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Engaging Parents with Preschools: Evidence from a Field Experiment*

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Abstract:

Parental engagement with schools is an important part of child development. Yet, parent engagement with preschools tends to be low. To increase parental attendance at school-sponsored family-engagement events, we conducted a 4-month RCT with 319 parents across six preschools in Chicago. We designed an intervention using a combination of financial incentives and two tools from behavioral economics: loss-framing and reminder messages. The treatment parents were given a $25 per event incentive to attend 8 events sponsored by their preschools, as well as weekly text message reminders about the events. The financial incentive was framed using loss aversion: parents were initially given $200 in a virtual account, and lost $25 for each missed event. We find that this intervention did not increase the fraction of parents that attended at least one event. implying that there was no extensive margin treatment effect. The intervention did lead to a modest increase in the attendance rate among parents who attend at least one event.

JEL Codes: D91, I20, J13.
Keywords: Early Childhood Education, Field Experiments, Loss Aversion

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1 Introduction

Many observational studies show that parents who attend school events, volunteer at school, communicate with teachers, and engage in school activities have children who perform better in school (for example, Castro et al., 2015; Dizon-Ross, 2019; Domina, 2005; Hill & Tyson, 2009; Liu & White, 2017; McNeal, 2012; Wang & Sheikh-Khalil, 2014). There is some causal evidence of positive effects of various forms of parental engagement in schools (Avvisati et al., 2014). Consistent with the high perceived benefit of parent engagement in schools, publicly supported preschools such as Head Start spend substantial effort promoting family engagement (E. F. Zigler & Muenchow, 1992).

Despite their prevalence, there is almost no experimental work evaluating the benefits of parent engagement programs. An RCT to evaluate existing parent engagement programs would need to exogenously induce parents to attend the engagement programs to which they currently have access. Such an evaluation would need to have a strong "first stage," whereby a substantially larger fraction of treatment parents attend engagement programs relative to control parents. Existing research, which shows that parental attendance tends to be low, suggests that a novel approach is needed. In this paper, we test whether a behaviorally-informed intervention that combines financial incentives and behavioral tools could lead to such a substantial increase in parent attendance at engagement events. If successful, this approach could not only be used by programs themselves but could be incorporated into future research to test the causal impact of parental attendance on child outcomes.

In particular, we use an RCT to test the combined impact of loss-framed financial incentives and text-message reminders on parental attendance for 319 parents at preschool-sponsored family engagement events at six subsidized preschools in Chicago, IL from

November, 2018 to March, 2019. Our experiment used an opt-out design. Only 20 parents opted out. We include the opt outs in the main analysis to identify the intent-to-treat effect, because it is more policy relevant.

Treatment parents were offered $25 per event for eight events. The average length of an event was 90 minutes, making the compensation slightly above the median hourly wage of parents in this demographic at the time. In addition to weekly text message reminders with details about event(s) that week, parents in the treatment group received their financial incentive using a loss-frame. That is, parents were initially given $200 in a virtual account (redeemable at the end of the experiment), but $25 was deducted from their account for each missed event. Each week, treatment parents received a second text message that indicated how much money they had remaining in their account.

We find that the treatment led to no significant difference in the fraction of parents that attended at least one event; 57% of control parents and 56% of treatment parents attended at least one event. We find a modest, statistically significant increase in the attendance rate among parents who already attended at least one event: among parents that attended at least one event, the overall attendance rate across all eight events was 22% for control parents and 29% for treatment parents. There was no significant heterogeneity in treatment effect by time of day or event length. We develop a theoretical framework to discuss our findings. One reason for the lack of treatment effect and low overall attendance rates might be because parents perceive such school-sponsored events as having a low expected return on investment for their time.

Our treatment design was based on existing studies designed to increase parental engagement in children's learning, broadly defined, especially in low-income families. Several recent experimental studies show that tools drawn from behavioral economics can boost parental

engagement and improve child outcomes, such as reading, math and preschool attendance among low income families (Kalil et al., 2021; List et al., 2018; Mayer et al., 2023; Mayer et al., 2019).

Three experimental studies relate closely to the present work. These studies and our relation to them are described in section 2.2. Gennetian et al. (2019) and Hill et al. (2021) use a bundle of behavioral tools including reminders, commitment devices, and personalized invitations to increase parental attendance at preschool events. While Gennetian et al. (2019) find that behavioral tools increase attendance, Hill et al. (2021) find no significant effect. One behavioral tool not included in these studies is loss-framing, which the present study tests.

Fryer et al. (2015) test whether financial incentives increase parental attendance at parenting workshops at a preschool run by that research team. They find that a $100 incentive for a 90-minute workshop led to a substantial effect on attendance. The present study differs from Fryer et al. in two ways: first the incentive we offer is only a quarter as large and second our study is situated in the ongoing parental engagement events and efforts offered by existing preschools in the community. This natural setting can help mitigate external validity concerns.

We contribute to the human capital development literature. Parent engagement is a crucial aspect of child development; we test whether engagement in school events can be increased in a natural setting. Given our opt-out design, the results are more likely to hold at scale as compared to studies requiring opt-in participation (Mayer et al., 2021). An experimental test for the effectiveness of parental engagement on student outcomes will require a treatment that could reliably provide a substantial exogenous increase in parental engagement. Our study provides evidence that modest financial incentives, even when combined with reminders, do not induce a substantial increase in engagement in preschool events.

