other things, advantaged parents spend more time on educational activities with their children (Guryan, Hurst, and Kearney 2008; Kalil, Ryan, and Corey 2012) and produce more cognitively stimulating home learning environments (Bradley et al. 2001; Harris, Terrel, and Allen 1999). In addition, the gap in the amount of time that advantaged and disadvantaged parents spend with their children overall and in educationally relevant activities has widened over the last twenty years because the rate at which highly educated mothers have increased time with their children has outpaced the rate of less educated mothers (Altintas 2012; Ramey and Ramey 2010; Hurst 2010).

Parent engagement appears to explain a large share of the gap by parental advantage in children's cognitive and noncognitive skills (Waldfogel and Washbrook 2011; Cunha et al. 2006; Heckman and Masterov 2004). Other research suggests that spending time with a child has a direct and causal effect on children's cognitive test scores (Villena-Rodán and Ríos-Aguilar 2012) and that the amount of time that parents spend in educational activities with their children is the most productive input for cognitive skill development (Fiorini and Keane 2014).

The experiment reported in this paper focuses on one particular type of parental engagement, namely reading to one's children. Observational research supports the widely held belief that parental reading to young children is associated with not only the child's greater literacy skill but also numeracy and social skills (see, for example, Kloosterman et al. 2011; Mol and Bus 2011; Hale et al. 2011; Raikes et al. 2006).[iii] The few attempts to estimate a causal relationship between parental reading and children's cognitive skills also conclude that parental reading time increases cognitive test scores. Price and Kalil (2017), using a birth order instrument for parental reading time, concluded that one additional day per month of mother-child reading (equivalent to about a .10 SD increase in reading time) would increase children's reading test scores in the preschool years by on average .08 SD. Kalb and van Ours (2014), using a similar instrument, found that five-year-old children who are read to 3–5 days a week score about 0.5 SD higher than those who were read to fewer than three days per week. They indicate that the effects remain at least until children are 10–11 years old.

According to the National Center for Educational Statistics (Aud et al. 2012), 92 percent of parents with a college degree reported that someone in the household read to the child at least three times in the last week, whereas only about 75 percent of parents with less than a high school education reported that someone in the household read to the child this often.

Similarly, whereas 11 percent of nonpoor parents of a three-year-old child reported that no one in the household read to the child at least three times in the last week, 26 percent of poor parents reported that no one in the household read to the child at least three times in the last week.[iv]

Various parenting interventions have tried to increase the amount of time that disadvantaged parents spend in educationally enriching activities, especially reading with children. However, in the United States most of these programs have had modest success at best.[v] For example, many practitioners and policy makers believe that home visiting programs are the best way to improve parental engagement in disadvantaged families. Yet, evaluations of these programs find little improvement in parental engagement (Olds, Henderson, and Kitzman 1994; Wagner, Spiker, and Linn 2002).

Other parent engagement programs have had modest success in increasing either parental engagement or child outcomes. Fryer, Levitt, and List (2015) provided financial incentives of up to $7,500 per year to parents to engage in behaviors designed to increase early childhood cognitive and executive function skills. Parents were rewarded for attendance at parenting meetings, completing homework assignments with their children, and for their child's demonstration of mastery on interim assessments. The incentive had no effect on cognitive scores, although it did improve noncognitive skills by 0.20 SD.

B. *Obstacles to Parents Reading to Their Children*

Several theories try to explain why disadvantaged parents are less likely to engage their children. However, we have remarkably little empirical evidence to support these theories. An important contribution of this paper is to try to understand the mechanisms that influence

parental engagement in one important behavior, namely how much time parents spend reading to their young children.

One common explanation for why disadvantaged parents engage less with their children is that they lack either information about the importance of parental engagement or about how to engage their children. According to this explanation, the differences in information lead to differences in expected returns to engagement and hence to differences in actual engagement. But when it comes to reading to children, the only nationally representative survey of parents in the United States that we could find indicates that almost all parents (86 percent) say that reading to their children is "extremely important," and almost all of the remaining parents report that it is "important" (Scholastic 2014).[vi] It would be hard to argue that the difference in reading time between advantaged and disadvantaged parents is due to differences in the importance that they attach to reading.

It is possible that disadvantaged parents engage their children less because they lack information about *how* to invest in educational activities. With respect to reading, parents know that reading is important but they might not know how often or how much they should read to their children. York and Loeb (2014) provide some evidence for this explanation. They use a text-messaging program for parents of preschoolers that provides tips on specific actions parents might take to increase their child's literacy skills. Their eight-month intervention of three texts per week increased students' scores on an assessment of early literacy by 0.11 SD (with a standard error of 0.054). York and Loeb were not able to directly measure the mechanism leading to the increase in test scores. Parents in the experimental group reported more engagement in the literacy activities that corresponded closely with the content of the text messages. However, the increase was estimated imprecisely and the self-reports of engagement may have been primed by

the experiment itself rather than reflecting actual behavioral changes. In the experiment we report on in this paper, we focus specifically on measuring parental engagement but we do not measure test scores.

It is possible that disadvantaged parents read less to their children because they have fewer skills, which makes the time they spend with their children less productive. In the United States, about half of adults who lack both a GED and a high school diploma read English at below a basic level of proficiency on the National Assessment of Adult Literacy.[vii] This means that they are only able to locate easily identifiable information in short, commonplace prose texts (Kutner et al. 2007). About a third of parents of children in the preschool program Head Start have less than a high school education. If a third of those have below basic reading levels, then about 10–11 percent of the parents of Head Start children may have literacy rates low enough to make reading some preschool books difficult. However, a large percentage of those with below basic English literacy skills are adults whose primary language is not English. For example, 61 percent of adults who primarily spoke Spanish at home had English prose literacy scores at the less than basic level. About a quarter of Head Start parents have a primary language other than English, and some of these parents are proficient and able to read in their native language. Some parents with low literacy levels may be able to easily read books that are written for preschool children and presumably all parents with basic literacy skills or better can comfortably read preschool-level reading material. Consequently, the vast majority of parents have reading skills sufficient to read to their preschool-aged children.

