The Schooling Decision: Family Preferences, Intergenerational Conflict, and Moral Hazard in the Brazilian Favelas

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Abstract

This paper analyzes the schooling decisions of poor households with adolescent children in urban Brazil using a high-stakes framed field experiment. Parents in our study were being paid large monthly transfers by the local government conditional upon their 13 to 15 year-old child attending school. We elicit parents’ choices between such conditional monthly payments and guaranteed, unconditional monthly payments of varying relative sizes. In the baseline treatment, an overwhelming majority of parents are willing to forego large, guaranteed sums to keep the current conditionality on the transfers they receive. However, parents reveal much weaker preferences for the conditionality if their child is not informed that the conditionality has been dropped or if they are offered to receive cell-phone text messages whenever their child misses school. We conclude that parent-child conflict plays a crucial role in these schooling decisions, with most parents being unable to control their child’s school attendance behavior, in particular due to lack of observability of the child’s actions. Further experimental treatments indicate that parental demand to control that behavior is not just to provide the child with skills but also to keep the child safe and off the streets.

JEL Classification: C79, C92, C93, D13, D19, J13

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1 Introduction

Schooling decisions, arguably among the most important choices in a person’s life, occur largely while the person is still a child and living with her parents. However, economic models typically view intrahousehold dynamics and, particularly, parent-child conflict as a secondary element in the process. This paper examines the extent to which parent-child conflict and intrahousehold agency problems play a central role in actual schooling decisions of poor households with adolescent children in urban Brazil.

To study these issues, we look directly inside the household “black box”. We analyze experimentally the preferences of adolescents and parents in poor urban Brazil and the decision-making process that leads to actual schooling choices. We model the schooling decision as a moral hazard problem between a parent and her child, in which the child is the agent of the decision. The parent values schooling more than the child does but cannot perfectly observe school attendance behavior. As a result of imperfect observability, the parent’s ability to incentivize her child to attend school is reduced. The parent will therefore be willing to pay for devices that either increase the incentives for the child to attend school or improve monitoring of the child’s actions, thus attenuating moral hazard.

We use a novel experimental approach to elicit preferences and understand the informational structure within households with adolescent children in slums surrounding the city of Brasilia, Brazil. To incentivize the questions, we use the set-up of the existing local conditional cash transfer (CCT) program, *Bolsa-Escola Vida Melhor* (roughly, “School Stipend, Better Life”). In the program, families with monthly per capita household incomes less than R$ 100 (approximately US$ 50) receive a transfer of R$ 120 per month conditional upon one of their children attending school 85% of days in that month.

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2The standard approaches consider either a single decision-maker, as in human capital theory (following Becker, 1964), parents making the decision for their children (as in Becker, 1981), or dynasties with unified utility functions (following Barro, 1974). A few models consider intergenerational conflict in the analysis of schooling choices, generally viewing the parent as the agent making the decision who fails to fully internalize the child’s benefit from schooling (see, for instance, the literature on child labor decisions, e.g. Baland and Robinson, 2000).

3Different from other CCT programs, the one we analyze has a strong concern for enforcing the conditionality, which includes random visits of government officers to schools to enforce the compliance to the rules.
We estimate parents’ willingness to pay to keep (or drop) the conditionality by offering them the opportunity to switch to unconditional monthly transfers of varying sizes delivered in the same manner as their current conditional payments. We randomly change across parents the conditions under which their choices are made. We vary the informational structure within the household—whether the child will know about the change in the program, and how often the parent is informed of the child’s attendance—as well as the form of the conditionality tied to the payment—whether the child has to be in class or just at school. Five percent of the subjects had one of their choices chosen to be their actual payments from the local government between September and December 2009 for that child. By analyzing parents’ choices under these high stakes, we elicit their preferences for schooling, the relevance of moral hazard problems inside the household, and their relative valuation of different benefits provided by school (such as safety, pure signaling, and of course, skills acquisition).

The results from the baseline experiment and two treatments point to a moral hazard problem between children and parents. In the Baseline treatment, parents are asked to choose between their current CCT program and unconditional payments of varying relative sizes. They are told that their children would be informed of any program change. The vast majority of these parents (over 80 percent) prefer to keep the conditionality even if the unconditional transfers pay strictly more. Furthermore, on average, they are willing to forego the equivalent of more than one third of their monthly per capita income to keep the conditionality for one month.

We analyze the determinants of the preference for the conditionality by means of two treatments. In the Don’t Tell treatment, we offer parents the same menu of choices as in the Baseline treatment, but parents are told that their children will not be informed about their choice, or that parents were even offered such a choice. We observe a substantial drop in the number of parents preferring conditional payments in this group, relative to the baseline. This suggests that child control is an important determinant of parental demand for conditionality and that preferences for schooling may differ across generations in the studied households.