We also contribute to the economics of education literature on parent communication. York et al. (2019) found that reminder messages to parents led to improved child literacy. Kraft and Rogers (2015) find that communication with parents of high school students increased the likelihood of passing a summer course. Castleman and Page (2017) found that a text message intervention helped increase parental engagement in the college enrollment process. Our intervention finds a treatment effect only for parents who were already engaging with their school. This study provides evidence of the limitations of parent communication interventions at engaging otherwise disengaged parents.

This study also adds to the recent economics literature on combining financial incentives with other tools to change behavior (Arad et al., 2023; List & Shah, 2022) For example, Barrera-Osorio et al. (2020) provided financial assistance to parent associations in Mexico, as well as information to individual parents. They find that while information led to a change in parent behavior at home, financial assistance did not lead to behavior change for students or parents. Economists have used loss aversion to increase the effectiveness of incentives in education (Fryer et al., 2022; Imas et al., 2017; Levitt et al., 2016). In some instances, loss-framed incentives may have negative consequences, such as neglecting unincentivized aspects of the task (Pierce et al., 2020). However, a recent field experiment in Uganda showed that loss-framed incentives can increase labor productivity (Bulte et al., 2020) and found no negative incentive spillovers (Bulte et al., 2021).

The remainder of this paper is organized as follows. In Section 2, we briefly describe the context of preschool parental engagement programs. In section 3, we describe our sample and intervention. In Section 4, we present our main results. In Section 5, we describe a theoretical model we use to interpret our results. Section 6 concludes with policy implications.

2 Background

2.1 Parent Engagement and Head Start

Although research has demonstrated that increasing parents' direct engagement with their children increases children's academic success (Cunha et al., 2006; Fiorini & Keane, 2014; Heckman & Masterov, 2007; Villena-Roldan & Ríos-Aguilar, 2012), there is little evidence on the efficacy of the kinds of programs that preschools provide under the umbrella of family engagement. From its inception, Head Start has emphasized parent involvement. A founding principle of the Head Start program was the "maximum feasible participation" of the parents (Harmon, 2004; E. F. Zigler & Muenchow, 1992; E. Zigler & Styfco, 2010). The Head Start Code of Federal Regulations[2] states:

> *A program must, at a minimum, offer opportunities for parents to participate in a research-based parenting curriculum that builds on parents' knowledge and offers parents the opportunity to practice parenting skills to promote children's learning and development.*

Parent engagement in preschool is also required in the "Every Student Succeeds Act"[3] and many state requirements for preschools also follow Head Start's code for parent engagement. A few observational studies have considered parent engagement in these types of programs, mainly finding positive correlations between parent engagement and child academic and behavioral outcomes (Ansari & Gershoff, 2016).

[2] Chapter XIII part 1302.51b; available online at https://eclkc.ohs.acf.hhs.gov/policy/45-cfr-chap-xiii/1302-51-parent-activities-promote-child-learning-development

[3] See https://www.everystudentsucceedsact.org/title-1--1-1-3-1-1-1-1-1

Regulations are clear that preschool programs should promote parent engagement in school activities. Yet, there are few guidelines about what preschools should do to accomplish this. The Head Start Performance Standards hold that teachers must regularly communicate with parents about their child's schooling, hold at least two parent conferences a year, have at least two home visits, provide parents the chance to volunteer at the school, implement intentional strategies to engage parents in their children's learning and development, offer activities that support parent-child relationships and child development including language, dual language, literacy, and bi-literacy development as appropriate, and provide family engagement services in the language and cultural context of the family[4].

In addition to the federal regulations, The National Head Start Association promotes the *Two Generations Together* initiative, which is focused on increasing awareness of the "two-generation" adult education and job training models that are part of the comprehensive child and family services delivered by Head Start programs across the country (Dropkin & Jauregui, 2015). Two-generation approaches focus on creating opportunities for and addressing the needs of children and their caregivers together with the goal of creating economic stability for the family. Providing parent engagement programs is not costless: not only must preschools dedicate personnel and space to host and promote the events, but regulations also require preschools to develop and submit plans for engaging parents. Those plans sometimes must be reviewed by multiple individuals[5].

Family engagement activities offered by Head Start programs include school information sessions, conferences with teachers, social events, parent-child activities, parent education

[4] See the list of requirements here: https://eclkc.ohs.acf.hhs.gov/policy/45-cfr-chap-xiii/1302-34-parent-family-engagement-education-child-development-services
[5] See https://www.everystudentsucceedsact.org/title-1--1-1-3-1-1-1-1-1, Section 1010, parts B and C

programs, social service programs, and volunteer opportunities at the school. In general, research shows that disadvantaged parents communicate less with their child's teachers and are less likely to attend parent-teacher meetings and other school events (McQuiggan & Megra, 2017; Turney & Kao, 2009). So, it is perhaps not surprising that parental attendance at preschool-sponsored parent engagement events tends to be low (Avvisati et al., 2010). For example, Mendez (2010) reported that parents attended fewer than two parent-engagement workshops out of nine offered. In another descriptive study, average attendance at Head Start parenting events was reported to be about 21% (Marti et al., 2018).