Some parents with basic reading skills may *expect* a lower return from reading to their children. Agee and Crocker (1996) used an instrument for the parents' discount rate for child investments and found that less educated and lower income parents discount their investments at

about twice the rate as more advantaged parents. Cunha, Culhane, and Elo (2013) also found that on average disadvantaged parents underestimate the return to their investment in their children, but they still estimate a substantial positive return. As we show below, our results indicate that parents believe there is a positive return to reading to their children. In any case, a lower expected rate of return could lead to either less or more investment in children, depending on whether parents try to make up for the lower return to time reading by spending more time reading.

Another explanation for the gap in parental engagement is that disadvantaged parents have less time and fewer resources to engage their children. However, evidence from time diary data shows that even when researchers take into account the number and ages of children in the home, whether the parent is married, and how much time the parent works, differences by education in the amount of time that parents spend with their children persist (Guryan, Hurst, and Kearney 2008). NCES data shows that the proportion of parents who report that someone in the household reads to their three-year-old child at least three times a week does not vary by the employment status of the mother, except that the proportion of parents reading to their child was lower when the mother was unemployed (U.S. Department of Education, NCES 2014).

It is well documented that on average low-income households have fewer books and fewer age-appropriate books for their children than high-income households. Many observational studies have found that the number of books in the home predicts literacy outcomes (Fryer and Levitt 2004). But the number of books in a home can reflect not only what parents can afford but also parent's attitudes about reading or their inclination to read to their child. Recent meta-analyses of experimental and quasi-experimental studies suggest that improving access to books in the home generally yields small positive impacts on children's literacy skills (Kim and Quinn 2013; Lindsay 2010; Allington et al. 2010). However, using

ECLS-K data, Bassok et al. (2016) show that the average number of books owned by a child at the beginning of kindergarten was 52.2 for children at the 10th percentile of the income distribution, 86.5 at the 50th percentile, and 130.5 for children at the 90th percentile. Although the number of books is much greater for affluent families, even 50 books seems like a sufficient number for reading regularly to a child. As we describe below, few parents in our sample say that they lack the time to engage with their children and almost none say that they do not read because they have nothing to read.

It appears that most parents know that reading to their children is valuable, and most parents report that they have the time to read and have books available. Thus, we turned to a different explanation for why some parents seldom read to their children, namely psychological factors that may interfere with parents doing what they themselves want to do.

One such psychological factor is the tendency to discount the future. It is well known that future outcomes are underemphasized (or equivalently, overdiscounted) relative to immediate outcomes, making it hard for people to give up things they enjoy today for the (undervalued) future. This results in problems of self-control, which affects decision-making in many domains, such as savings and borrowing (for example, Meier and Sprenger 2010; Eckel, Johnson, and Montmarquette 2005), dieting, exercising, and smoking (e.g., Chabris et al. 2008), or investments in human capital (Sutter et al. 2013; Castillo et al. 2011).

Differences in the discount rate on the future may also be a cause of low income if it results in less education, or if people with a high discount rate prefer jobs with flat wage paths to jobs that promise high wages only after a period of training or education. Some research suggests that differences in discount rates on the future might help account for differences in child-rearing behavior. For example, Pabilonia and Song (2013) found that even after

controlling for parental differences in income, employment, and education, single mothers who are more present-oriented spend significantly less time with their children overall and less time engaged with their young children in educational activities. Their children also have lower scores on reading comprehension tests.

Disadvantaged parents face a host of stressors such as income instability, child care problems, or transportation issues that place cognitive and emotional demands on their attention in the present and leave little energy for thinking about the future (Spears 2011; Mani et al. 2013; Gennetian and Shafir 2015). These distractors may draw attention away from actions for targeting long-term goals. Suboptimal decisions may thus arise when particular options are not salient (Gabaix 2014).

III. The Parents and Children Together Experiment

Research has demonstrated that certain behavioral tools such as commitment devices, reminders, and social rewards can help overcome problems associated with discounting the future and result in decisions that the individual prefers. We designed an experiment to test whether these behavioral tools affect parents' engagement with their children as a means to understand the role of present bias in parent engagement. We called our experiment the Parents and Children Together (PACT) experiment. In this section we describe the experiment, the eligibility requirements, and the recruitment process.

Parents with children enrolled in a subsidized preschool program (for example, Head Start) in Chicago were given an electronic tablet to borrow for the six-week experiment. The tablet had an application preloaded on it that included over 500 children's books in English and Spanish. When the application was opened, an audio and video recording of the parent reading to the child opened in a small window on the tablet so parents could view themselves being

recorded. Within each preschool, parents were randomized to either a treatment group or control group.

The treatment included three behavioral tools (a commitment device, reminders, and a social incentive) plus information on the importance of parents reading to their children. Previous research has suggested that these are useful for focusing attention on a particular behavior and overcoming problems associated with discounting the future. We included information about the importance of reading because most previous attempts to increase parental reading are based on the hypothesis that parents lack such information.

A *commitment* is a pledge to carry out a specific behavior or take actions necessary to achieve a specific goal (for recent relevant studies, see Ashraf 2013; Bryan, Karlan, and Nelson 2010). People are strongly motivated to be consistent with their own past actions, especially actions taken publicly. When a commitment formalizes a person's pledge to do something or achieve an objective, it increases the chance that he or she will do it by increasing the psychological cost of not doing the behavior to which the individual committed.

Reminders can also overcome problems of self-control by getting people to focus attention on the task. A reminder can change time allocation today by drawing attention to the relationship between future outcomes and current choices. Text messages are the most common way to communicate reminders and are now a common feature of many interventions (see, for example, Richburg-Hayes et al. 2014; Castleman and Page 2014; and Bergman 2015.)

Social incentives are designed to shift preferences by increasing the utility of specific behavior. While a large research literature documents the importance of monetary incentives for changing behavior, a growing body of research also documents the importance of nonmonetary or social incentives for changing behavior. Nonmonetary incentives may be at least as effective

as monetary incentives at changing a behavior, such as parents engaging with their children, which is considered prosocial or normatively desirable (Gneezy and Rustichini 2000; Bénabou and Tirole 2003, 2006; Besley and Ghatak 2005; Dixit 2002). Social incentives work by both increasing the utility of the rewarded behavior and, like commitment devices, reducing procrastination and impatience by focusing attention on the rewarded behavior.

For the experimental treatment we use what Bryan, Karlan, and Nelson (2010) call a "soft commitment device." At the beginning of each week of the PACT experiment, a research assistant asked each parent in the treatment group to set a goal for how much time he or she would spend using the reading app during the next week. The research assistant recorded this number in the "virtual goal keeper" (VGK), which is a web site that we developed for the project. At the end of the week, parents could see a bar chart on the VGK or in a text message that displayed the parent's goal and how much the parent actually read. This provides what has been called "narrow goal bracketing" (Hsiaw 2017). Narrow goal bracketing provides feedback on short-term goals rather than on "big picture" or long-term goals. Hsiaw's theoretical model suggests that narrow bracketing is more effective at overcoming cognitive biases that lead to problems of self-control. Parents were then asked to set a goal for the next week either by sending a text message or telling a research assistant the new goal. This was repeated for every week during the experiment. Research assistants collected the tablets from both the control and treatment group members during one morning a week and we downloaded the data on reading time. They then returned the tablets that same afternoon. During this time, research assistants did not discuss anything relevant to reading or to the parent's goals or progress.

Parents in the PACT treatment group received a text message reminder every weekday. Text messages mainly reminded parents to work toward their reading goal. Messages also

sometimes reminded parents about the importance of reading to their child. Some examples of the text messages include the following: “Are you working toward your goal this week?”; “Remember that reading with your child is very important to your child's future”; “What’s your child’s favorite book? Find some books your child loves and have fun reading them tonight!”

As a social reward, if a parent met the reading goal, that parent received a congratulatory text message; a cartoon bear did a celebratory dance when the parent viewed the reading minutes and goal on the VGK. In addition, each week a text message went to all treated parents in a center saying, “This week in your group, the parent with [tablet number] did the most reading with their child! Congratulations to that parent!” The parent with that tablet number would therefore know that he or she had done the most reading but other parents would not know who that parent was.

Information on the importance of reading was available in thirteen videos and two PDF documents about parenting, language development, and the importance of parents’ reading to their children. These were preloaded on the tablet and the parents had access to theses materials for the entire six-week experiment.

Parents in the control group were also given a tablet with the digital library preloaded on it. Research assistants also collected the tablets from the control group members during one morning a week so that we could download the data on reading time. They then returned the tablets that same afternoon. Parents in the control group received “placebo” information in the form of fourteen videos and one PDF document about nutrition, health, and dental hygiene. For both treatment and control group parents, we were able to identify whether a parent ever opened one of the information documents. All documents were available in Spanish and English.

Parents in both the treatment and control group were asked to designate a "focal" child in the event they had more than one child in preschool; they were instructed to use the reading app with this child. Parents were told that other children could be present when they used the app but that we were most interested in their use of the app with the focal child.

We identified the number of minutes that parents read to their children by using the time stamps provided in the reading application. We implemented the experiment in two six-week rounds to minimize the number of electronic tablets that we needed to purchase. The first round was in the autumn semester of the 2014–15 preschool program and the second was in the winter semester. As we describe below, we implemented two short follow-ups with participants to test whether the effect of PACT persisted after the experiment ended.

We collected data from both treatment and control group parents in three surveys. Survey 1 was administered when parents first received the electronic tablets. Parents were asked to report on general characteristics of the child, the household, and themselves. Survey 2 was administered in the middle of the experiment period and consisted of a time preference assessment, which we discuss below. Survey 3 was administered at the end of the six-week experiment. It asked questions about parenting beliefs and practices.

Eligibility. Parents whose primary language was either English or Spanish, who had a child enrolled in one of eight subsidized preschool programs in Chicago, and who were willing to sign a consent form and a pledge to borrow, safeguard, and return an electronic tablet at the end of the program were eligible for the experiment.[viii] The preschool programs were located throughout the city[ix] and have a racially and ethnically diverse population.

Recruitment and Randomization. We recruited parents when they dropped off or picked up their children at the centers. We spent between six and ten hours in each center inviting parents to

participate. Parents received different tablets depending on whether they were in the treatment or control group. Every other parent who we recruited received a "treatment" group tablet; the remaining parents received a "control" group tablet. The tablet number indicated the group to which they belonged and acted as the subject identifier.

In total, the eight preschool programs where we recruited parents served about 965 children at the time we began recruiting. Based on conversations with the staff of the preschools, we estimate that about 15 percent of the 965 children were not eligible due to the language condition. We estimate that an additional 15 percent were siblings of another child in the center and only one sibling could be a focal child in PACT. Therefore, across the eight centers, about 580 children were eligible to participate in PACT, although we were not able to contact all of these parents to invite them to participate in PACT during the time that we were recruiting.[x]

IV. Results

A. *Sample and Randomization Balance*

A total of 169 parents participated in the PACT experiment. About half were randomized to the treatment (N=84) and half to the control (N=85) group. Of the 169 participants, 160 took Survey 1 (80 treated, 80 control). Table 1 uses this survey information to describe our sample. Most of the participating parents were mothers. The average age of the parents was 31 years, and the average age of the focal children was 3.8 years. Overall, our sample is similar in characteristics to a national sample of parents of children in Head Start programs collected in the 2010 (the latest year for which data is available) Head Start Family and Child Experiences Survey (FACES) survey. In FACES, 31.8% of parents had less than a high school education, compared to 27.5% in PACT; in FACES, 33% of parents were African American, compared to 32.1% in PACT; in FACES, the mean income was $22,000, compared to

$21,000 in PACT; in FACES, 14.1% of parents reported that their child had a disability, compared to 12.0% in PACT. The only major difference between our sample and the FACES sample is that in FACES, 36% of parents are Latino/Hispanic; in PACT, 65% of parents are Hispanic/Latino. This difference is not surprising because of the higher Hispanic/Latino population of Chicago as compared with the United States as a whole, and the sample was selected to include only parents who spoke English or Spanish.