At least three reasons might explain the low attendance. One reason is structural barriers to attendance, such as work conflicts, lack of transportation, or other inflexible obligations. A second reason might be that parents don't believe the events are worth attending. This may be because the expected benefit from attending is low or because the opportunity cost of attending is high. A third potential reason for low attendance is that cognitive biases, such as inattention or present bias, might influence parenting decisions (Mayer et al., 2019). Parents may want to attend an event and believe it is worthwhile to attend, but simply forget to attend or forget to arrange transportation to attend. The intervention we describe here addresses the second and third of these reasons simultaneously: financial incentives are intended to offset parents' opportunity cost, and reminder messages are intended to offset the influence of present bias and inattention.

2.2 Related RCTs on Preschool Parent Engagement

We know of only two experimental studies that have used behavioral tools to boost parent attendance at parent events at preschools serving low-income children. First, Gennetian et

al. (2019) designed and tested a 23-week behaviorally-informed intervention for 99 families in Head Start to manage several different behavioral bottlenecks. Their program bundled various treatments, including personalized invitations to parents informing them of the upcoming fall "kick-off" event at the preschool, text messages to remind parents when that event at the preschool was taking place, and text-based prompts designed to elicit parents' commitment to attend that event. The results showed that parents randomly assigned to the treatment group were significantly more likely to attend the program "kick-off" event at the beginning of the school year (53% versus 38% in the control group) and were more likely to attend at least one workshop out of seven subsequent ones offered over the approximately 6-month study period. Nonetheless, the rate of treated parents' attendance at any given event during the school year never exceeded 35%.

Second, in a study using a similar approach, Hill et al. (2021) aimed to boost attendance using behavioral tools at a 14-session parenting program in New York City's Pre-K for all centers. This study was conducted on a large scale and included both a site-level and a family-level intervention. Parents at treatment sites received behaviorally-enhanced outreach materials, including a personalized brochure, a letter of invitation to the program, an "affirmation postcard" asking parents to describe a time they had felt successful or proud, and other similar tools. Parents also received text messages reminders of the day and times of meetings. However, unlike Gennetian et al. (2019), this study found no significant increase in parent engagement from the behavioral tools.

Given the mixed findings from these two studies using behavioral tools alone, our experiment assesses whether the impact of behavioral tools can be substantially enhanced by pairing them with financial incentives. We know of one experimental study that offered financial

incentives to low-income parents to attend events at their child's preschool Fryer et al. (2015). Here, the research team developed a "Parent Academy" that included 90-minute sessions to promote positive interactions with their children. Treatment parents were given $100 per session for attendance if they arrived less than five minutes after the session began, and $50 if they were between 5 and 30 minutes late. Parents were also given various assignments intended to reinforce the material in the sessions and were paid various amounts for completing these assignments with various levels of quality. The treatment was highly effective at boosting attendance: 0% of the parents assigned to the control group attended any of the sessions, compared to 88% of those in the treatment group. Nearly half of the treated parents had a perfect attendance rate.

While the financial incentives in Fryer et al. (2015) had a substantial impact on attendance, the high dollar amount is unlikely to be feasible for most preschools. Additionally, the population in this study self-selected into attending the experimental pre-school and Parent Academy. Our study tests whether the impact can still be achieved with more modest financial incentives – a quarter of the amount in Fryer et al. (2015) – and for a population of parents whose children attend one of the existing subsidized preschool options. Additionally, our intervention did not include any components designed to motivate parents to engage in their child's learning at home: its sole focus was to boost participation rates at school events. By simplifying the components and goals of the intervention it is possible that our study design reduced frictions to attendance. The success of our intervention might depend on whether parents think of engagement at home and engagement at school as substitutes or complements. If parents think of these activities as complements, our intervention will be less effective because it only

focuses on school. In contrast, if parents think of these activities as substitutes, then a targeted focus on one context is more efficient.

3 Experimental Design

3.1 Sample and Data Collection

This experiment was conducted between November 2018 and March 2019 at six preschools serving low-income children in Chicago. Five of these preschools were Head Start centers. From administrative records, we were able to access three covariates: child's age, child's gender, and family's primary language.

There were 319 parents in total at the six centers, of whom 159 were assigned to the control group and 160 to the treatment group. The randomization was stratified by center to ensure that treatment status was balanced within each center. All parents at a center were automatically enrolled in the experiment, unless they opted out via text, email, or phone call. Twenty parents opted out (this is discussed in more detail in Section 4.2).

During the 4-month intervention period, different centers had varying numbers of parent events but no mechanism for collecting attendance at these events, so research assistants attended the events in person and collected attendance. We counted the participant as being "present" for a given event as long as any adult member of their child's family attended that event.

To maintain consistency across centers, we chose eight[6] events per center for which we tracked attendance. Any event open to all parents was eligible for inclusion, and if a center had more than eight such events, we chose a random subset of eight to include in our intervention.

[6] At one center, the total ended up being seven events due to unanticipated weather-related cancellation.

For centers that had fewer than eight events, we organized additional events with that center so they could offer eight events to parents.

Our primary outcome is a binary variable indicating whether or not the parent attended at least one event. Using a linear probability model, the treatment effect will indicate the fraction of parents that were induced to attend at least one event. Our secondary outcome measure is the attendance rate, which is the proportion of the eight events that each parent attended. For example, if a parent attended two out of eight events, we calculated their attendance rate as 25%. In addition, four of the six centers offered at least one event beyond the eight used in our study. We also measured attendance at these additional events, which allows us to assess whether there were any positive or negative incentive spillovers (see Section 4.5).