Table 1 also shows the main characteristics of our sample by treatment status. No differences between the treatment and control group is statistically significant at the 5% level. Three differences were significant at the p=0.10 level, which is about what we would expect by chance.[xi] The focal child from the treatment group was more likely to have been born prematurely (p=.07). Treatment group parents were more likely to have a GED (p=.10), whereas control group parents were more likely to have some college (p=.10). As we describe below, conditioning on the characteristics in Table 1 makes virtually no difference to the results.

B. *Effect of PACT*

Our main outcome of interest is the time parents spend reading to their children using the digital library on the tablet.[xii] We also have information on related outcomes including the number of stories read by the parents, the average time spent reading each story, and whether parents read the same or different stories. To estimate the effect of PACT on these outcomes we use the following equation:

$$(1) \qquad Y = \beta_0 + \beta_1 T + \varepsilon$$

where Y is an outcome variable, T is an indicator for random assignment to the treatment group, and ε is an idiosyncratic error term. Our parameter of interest is β_1, which equals the average

difference for parents randomized to the treated group compared to those randomized to the control group. Below, we add relevant covariates to this model to test the robustness of the results to their inclusion.

Table 2 shows results for the estimation of equation (1) on three measures of parents' reading using the digital library. Column 1 shows the number of minutes parents read to their children as the outcome. On average, the treatment group read 88.4 minutes more than the control group over the six weeks of the experiment, more than doubling the amount of time that the control group spent reading to the child using the digital library (an increase from an average of 63.3 to 151.7 minutes). This represents a one standard deviation difference between the treatment and the control group.

Column 2 shows that the average number of books read during the six-week period was 14.8 for the control group and 14.8+16.7 =31.4 for the treatment group. In other words, parents in the treatment group read an average of almost five books per week using the digital library, compared to the control group who averaged only two or three books per week using the digital library.[xiii]

The last column in Table 2 shows that the treatment also increased the fraction of parents who ever used the digital library to read a story to their child, from 84 percent for the control group to 96 percent for the treatment group.

We conducted short-term follow-ups of parents who participated in PACT. One group of parents kept the tablets for an additional three weeks immediately after the experiment ended; another group got the tablets back for three weeks, but three months after the experiment ended. No behavioral tools were used during either of the three-week follow-ups. We then compared the amount of time that treatment and control group parents spent using the digital library during the

follow-up period. Although the effect sizes remained very large for the follow-ups, these results were inconclusive because of the small sample sizes and selection into the follow-up groups. These results are available in the Online Appendix.

C. *Do Parents Who Discount the Future More Respond to the Behavioral Tools More?*

We examined whether parents in our sample who discount the future more read and respond to the behavioral tools more than parents in our sample who discount the future less. To explore this hypothesis, we implemented a task designed to measure parents' time discount rate.[xiv] We used the Convex Time Budget (CTB)[xv] method first introduced by Andreoni and Sprenger (2012) to estimate both a discount rate for parents and their present bias, which have been shown to best predict outcomes in previous research (Burks et al. 2012). We then examined whether parents with a higher discount rate read less and responded more to our treatment.

Parents were asked to choose between an amount of money that they could receive immediately or a different amount that they could receive later. To induce parents to answer truthfully, all respondents were entered into a lottery and one parent in each of the two rounds in each of the eight preschool centers was randomly selected to actually receive the payout.[xvi] In total, each parent answered fifteen questions with four choices each (two inner solutions and two corner solutions), as in Andreoni, Kuhn, and Sprenger (2013). The first five questions gave parents the choice of receiving payments immediately or three weeks later, the next five questions gave parents the choice of receiving payments immediately or six weeks later, and the last five questions offered the choice of receiving payments in three or six weeks. Within each time horizon, each question presented an increasing price for the earlier payment.[xvii] The set of choices is shown in the Online Appendix. The three- and six-week time horizons were chosen because the experiment lasted six weeks, so the payout could be made during or at the end of the

experiment. Comparing decisions with a delay of twenty-one days to those with a delay of forty-two days allows us to identify the discount rate. High-discount (low-discount) parents are defined as those with an estimated discount rate above (below) the sample median.[xviii] Parents classified as high and low discount-rate parents are balanced by treatment status.

Of the 169 participants who were eligible to participate in the time preference task, 112 participated.[xix] To understand how participation affected our treatment effects, we repeat the estimation of equation (1) shown in Table 2 for the 112 parents who participated in the time preference task. The first column of Table 3 repeats the result from Table 2, and the second column shows the results for the 112 parents who participated in the time preference task. The coefficient on the treatment condition is 82.1 for the parents who participated in the time preference task, compared to 88.3 for the full PACT sample. The difference between these coefficients is not statistically significant.[xx] We also compared parents who did and did not participate in the time preference task on the characteristics shown in Table 1. On these twenty-two characteristics, no difference between the two groups was statistically significant at p=0.05.

Column 3 in Table 3 shows that the treatment effect for high-discount parents was 124.5 minutes. Column 4 shows that the treatment effect for low-discount parents was only 42.3 minutes. This difference of 82 minutes is significant at p=0.095. The constant indicates the amount of reading net of the treatment. It shows that low-discount parents read 97.3 minutes compared to high-discount parents who read only 68.33 minutes net of the treatment. Thus, absent the treatment, low-discount parents read about the same or more than high-discount parents, but the treatment has a greater effect for high-discount than for low-discount parents. Among the treatment group of parents, the high-discount group read 192.9 minutes whereas the low-discount group read 139.5 minutes for a difference of 53.3 minutes (p-value =.174). Among

the control group, the difference between high- and low-discount parents is also not significant. Whereas the effect of the treatment on reading time is significant for high-discount parents, the difference in the reading time for high- and low-discount parents is not significant for either the treatment or the control group. We interpret this to mean that the treatment increased the reading time of high-discount parents to about the level of low-discount parents.

These results do not prove but are consistent with the theory that among a group of disadvantaged parents, those who discount the future more invest less in their children's future. The behavioral tools in PACT increase investment more among these parents compared to parents who discount the future less. Factors correlated with a parent's discount rate may account for the differences in the effect of PACT. For example, the time preference task might be highly correlated with financial constraints if highly constrained parents choose money now rather than later and are less likely to have books in the home. However, in this sample, the discount rate was not correlated (at least p=.05) with parents' income, education, or age.