Events lasted from 60 to 120 minutes, with a median of 90 minutes. No event was offered on a weekend, and 60% were offered on a Thursday. The start times of events ranged from 7:30 AM to 6:00 PM, with a median start time of 4:00 PM and a mode of 3:30 PM. Most events focused on parent-child interactions, such as "Holiday Crafts Night," "Healthy Smiles Parent-Child Workshop," and "Chili and Chill Family Fun Night."

A few focused exclusively on parents, such as "Digital Literacy" and "Employment Workshop." For these events where the children are not expected to join, the schools all provided on-site childcare for the duration of the events.

3.2 Treatment Description

We did not make any contact with the control group beyond taking attendance at events. In contrast, for the treatment group, we sent two text messages per week and offered them $25

for attending each of the eight events. One of the text messages (sent on Sundays at 6:00 PM) reminded parents of events taking place that week as follows:

> *Plan to go to and sign in at [Preschool Name]'s event this week. You or another adult who cares for [Child Name] may go.*

> *[Event Name] is on [Day of Week] [Date] at [Time]. Hold onto the $ in your Bank. Mark your calendar!*

Parents received a second text message on Fridays at 6:00 PM, reminding them of the financial incentive. Treatment parents were told that they could redeem $200 at the end of the study (March 2019), but would lose $25 for each event missed. The weekly Friday text was as follows:

> *Your balance is [Amount] as of [Date]. Remember you started with 200 and lose 25 for every event you miss.*

Head Start is targeted to parents whose household income is at or below the federal poverty line. In 2021 the poverty line for a family of three was 21,960. A single parent working full time would earn $10.98 per hour to have an income at the poverty line. The median hourly earnings of workers in the bottom quintile of the earnings distribution is $10.22 (Ross & Bateman, 2019). Assuming transportation to and from the event takes about an hour, the median

total time spent to attend each event would be 2.5 hours. This would make our incentive equivalent to median parents' financial opportunity cost.

The cash incentive was provided in a behaviorally informed design to capitalize on loss aversion. Loss aversion refers to the idea that people put a greater weight on losses than on equivalent gains. The theory of loss aversion implies that people will be more responsive when money is taken from them versus when an equal amount of money is given to them (Kahneman et al., 1991).

3.3 Limitations

One limitation of this design is that the treatment bundles financial incentives, loss framing, and text message reminders so we cannot identify their unique contribution to any treatment impact. We bundled the treatments because our goal was to do a practical efficacy test to boost parental engagement as much as possible.

Another limitation is that parents could not collect their financial incentive until the end of the intervention. Adherence to this design may be challenging for parents who are present-biased; that is, disproportionately valuing the present. Present bias is a relevant cognitive bias that might affect parenting decisions (Mayer et al., 2019). As such, this works against our finding a treatment impact. The treatment impact could also be diminished if parents did not trust that they would receive the financial incentive. Theoretically, a clawback design can give the money upfront and take it back for failure to comply. We did not feel it was ethical to adopt this approach with a sample of low-income parents.

Another compliance concern is that we cannot be sure that participants received the text messages or that their phone numbers did not change during the course of the intervention. We

updated phone numbers if and when the schools did so. However, in our prior work with low-income parents we found that 97% of the phone numbers we had on file were still active one year after we had collected them (Kalil et al., 2020; Kalil et al., 2023; Kalil, Mayer, & Shah, 2023; Mayer et al., 2023).

4 Results

4.1 Descriptive Results

Table 1 shows the means and standard deviations for the treatment and control groups for the three covariates available from administrative data. There was no significant difference across treatment and control for any of the variables.

(Table 1 here)

Figure 1 shows the distribution of total number of events attended by parents. The mode is zero and the median is one. Our primary outcome variable is a binary variable with a value of 1 for parents who attended at least one event, and 0 otherwise. Our secondary outcome is attendance rate, which we compute by dividing the total number of events a parent attended by the total number of events offered (eight for 89% of parents, and seven for the remaining 11% whose school had an unexpected weather-related cancellation for the eighth event).

(Figure 1 here)

4.2 Extensive Margin Treatment Effect

We use our experimental data to estimate the following regression:

$$A_i = \alpha + \beta\, T_i + \gamma X_i + \delta_s + \varepsilon_i$$

A_i is an indicator for whether parent i attended at least one event, T_i is the treatment status indicator, X_i is a vector of observable child demographics (age, gender, and Spanish as primary language), δ_s represents school fixed effects, and ε_i is the error term. Table 2 provides estimates of β, with and without school fixed effects and child covariates.

(Table 2 here)

We find that that 57% of parents in the control group attended at least one event, and that fraction in the treatment group (56%) was not statistically significantly different ($p = 0.77$). The result does not change when the regression includes school fixed effects or covariates.

4.3 Attrition and Robustness Checks

Because the study had an opt-out design, all 319 parents were automatically enrolled in the study unless they opted-out via text, email, or phone call. 20 participants dropped out by week two of the study, but none did so after that. However, the dropout rate was not balanced between treatment and control: 4 control parents (2.5% of the control group) dropped out, and 16 treatment parents (10% of the treatment group) dropped out. This difference is statistically significant ($p = 0.006$).

It was easier for treatment parents to opt out because they were already receiving text messages and could opt out simply by replying to a text. Control parents, on the other hand, would have had to initiate a text message, email, or phone call to opt out. It is also likely that the study was more salient to treatment parents given the text messages they were receiving. This may have reminded them that they were in a study they no longer wished to be a part of.

We stopped tracking a parent's attendance after they dropped out. If there is a relationship between a parent's decision to drop out and their attendance rate, then removing these 20 observations may lead to an imbalance in expected unobservable characteristics between treatment and control. Therefore, we kept these observations and imputed zero for their attendance for two reason. First, the modal parent attended zero events and second, there was zero attendance among these 20 parents for the data we do have for them prior to their dropping out.

The binary outcome in the regressions in Table 2 has zero imputed as the outcome for the 20 missing observations. This might downplay the true treatment effect in any case, because most of these observations were in the treatment group. In Table 3, we show the results of running the same regression as in Table 2, but for a few alternative imputations for the missing outcomes.

(Table 3 here)

In column 1 of Table 3, the missing values of the outcome have 1 imputed, rather than 0. The treatment effect is still statistically insignificant, showing that our main findings do not depend on whether we impute 0 or 1 for the missing values of the binary outcome. As a bounding exercise, columns 2 and 3 of Table 3 show the lower and upper bound of the treatment effect. In column 2, we impute 0 for missing treatment values, and 1 for missing control values, which gives the lower bound of the treatment effect. In column 3, we impute 1 for all missing treatment values and 0 for all missing control values, which gives an upper bound of the treatment effect. None of these estimates of β are significant at the 5% level, showing that the imputation technique is not driving the results.

4.4 Intensive Margin Treatment Effect

Table 4 shows the treatment effect when the outcome is a continuous variable representing the attendance rate, which is computed as the total number of events a parent attended divided by the total number of events.

(Table 4 here)

Column 1 shows the regression results from the full sample. The attendance rate for the control group is 12.9%, and that of the treatment group is 16.5%, leading to a 3.6 percentage point treatment effect. This is the Intent To Treat (ITT) estimate of being assigned to the treatment group on attendance. This difference is statistically significant at the 10% level, but not the 5% level.

Column 2 shows the results of the regression when the sample is limited to parents who attended at least one event. Because the extensive margin effect is zero in this sample, the group of parents who attend at least one event in the treatment and control are arguably comparable on unobservable characteristics. Any difference in their outcomes could therefore be interpreted as the intensive margin treatment effect under this assumption. Of the 91 control parents that attend at least one event, the attendance rate across all events is 22%. Of the 89 treatment parents that attend at least one event, the attendance rate is 29% across all events. Column 2 of Table 4 shows that this 7-percentage-point difference is statistically significant at the 5% level. In relative terms, this is a 32% intensive margin treatment effect on attendance rate.

Despite this high relative treatment effect, the absolute level of attendance for the treatment group is still low in absolute terms, which we interpret using a theoretical framework described in Section 5.

We explore heterogeneity in the treatment effect of the continuous-specified attendance rate. We looked at heterogeneity based on two quantitative features of events that we could observe: event length and event timing. The events lasted between 60 and 120 minutes, with a median length of 90 minutes. The treatment effect on attendance rate was larger for shorter events, but this difference was not statistically significant ($p = 0.334$). For event timing, the median start-time for events was 4:00 PM (Note: school pick-up time for most centers was between 3:30 and 4:30 PM). There was no significant difference in the treatment effect for events that took place in the evening versus during the daytime, nor was the interaction term significant when event start time is treated as a continuous variable.

4.5 Incentive Spillover

One potential concern with our treatment is that rather than increasing the total number of events a parent attends, our treatment might simply make parents reprioritize which events to attend. For instance, if a parent in the treatment group was planning to attend exactly three events in total for the year, then our treatment might simply induce them to attend our events, for which they would be paid, instead of other events the school may offer. In that case, our treatment would have a negative *incentive* spillover.

In contrast, attending more events as a result of the treatment might make a parent develop a habit of attending events. As a parent attends more events, they might view the barriers to attending as less costly or the benefits to be gained as greater than previously perceived. Or, perhaps they form friendships with other parents or teachers at the additional events they attend, which increases their expected return on attending events in general. This increased likelihood of habit formation to attend events would be a potentially positive incentive spillover.

We can assess incentive spillovers for 211 parents because four of the six centers offered at least one event beyond the eight used in our study[7]. There were seven such events in total across the four centers. These events took place within the four-month window of our study, after the eight incentivized events. Specifically, for 134 parents (42% of our sample) we have attendance data for at least two events beyond the eight in the experiment. The treatment effect on the attendance rates at these unincentivized events is shown in Table 5.

(Table 5 here)

The results show that there was a positive incentive spillover: treatment parents are nearly twice as likely as control parents to attend one of the unincentivized events. Specifically, 14.8% of the treatment parents attended at least one event after the incentives ended, as opposed to 8.3% of the control parents. This difference is statistically significant at the 10% level but not the 5% level.

One concern might be that the treatment parents might not be aware that the incentives have ended. However, this is unlikely to be the case because treatment parents received weekly text messages with the specific name of each upcoming incentivized event, and a reminder of the incentive for attending that event. Such texts were not sent out for the unincentivized events.

While this sample of seven total events across four centers might not be convincing enough to confidently claim that the treatment causes parents to get into a regular habit of attending events after incentives are removed, the evidence is suggestive. This also shows negative incentive spillovers are likely not a concern: our treatment does not seem to divert attendance away from unincentivized events.

[7] The treatment effect for these four centers is similar to the treatment effect for the overall sample.

5 Theoretical Framework

To better interpret our findings, we next present a model of parental decisions to engage with their child's school.