One caveat is that we measured discount rates after the intervention had begun. We had both practical and theoretical/empirical reasons to do so. The practical reason is that parents were not likely to complete both the baseline survey and the time preference task all at once. We surveyed parents when they were dropping off or picking up their children so they did not have much time available to respond. To maximize the response rate to both the survey and the time preference task, we decided to separate them.

The theoretical/empirical reason supporting our decision of administering the time preference task at a later date was based on the literature that shows that time preferences are generally a stable trait of individuals in the absence of large adverse shocks (Meier and Sprenger 2015; Dean and Sautmann 2016; Chuang and Schecter 2015). Consequently, we did not expect

the intervention to change the discount rate of parents. Instead, we expected the treatment to help parents overcome the procrastination that comes from present bias. The data provides some reassurance that our reasoning was correct. The discount rate was balanced for the treatment and control groups. Of course, it is still possible that the level of discounting for both treatment and control groups is different from what it would have been had we measured at baseline.

While we cannot fully separate the effects of the various components of the experiment, we can provide some evidence on this question. We turn to that evidence now.

D. *Did Parents Learn from Goal-Setting?*

In theory, goal-setting both focuses attention on the behavior that is the object of the goal, thus reducing procrastination or impatience, and imposes a psychological cost if the goal is not met. If not meeting the goal is psychologically costly then we would expect that over the six weeks of the experiment, the difference between the goal and the actual amount of reading time would decrease. This, in fact, happened. Each week, parents were asked, "How many days and how many minutes per day do you commit for your goal this week?" In week one, the average actual reading time for parents in the treatment group was 23 percent of the average goal. By week six, the average actual reading time was 65 percent of the average goal. The convergence mainly occurred because the goals decreased each week (from an average of 93 minutes in week one to an average of 49 minutes in week six). The actual reading time varied from 26 minutes per week to 34 minutes per week, but there was no clear trend in the amount of reading over the experiment period.[xxi] Although parents became more realistic about how much they would actually read in the coming week, after six weeks the difference between the goal and actual reading time was still sizable. The fact that parents appeared to respond to the information on the difference between their goal and their actual reading suggests that goal-

setting may have been an important part of the experimental intervention. The fact that parents consistently set high goals may indicate the importance they attached to reading to their children.

E. *Did Information about Reading Matter?*

As noted, the electronic tablets for parents in the treatment group had videos and PDFs on the importance of parental engagement, especially reading, while the tablets for the control group had placebo videos and PDFs. However, only 15 parents in the treatment group and 15 parents in the control group ever opened a video or PDF during the six-week experiment. Informal discussions with parents suggest that they mainly opened these as part of their exploration of what was on the tablet and not to engage in the content.

In addition, the survey that we administered at the end of the experiment asked three questions about parental engagement, which was the subject of the informational material on the treatment group tablets. If the information on the tablets made a difference, we would expect different responses from parents in the treatment and control groups to these questions.

For example, we asked the following question: "If you were to spend 15, 30, 45, or 60 more minutes per day in educational activities with your child, how do you think your child's reading skills would compare to all other children of the same age across the United States on a 0–10 scale?" Responses indicate that parents in both the treatment and control groups believe that spending more time in educational activities with their child would increase the child's skills and the small differences between the treatment and control parents was not statistically significant.

In the same survey we also asked parents, "Whose job is it to teach math and reading to preschool children."[xxii] If parents think it is mainly the job of the preschool to teach math and

reading, it implies that they do not believe that they need to spend educational time with their child in order to improve these skills. But only 1 percent of parents of the treatment and control groups combined reported that it was "only" or "mostly" the job of the preschool to teach math and reading to preschoolers; there was no differences between the treatment and control groups on this measure.

We also asked, "Think about a child's ability to talk and use language. How much do you think this ability is set from birth and how much is based on what parents teach the child?" If the information provided on the tablets was an important part of the treatment effect, we would expect that parents in the treatment group would be less likely to answer that they believe ability is set. However, only 6 percent of the parents in the combined treatment and control groups indicated that a child's ability was "all" or "mostly" set at birth and there was no difference between the treatment and control group.

The survey results suggest that parents in both the treatment and control groups believe that spending educational time with their child will increase their child's skills and that it is the parents' job to do this. A fuller description of these results can be found in the Online Appendix. It is perhaps not surprising that there is so little difference in the responses to these questions, given that an important mission of Head Start is to convey to parents the importance of engaging their children and especially the importance of reading to their children. Parents clearly hear the message, which suggests that additional information about reading and parental engagement as presented in the PACT experiment played almost no role in the treatment effect.

F. *Robustness Checks*

In this subsection, we discuss the robustness of our estimates to dropouts, outliers, the inclusion of covariates, school-fixed effects, and clustering. We close with a discussion on the possibility that parents substituted reading in the tablet with reading nonelectronic books.

Dropouts. Of the 169 parents, eleven returned their tablets and stopped their participation in PACT before the end of the experiment. Six of these parents belonged to the control group and all of them stopped during the first week of the experiment. Two of them recorded some reading. Of the five treated parents who stopped, three did so during the first week and two did so during the second week. We asked all eleven parents why they dropped out. The reasons included that they did not enjoy the check-ins (when we downloaded data from the tablets); the child was too young to use the tablet; the child was not interested in reading on a tablet; the children in the home were fighting over the tablet; the experiment felt like an invasion of privacy; and that the child was sick.

The results in Table 2 include the parents who dropped out and count them as not spending any time on the app after they dropped out. If we omit the eleven parents who dropped out, the treatment effect on the number of minutes spent reading with the app increases—though not significantly—from 88.3 to 92.3 minutes.

Outliers. Table 4 shows the sensitivity of our results on reading time to the omission of outliers. Column 1 shows our estimation for the effect of the treatment on reading time for the full sample. Columns 2, 3, and 4 exclude parents who read more than 600 minutes (three standard deviations [SD] above the treated group mean), 450 minutes (two SD), and 300 minutes (one SD), respectively.[xxiii] Not surprisingly the coefficient on the treatment declines as we omit outliers. However, the effect sizes remain very large, ranging from 1.38 in the second column to 1.00 in the fourth column. As the Online Appendix shows, our results are also robust

when looking at additional outcomes, such as the number of books read and doing any reading. We also tested the sensitivity of the results to omitting those who did not read at all, as shown in Column 5 in Table 4. Among parents who read at all, parents in the treatment group read 83.4 more minutes than those in the control group, which equals an effect size of .92.

Covariates, School-Fixed Effects, and Clustering. Of the 169 parents, 160 answered our initial survey. From those 160 parents, 151 have complete information (that is, nonmissing values) for the set of covariates described in Table 1 (except family income and race and ethnicity, because these had a large number of missing data). The 18 parents with some missing information include 10 in the treatment group and 8 in the control group. The fourth column in Table 2 shows the results for the sample with complete information in the set of covariates in Table 1. The coefficient on treatment drops from 88.3 to 79.5. Column 5 adds school-fixed effects, column 6 controls the covariates from Table 1, and column 7 adds school clusters. The results show that both the treatment effect and its statistical significance remain virtually unchanged when we condition on a large set of covariates, use school-fixed effects, and when we cluster on schools.

Substitution. We were mainly interested in whether the treatment in PACT could motivate parents to engage in the specific task of using the digital library to read to their children and not necessarily in increasing the total reading time of parents. As noted above, the time parents spend using the tablet and app is important because the first-order problem with getting parents to read more to their children is motivating them to participate in programs aimed at increasing reading time and other forms of engagement.

However, increasing total reading time is also an important goal, which raises the question of whether parents substituted reading on the tablet for reading nonelectronic books.

The question of substitution arises in many RCTs because we seldom can measure all the alternative behaviors that participants might have engaged in absent the experiment. But in the case of PACT, the attractiveness of the tablet and the number and variety of books on the tablet may have encouraged parents to forego paper books for books on the tablet. If parents in the treatment group were induced to switch all their reading to the table while parents in the control group continued to read paper books, our results would overstate the extent to which parents in the treatment groups read more than parents in the control group.

We have no direct evidence on the extent of such substitution. However, indirect evidence suggests that little substitution occurred overall or at least that it did not occur differentially for treatment and control group parents. First, parents in both the control and treatment group had tablets and the reading app, and both groups received the same instructions on using the tablet and app. Both groups were told that we would download data each week about the use of the app. Both groups were told that they could use the apps as much as they wanted. If the tablet and app induced parents to substitute the tablet for paper books, it is likely that this would have applied equally to both the control and treatment group. Second, the amount of time that the parents in our sample spend reading to their children is small. As Table 2 shows, the control group read 63.3 minutes over the entire six-week experiment. This equals about 1.5 minutes of reading per day. If the tablet and the app enticed parents to read using the app rather than reading paper books, it implies that parents spent close to no time reading from paper books, which leaves little room for substitution. This is consistent with previous research, described above, that shows a high proportion of disadvantaged parents spend very little time reading to their children.

If parents had access to their own reading apps they may have used these rather than the app that we provided, resulting in our underestimating total reading time. But this is unlikely, because none of the parents in the treatment or control group had an electronic tablet of their own, only three or four had access to the internet at home, and an equally small number had smart phones that could support a reading app.

This indirect evidence makes it reasonable to assume that prior to the experiment, the amount of time that PACT parents read to their children was extremely small and therefore there was little room for substitution.

V. Conclusions

We designed an experiment to test whether a set of behavioral tools could increase the amount of time that disadvantaged parents spend reading from a digital library on a tablet to their preschool children. We used behavioral tools because previous research suggests that some disadvantaged parents do not read to their children because they undervalue the future gains for their children relative to the present time and effort involved in reading. Our results show that the treatment more than doubled the amount of time that parents read using the digital library (a 1 SD effect size) after six weeks of intervention. Our evidence that high-discount parents read the least to their children but responded the most to the treatment supports the hypothesis that a high-discount rate on the future reduces parents' investments in their children's future, although we cannot rule out the possibility that other factors, correlated with time preference, impede reading to one's children.

The findings from PACT can be put in perspective using the results from the research on the effect of reading to children on test scores. For example, in Price and Kalil (2017), the average benefit from having a parent read an additional day per month, which is equivalent to

about a half an hour of additional reading time per month, is .08 SD on the PPVT. In PACT, the increase in reading time is twice the increase reported in Price and Kalil, so if the effect of reading time on test scores is linear, we expect reading skills to increase by a factor of two. However, we do not know how long the effects are sustained, increases in reading time were not necessarily distributed across days in a month in the same way in the two studies, and we do not know if the relationship between parents' reading time and skill is linear. In addition, the participants in our sample are more disadvantaged than the in sample of Price and Kalil, which could imply a greater or lesser benefit to parents' reading time. Much more research on the effect of parents reading to their children is needed.

Because the PACT intervention used several behavioral tools at once to motivate parents to read to their children, we cannot say for certain which of these tools or what combination of tools resulted in increased reading. Evidence from the baseline surveys of parents suggests that information about the importance of reading was unlikely to have increased reading time, because parents in the treatment and control groups already believed that reading more to their children would improve their children's literacy skills and because the treatment did not change parents beliefs about the importance of reading for parents in either the treatment group or the control group. The fact that parents became more likely to achieve their goal over the course of the intervention suggests that goal-setting may have been an important part of the experimental intervention, at least when goal-setting was combined with reminders to meet the goal and feedback on progress toward meeting the goal.

Several theories try to explain why advantaged parents are more engaged with their children than disadvantaged parents are, especially in educationally related activities. These explanations include differences in the time and resources that are available to parents,

differences in expected returns for the time that parents spend with their children, and differences in information about the importance of educational activities, or how to engage in educational activities. One or more of these theories has guided most of the interventions intended to increase parental engagement. However, neither our results nor previous research suggests that these explanations are likely to entirely or even mostly explain the gap in parental engagement. Instead, our experimental results suggest that attempts to design programs to increase the time that parents spend reading to their children may benefit from the addition of behavioral tools that take into account differences in parental time preference or factors correlated with time preference. Even though the adoption of behavioral tools for this purpose is still in its nascent stage, the evidence nonetheless suggests that these tools can be cost effective and may provide a promising way to help parents engage with their children more often and more productively.