5.1 A Simple Model of Parental Decision Making

Suppose $e_i \in \{0,1\}$ represents parent i's decision on whether to engage with their child's school (such as attending school-sponsored events), where $e_i = 1$ indicates engaging and $e_i = 0$ is not engaging. Suppose B_k is the parent's perceived benefit from attending event k and $c_i \in \mathbb{R}^+$ is the parent's opportunity cost of attending event B_k. If event k is an hour long, then c_i would represent that parent's hourly wage. Let $F(c)$ represent the cumulative distribution function for hourly wage (opportunity cost) c. The parent's utility function $u(e_i)$ is given by:

$$u(e_i) = (e_i)(B_k) + (1 - e_i)(c_i)$$

A parent's optimal decision would be to attend ($e_i^* = 1$) the event when $u(1) \geq u(0)$, which happens when $B_k \geq c_i$. This happens with probability $F(B_k)$.

5.2 Reminders

One way in which a reminder can influence behavior in this context is to simply inform the parent of something that they were not aware of (such as the event's time and location, or that it is happening at all). Another channel is that even if the parent was aware of the event details, a reminder can bring the event "top of mind" for the parent. Parents who would optimally choose to attend an event (ones with $c_i < B_k$) might not attend if the event is not on the top of their

mind. This might occur if the parent has multiple competing demands on their attention. For this type of parent, a text message reminder can prompt action.

Suppose there are two types of parents: attentive and inattentive. Let this be denoted by $\alpha_i \in \{0,1\}$, where $\alpha = 1$ denote the 'attentive' type of parent, who is both informed of an event and it is on their top of mind. For this type of parent, a text message reminder would not change anything. Let $\alpha = 0$ denote an inattentive parent, who is either uninformed of the event, or is informed but the event is not at the top of their mind. We will not distinguish among the reasons a parent might be inattentive, and will just note that a reminder can influence this type of parent if their optimal decision is to attend ($c_i < B_k$). Note that we are ignoring the possibility that a parent might be so inattentive that they miss a reminder message; we are assuming that even the $\alpha = 0$ parent would notice a reminder message in time for the event. Overlaying attention with opportunity cost, our type space for parents is $\tau = (\alpha_i, c_i) \in \{0,1\} \times \mathbb{R}^+$. We can amend the simple utility function above to incorporate α as follows:

$$u(e_i) = (e_i)(\alpha_i B_k) + (1 - e_i)(c_i)$$

The attentive types have the same utility function as before, and their decision to attend will be dependent on whether $c_i < B_k$. However, an inattentive type would never choose to attend the event, assuming $c_i > 0$.

Let us now define a reminder intervention as one that sets a parent's $\alpha_i = 1$. This means that for already attentive types, nothing changes. Additionally, those with $c_i > B_k$ will choose not to attend regardless of their attentiveness type, and will therefore also not be affected by this intervention. The only parent that would be prompted to go because of a reminder is one with both $\alpha_i = 0$ and $c_i < B_k$.

Denoting the proportion of attentive parents $P(\alpha = 1) = \theta$, the expected proportion of parents who attend event k without the reminder intervention is $\theta F(B_k)$. The expected proportion with the reminder intervention would be $F(B_k)$, making the expected treatment effect $(1 - \theta)F(B_k)$.

5.3 Financial Incentives with Loss Aversion

Now to incorporate financial incentives, suppose that a parent is given m_1 dollars for attending the event, and m_0 for not attending the event. This makes the utility:

$$u(e_i) = e_i\left[\alpha_i\left(B_k + v(m_1)\right)\right] + (1 - e_i)[c_i + v(m_0)]$$

Where $v(m)$ is given by the standard reference-dependent *loss aversion* value function:

$$v(m) = \begin{cases} m - r, & m \geq r \\ \gamma(m - r), & m < r \end{cases}$$

Loss aversion is when $\gamma > 1$. Empirical estimates of γ in the literature are around $\gamma \approx 2$, implying that a losses have twice the impact of an equivalent gain (Benartzi & Thaler, 1995).

Consider an intervention that gives m dollars to parents for attending event k. If the money is to be given after the event, we can think of the reference value r as being 0. With $m_1 = m, m_0 = 0, r = 0$, this gives $v(m_1) = m, v(m_0) = 0$, implying that for an attentive type of parent, the optimal decision is to attend if $B_k + m \geq c_i$.

If instead the parent is given m dollars up front, and then are asked to return the money in case they do not attend the event, this now changes our reference to $r = m$. With $m_1 = m$, $m_0 = 0, r = m$, this gives $v(m_1) = 0, v(m_0) = -\gamma m$, implying that for an attentive type of parent, the optimal decision is to attend if $B_k + \gamma m \geq c_i$. This implies that a loss averse framing would induce a higher share of parents to attend the event, since it increases the opportunity cost threshold above which a parent would choose to not attend.

The new expected proportion of parents who would attend the event after an intervention that combines reminders with a loss-aversion-framed incentive of m dollars would be $F(B_k + \gamma m)$. This would make the expected treatment effect equal to $F(B_k + \gamma m) - \theta F(B_k)$. Note that the treatment effect is larger when cognitive biases are stronger (the larger the γ and lower the θ).