While the PACT study results strongly suggest the promise of using behavioral tools to motivate parental engagement with their children, many questions for future research remain. Among these are how long a behavioral intervention must last before parents no longer need the behavioral tools to reinforce the new behavior; which specific behavioral tools lead to the greatest change in parental behavior; and how much does altering parental behavior alter child outcomes. Finally, the results of PACT—like the results of all research—should be replicated. If replications support the evidence found in PACT, finding ways to widely implement behaviorally informed programs to alter parental engagement is also a high priority.

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Table 1

Summary statistics for the parents and children in the total sample and in the treatment and control groups

Variable	Total Sample Mean	Treatment Mean	Control Mean	Mean Difference	*p-value*
Parent's characteristics					
Female (percent)	0.92	0.91	0.94	-0.03	0.63
Single	0.61	0.62	0.60	0.02	0.78
Age (years)	31.13	31.55	30.71	0.85	0.39
Black or African American (percent)	0.32	0.29	0.36	-0.07	0.72
Hispanic (percent)	0.66	0.71	0.59	0.13	0.32
Less than high school (percent)	0.28	0.25	0.30	-0.05	0.36
High school diploma (percent)	0.12	0.15	0.09	0.06	0.30
GED (percent)	0.06	0.10	0.01	0.09	0.10
Some college (percent)	0.29	0.24	0.34	-0.10	0.10
Associate's degree (percent)	0.13	0.14	0.13	0.01	0.78
Bachelor degree or higher (percent)	0.13	0.13	0.14	-0.01	0.97
Focal child's characteristics					
Age (years)	3.76	3.74	3.77	-0.03	0.89
Boy (percent)	0.44	0.47	0.41	0.06	0.28
Born early (percent)	0.16	0.20	0.13	0.07	0.07
Born late (percent)	0.13	0.11	0.15	-0.04	0.46
Born on time (percent)	0.70	0.69	0.72	-0.03	0.41
<5 pounds at birth (percent)	0.24	0.24	0.24	0.00	0.90
Has disability (percent)	0.12	0.13	0.11	0.01	0.71
Household characteristics					
Number of children in the household	1.99	1.94	2.05	-0.11	0.45
Number of adults in the Adults in household (besides parent)	1.28	1.38	1.18	0.18	0.28
Household income last year ($1,000s)	20.85	18.82	22.59	-3.76	0.31
English spoken at home (percent)	0.65	0.62	0.67	-0.05	0.71
Observations	160	80	80		

Notes: Table 1 reports summary statistics for the subsample of 160 parents (out of 169) who answered our initial survey (4 and 5 parents from the treatment and control group, respectively, did not answer. From the 160 parents who answered, 151 have complete information (that is, nonmissing values) for the characteristics described in Table 1 (except family income and race and ethnicity, which had more missing values). The parents with some missing information are balanced by treatment status.

Table 2
The effect of the PACT treatment on measures of parents' reading to their children using the digital library

Dependent Variable	(1) Any Reading	(2) Number of books	(3) Minutes Read	(4) Minutes Read	(5) Minutes Read	(6) Minutes Read	(7) Minutes Read
Treatment	0.12** (0.05)	16.66*** (4.50)	88.32*** (18.79)	79.50*** (20.00)	80.09*** (19.27)	83.03** (21.07)	83.03** (26.06)
Constant	0.84*** (0.04)	14.78*** (3.00)	63.34*** (9.49)	66.87*** (10.25)	44.98*** (14.26)	-40.30 (89.86)	-40.30 (103.61)
SD Control Group	0.37	27.68	87.48	89.89	89.89	89.89	89.89
Dependent Variable Mean	0.89	23.06	107.24	105.83	105.83	105.83	105.83
Effect Size	0.31	0.60	1.01	0.88	0.89	0.92	0.92
Observations	169	169	169	151	151	151	151
Complete Information[(a)]	-	-	-	Yes	Yes	Yes	Yes
Preschool FE	-	-	-	No	Yes	Yes	Yes
Covariates[(b)]	-	-	-	No	No	Yes	Yes
Preschool Clustering	-	-	-	No	No	No	Yes

Notes: Huber-White sandwich estimator standard errors are in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01. The table shows results for the estimation of equation (1) using "Any reading," "Number of Books," and "Minutes Read" as outcomes after the six-weeks intervention, each of which are explained as follows: Column 1: "Any reading" is the fraction of parents who read at all to their children using the app. Column 2: "Number of books" is the number of books parents read to their children using the app. Columns 3–7: "Minutes read" is the number of minutes parents read to their children using the app. Columns 1–3 include all parents who participated in PACT. SD Control Group is the standard deviation of the control group for the respective samples. The effect size is the ratio of the coefficient for the treatment group to the standard deviation of the control group.
[(a)] Columns 4–7 exclude parents with missing values for some covariates and keeps the 151 parents who have complete information for the set of covariates described in Table 1. [(b)] The covariates included in column 6 and 7 are all those listed in Table 1.

Table 3

Treatment effect of PACT on the number of minutes spent reading with the digital library for parents with high and low discount rates on the future

Dependent Variable:	Minutes Read			
	(1)	(2)	(3)	(4)
	Baseline: All PACT Parents	Participated in time preference task	High discount parents	Low discount parents
Treatment	88.32***	82.12***	124.52***	42.26
	(18.79)	(24.08)	(33.25)	(33.95)
Constant	63.34***	83.61***	68.33***	97.26***
	(9.49)	(14.11)	(12.57)	(24.19)
SD Control Group	87.48	102.76	62.95	128.08
Dependent Variable Mean	107.24	63.34	135.20	119.11
Effect Size	1.01	0.80	1.98	0.33
Observations	169	112	54	58

Notes: *Huber-White sandwich estimator* standard errors are in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01. The table shows results for the estimation of equation (1) using "Minutes Read" (after six weeks of intervention) as the outcome. Column 1 includes all parents who participated in PACT. Column 2 includes parents who participated in the time preference task. Column 3 and 4 divides these parents into low-discount (high-discount) parents depending on whether they score below (above) the median on the time orientation task. SD Control Group is the standard deviation of the control group for the respective samples. The effect size is the ratio of the coefficient for the treatment group to the standard deviation of the control group.