5.4 Interpreting the Results

If all parents behave like rational agents ($\gamma = 1$ and $\theta = 1$) the expected treatment effect is $F(B_k + m) - F(B_k)$. Given the low baseline attendance rate in the control group, $F(B_k)$ is a fairly low number, meaning most parents have an opportunity cost greater than perceived benefit B_k. Because our monetary incentive is approximately equal to the median financial opportunity cost, we would expect that receiving m in addition to the perceived event benefit B_k would induce a majority of parents to attend in a frictionless world. That is, we would expect $F(B_k + m)$ to be greater than 0.50 if there were no structural barriers preventing parents from attending beyond the opportunity cost of attending. However, we estimate that $F(B_k + m)$ is far below this, because the treatment group's attendance rate across all 8 events was less than 20%, let alone 50%. Note that lack of substantive treatment effect is even more puzzling if parents have cognitive biases ($\gamma > 1$ and $\theta < 1$).

One potential interpretation here is that structural barriers to attendance, such as work conflicts or inflexible obligations, are far too prevalent for interventions such as ours to reasonably change parental engagement. Another interpretation is that parents' perceived benefit of attending an event, B_k is in fact negative. That is, if parents view attending such events as having a psychic cost and no associated benefit $B_k < 0$, then this is consistent with observing

attendance rates of less than 50% despite parents being offered the median opportunity cost as financial compensation.

Whether it is structural barriers or no perceived value added for events, the implication here may be that financial compensation to increase parental engagement will likely not increase parental attendance to substantial levels unless the compensation is much higher than the median parent's hourly wage (for example, Fryer et al., 2015). Depending on cost constraints, this implies that providing parents with financial incentives is likely not a feasible strategy to increase parental engagement for most preschools.

6 Conclusion

We designed an experiment to test whether financial incentives combined with reminders could increase low-income parents' attendance at parent engagement events at their children's preschools. We focus on these events because schools are mandated to offer them, because prior research shows that lack of attendance is a persistent problem, and because parent-school connections are theoretically relevant for children's skill development. We used financial incentives to test a theory about parents' assessments of the value of their attendance. We chose an amount for a financial incentive that approximated parents' opportunity costs in the labor market. To maximize the impact of the financial incentives we offered them to parents in a loss aversion framework. We used reminders – a common behavioral tool – to mitigate information frictions and parental inattention, that is, to make the information about the school events salient or “top of mind” to parents.

Our results show that the treatment had no impact on parents who did not attend any events, but did modestly increase the attendance rate among parents who attended at least one

event. The treatment also increased parents' likelihood of attending unincentivized events, which provides suggestive evidence that our treatment might help already-engaged parents strengthen a habit of engaging.

Even among parents who attend at least one event, the attendance rate for treated parents is still far below what many schools hope. It would be hard not to conclude from this study and related recent ones that preschools serving disadvantaged children should abandon or wholly reimagine their efforts to induce parents to attend school events. Structural barriers to attendance such as work conflicts may put a ceiling on expected parental engagement even in an ideal scenario. Parents may also view such events as having a low expected return for their time, and may need to be compensated substantially more than their lost earnings to attend. Schools may have more success in parental attendance by offering events that parents perceive as being worthwhile. As such, future work can randomize the type of events schools offer to better understand parents' preferences.

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Tables

Table 1: Descriptive Statistics and Balance Test

	Control			**Treatment**			**Difference**	
	N	**Mean**	**SD**	**N**	**Mean**	**SD**	**Diff**	**SE**
Female	159	0.51	0.50	160	0.50	0.50	−0.009	0.056
Spanish	159	0.22	0.42	160	0.24	0.43	0.024	0.047
Child's Age	159	4.18	0.63	160	4.08	0.61	−0.104	0.070

Note. The Diff column is the coefficient of a regression of treatment status on the variable, and SE is the robust standard error of that coefficient. Female is the proportion of children who are female. Spanish is the proportion of whose primary language is Spanish. Child's Age is the child's age (in years) as of November 2018.
*** p<0.01, ** p<0.05, * p<0.10.

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Table 2: Treatment Effect (Extensive Margin) on Attendance

	(1) Attended	(2) Attended	(3) Attended
Treatment	-0.0161 (0.0557)	-0.0145 (0.0491)	-0.0135 (0.0493)
Female			0.0394 (0.0498)
Spanish			0.0652 (0.0648)
Child Age			0.0240 (0.0409)
Constant	0.572*** (0.0394)	0.680*** (0.0658)	0.562*** (0.181)
Observations	319	319	319
School FE	No	Yes	Yes
Covariates	No	No	Yes

Note. Robust Standard errors are in parentheses. The outcome is a binary variable indicating whether or not the parent attended at least one event. Female is an indicator for whether the child is female, Spanish is an indicator for whether the household's primary language is Spanish, & Child Age is the child's age in years as of November 2018. *** p<0.01, ** p<0.05, * p<0.10.

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Table 3: Treatment Effect with Alternative Outcome Specifications

	(1) Attended (Alt)	(2) Attended (Low)	(3) Attended (High)
Treatment	0.0634	-0.0385	0.0883*
	(0.0471)	(0.0485)	(0.0480)
Female	0.0141	0.0358	0.0177
	(0.0482)	(0.0491)	(0.0489)
Spanish	0.0530	0.0663	0.0520
	(0.0619)	(0.0624)	(0.0641)
Child Age	0.0433	0.0248	0.0425
	(0.0382)	(0.0401)	(0.0391)
Constant	0.540***	0.589***	0.513***
	(0.169)	(0.177)	(0.174)
Observations	319	319	319
School FE	Yes	Yes	Yes
Covariates	Yes	Yes	Yes

Note. Robust Standard errors are in parentheses. All outcomes are a binary variable indicating whether or not the parent attended at least one event. The outcome in column 1 imputes "1" for all missing outcome data, as opposed to the 0 that was imputed in the regression in Table 2. Columns 2 and 3 impute missing data such that column 2 produces a lower bound on the treatment effect, and column 3 an upper bound. Specifically, column 2 imputes a 0 for all missing treatment observations and a 1 for all missing control observations, and column 3 does the reverse. Female is an indicator for whether the child is female, Spanish is an indicator for whether the household's primary language is Spanish, & Child Age is the child's age in years as of November 2018.
*** p<0.01, ** p<0.05, * p<0.10.

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Table 4: Treatment Effect (Intensive Margin) on Attendance

	(1) Attendance Rate	(2) Attendance Rate
Treatment	0.0356* (0.0195)	0.0700** (0.0270)
Female	0.0249 (0.0203)	0.0326 (0.0271)
Spanish	0.0548** (0.0264)	0.0469 (0.0345)
Child Age	0.00862 (0.0142)	0.00496 (0.0190)
Constant	0.144** (0.0629)	0.241*** (0.0817)
Observations	319	180
School FE	Yes	Yes
Covariates	Yes	Yes

Note. Robust Standard errors are in parentheses. The outcome is the attendance rate for each parent, computed as the percentage of all 8 (or 7) events attended by that parent. The regression in column 1 includes all observations, whereas the regression in column 2 conditions the regression on the parent attending at least one event. Female is an indicator for whether the child is female, Spanish is an indicator for whether the household's primary language is Spanish, & Child Age is the child's age in years as of November 2018.
*** p<0.01, ** p<0.05, * p<0.10.

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Table 5: Treatment Effect on Attendance at Unincentivized Events

	(1) Unincentivized Attendance Rate	(2) Unincentivized Attendance Rate
Treatment	0.0653*	0.0694*
	(0.0359)	(0.0362)
Female		-0.00164
		(0.0369)
Spanish		-0.00124
		(0.0360)
Child Age (Years)		0.0328
		(0.0313)
Constant	0.0825***	-0.0176
	(0.0231)	(0.138)
Observations	211	211
School FE	No	Yes
Covariates	No	Yes

Note. Robust Standard errors are in parentheses. The outcome is the attendance rate for each parent at events that were not financially incentivized by the program, after the treatment ended. These regression only include the 211 parents at the 4 centers that had these additional events. Female is an indicator for whether the child is female, Spanish is an indicator for whether the household's primary language is Spanish, & Child Age is the child's age in years as of November 2018.
*** p<0.01, ** p<0.05, * p<0.10.

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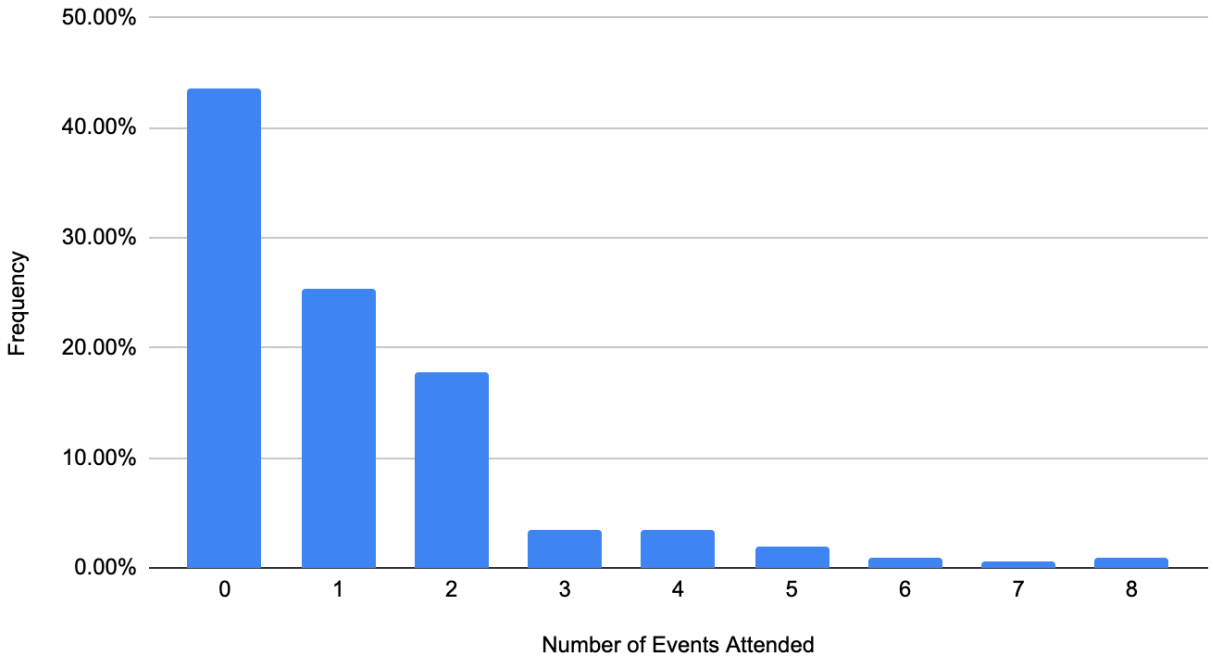
Figures

Figure 1: Distribution of Total Events Attended



Note. The y-axis shows the proportion of all parents (N=319) who attended that number of events, out of 8 total. The modal parent attended zero events, and the median parent attended 1 event.

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