Table 4

Treatment effects on the number of minutes that parents spent reading to their child with the digital library excluding outliers

Dependent Variable:	Minutes Read				
	(1)	(2)	(3)	(4)	(5)
	Baseline: All PACT Parents	Excludes parents who Read more than 600 minutes	Excludes parents who Read more than 450 minutes	Excludes parents who Read more than 300 minutes	Excludes parents who Did not read at all
Treatment	88.32*** (18.79)	86.48*** (15.57)	72.48*** (13.74)	51.30*** (10.75)	83.42*** (19.77)
Constant	63.34*** (9.49)	56.69*** (6.85)	56.69*** (6.85)	52.67*** (5.62)	75.82*** (10.76)
SD Control Group	87.48	62.78	62.78	51.16	90.68
Dependent Variable Mean	107.24	99.67	92.04	76.50	120.02
Effect Size	1.01	1.38	1.15	1.00	0.92
Observations	169	167	164	155	151

Notes: *Huber-White sandwich estimator* standard errors are in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01. The table shows results for the estimation of equation (1) using "Minutes Read" (after six weeks of intervention) as the outcome. Column 1 includes all parents who participated in PACT. Column 2 excludes parents who read more than 600 minutes (1 from the control group, 1 from the treatment group). Column 3 excludes parents who read more than 450 minutes (1 from the control group, 4 from the treatment group). Column 4 excludes parents who read more than 300 minutes (2 from the control group, 12 from the treatment group). Column 5 excludes parents who did not read at all (14 from the control group, 4 from the treatment group). SD Control Group is the standard deviation of the control group for the respective sample. The effect size is the ratio of the coefficient for the treatment group to the standard deviation of the control group.

i. See Bus, van IJzendoorn, and Pellegrini (1995) for a previous meta-analysis. They report that in all of the studies reviewed, parental book reading was measured using self-reports except Wells (1986) who reported observation data gathered with the help of a microphone affixed to the children's clothes.

ii. In the United States, the average length of the school day was 6.4 hours in the 2009–10 school year and the average length of the school year was 180 days, according the National Center Educational Statistics, Schools and Staffing Survey for that year. If we assume that this number is constant over all the years that a child attends school and that the number of years including kindergarten is 13, then (6.4*180)13 =14,976 is the total number of hours spent in school over the first 18 years of a child's life. If we assume that children average 16 hours awake each day, the number of waking hours in the first 18 years is (16*364)18 = 104,832. Thus, 14,976/104,832 = 14.2 percent of a child's waking hours in the first 18 years is spent in school.

iii. See also Price and Kalil (2017) for a review of the observational literature on the relationship between parental reading and children's cognitive skills.

iv. The NCES reports that the percentage of parents by parental education and poverty status that reports regularly telling stories to their children is very similar to the percentage that reports they regularly read books to their children.

v. In developing countries, home visiting programs that provide instruction in and materials for psychosocial stimulation to children to disadvantaged parents have had more success at improving cognitive skills (for example, Attanasio et al. 2014).

vi. Scholastic is a reading advocacy organization that may have an interest in showing that parents believe that reading is important. But the survey was done by YouGov, a reputable research organization.

vii. The National Assessment of Adult Literacy was conducted in 2003. The figures reported here were supplemented with data collected in 2006. Although these data are somewhat old, newer data from the 2008–13 Program for the International Assessment of Adult Competencies (PIAAC) suggests that overall literacy levels in the United States have not changed at all since 2003. Demographic breakdowns of the PIAAC scores are not published.

viii. All tablets also had six instructional videos (each in Spanish and English) preloaded on them. Two of these were about how to use the reading app and the other four were an introduction to the PACT study, the consent form, and the tablet agreement form that participants were asked to sign.

ix. A map of the City of Chicago with the geo-referenced preschools can be found in the Online Appendix.

x. More detailed information on the recruitment procedures is available in the Online Appendix.

xi. We compare differences in 22 variables in Table 1, so we expected 2.2 of them to be significant at the 10% level due to chance.

xii. Some parents may "outsource" the time investment to another family member, such as an older sibling, a coparent, a grandparent, or someone else. While in principle we could distinguish who was reading to a child, in practice this became burdensome. In addition, there is no reason to expect that there would be differences between the control and treatment group in the tendency to outsource reading to others. Therefore, we counted all minutes in which the app was used for actual reading. We spot-checked videos to ensure that there was both an adult and a child in them.

xiii. We found no statistically significant difference between the treatment and control group parents in the average time that they spent reading each story, or in the number of different books read by parents.

xiv. See the Online Appendix for more on details, procedures, and the questionnaire.

xv. The CTB method identifies the curvature in the utility function over time by estimating a respondent's sensitivity to changing interest rates. The key in using this approach is to vary the implicit interest rate in the options presented across subsequent sets of options. The sensitivity to changing interest rates across the question sequences identifies the utility curvature; the time preference is identified through the stated preference over the timing of payments.

xvi. For a parent who won the lottery, the amount and timing of the payout was determined by randomly selecting one of the fifteen payout choices that the parent preferred. Parents always had a greater than one-in-ten chance of receiving a payout and they were informed of these odds prior to participating in the task.

xvii. Decreasing the earlier payment is equivalent to increasing its price relative to the future payment.

xviii. We tested the robustness of our results to alternative measures of impatience, like the extent of "present bias" (identified by beta in our intertemporal choice model) and the percentage of the budget allocated to the earlier time point with similar results.

xix. 70 percent of the treated parents participated in the time preferences task, while 62 percent of the control group parents did. The mean difference of eight percentage points has a standard error of 7.3 percentage points, and is therefore not significant at the .05 level.

xx. The z-test for the difference is given by $z = (B1 - B2) / \sqrt{(seB1^2 + seB2^2)} = .203$, and hence is nonsignificant.

xxi. The Online Appendix describes the process for estimating the average weekly goals and the average weekly actual amount of reading time.

xxii. In a pretest, we asked separately about math and reading. Because parents almost always gave the same answer for math as they did for reading we combined them in the survey for the whole PACT sample.

xxiii. The standard deviation for the treated group is 148 minutes.