In the second treatment, the Text message treatment, we address the issue of information and monitoring in the household. Before eliciting parents’ demand for the conditionality, they are offered the option of receiving free cell phone text messages every time their child misses school, regardless of parents’ choices between conditional and unconditional cash transfers. In this treatment, the child is
aware of the parents’ choice. We observe again a significant decrease in the number of parents preferring to keep the conditionality, relative to the baseline. When parents are given the ability to perfectly monitor their children, the vast majority find the conditionality to be unnecessary.

These treatments provide evidence that, unlike conventional analyses of schooling decisions, family conflict can play a crucial role: parents are willing to pay substantial sums to have a device to control the actions of their child and increase the probability of their school attendance. Furthermore, the results suggest that under-investment in education in those households would be more a consequence of the child’s lack of interest in schooling rather than the parent’s decision to keep the child out of school.

Under the same experimental set-up, we also study parents’ valuation of different components of schooling to understand what drives parental demand to control school attendance. If conditional cash transfers increase the probability of the child attending school, the willingness to pay for the conditionality is also willingness to pay for schooling. The results from our baseline treatment indicate a high parental valuation of schooling. However, it is unclear the extent to which this valuation is driven by skills acquisition in school, the value of having a school degree (such as labor-market signaling, as in Spence (1973), or more generally as social promotion), or the non-classroom content of schooling (such as keeping the child off the streets). To separately identify the importance of these factors, we proceed to three additional treatments.

The Non-classroom treatment exploits the fact that public schools in Brazil offer a student classes during either a morning or an afternoon shift, but not both. In this treatment group, parents were offered the same choice between conditional and unconditional payments. The conditionality, however, is now on the child being in school during the shift in which she has no classes, with no obligation to attend class. The requirement for the no-class shift is just to be within the school limits. There are no classes being offered, but the child can do whatever she likes, like playing sports. The child is monitored by a school guard. This treatment therefore incentivizes the child to be at school (and off the streets) but removes the conditionality on classroom attendance.

The second treatment, the Signaling treatment, offers the same choices as the Non-classroom treatment but additionally guarantees the parent automatic promotion of the child to the next grade in school in case the parent chooses conditional payments (without the child’s knowledge). This treatment main-
tains the non-classroom and the signaling/social promotion contents of schooling. However, it does not impose any conditionality on skills acquisition. In both the Non-classroom and Signaling treatments, the willingness to pay for the conditionality remains high, indicating parents substantially value both the non-classroom and signaling contents of schooling.

Finally, we implement another treatment, the All components treatment. This treatment is identical to the baseline but the conditionality guarantees automatic promotion of the child to the next grade in school (again, without the child’s knowledge). The goal of this treatment is to provide the child with all three analyzed components of schooling. It incentivizes the child to be at school, in class, while guaranteeing automatic promotion. The effects of this treatment are almost identical to the baseline, suggesting that parents believe that their current conditional cash transfer program already provides a high likelihood of promotion to their child.

These results have important policy implications, particularly in developing countries such as Brazil where educational attainment and school attendance are very low. In 2008, 10% of the Brazilian population were illiterate, and the average number of years of schooling was 7.1 (PNAD, 2008). This could lead an observer to believe, based on the standard model of schooling, that poor parents do not value education (despite the very high levels of returns to schooling in that country) or cannot afford to send their children to attend school because they need the children to provide resources for the household). Our results suggest that there might be a further reason behind these unfortunate facts in the developing world: agency issues in the household. Our findings suggest that providing parents with improved information and monitoring on their children’s actions could be an effective policy option to increase school attendance. These findings are consistent with recent work in the literature. Jensen (2009) provides evidence that in the Dominican Republic the perceived returns to schooling by eighth grade students are significantly lower than the actual returns. Moreover, when provided with information on actual returns to schooling, the least-poor students in Jensen’s analysis were significantly less likely to drop out of school in subsequent years. Attanasio and Kaufmann (2009) use

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4The average wage of someone with a high-school (university) degree in Brazil is 116 (340) percent higher than that of someone with no schooling (PNAD, 2007).

5See for instance Basu and Van (1997), and Basu and Tzannatos (2006).

6The 2006 Brazilian National Household Survey (PNAD) asked 15-17 year-old adolescents about their main reason for not attending school: 39.1% reported their pure own lack of interest in going to school, 20.7% mentioned to be working or looking for a job, 3.7% reported to have to help at home, and only 1.5% reported that they were prevented from attending by their parents.
data from a Mexican household survey and provide evidence that mothers have significantly higher expectations on returns to high school than their children in junior high school do.

This paper also relates to several recent empirical studies on household decision-making, especially in the context of developing countries. Berry (2009) provides evidence of a differential impact of incentives for learning depending on whether the recipient is the parent or the child. This effect is consistent with the existence of a parent-child, two-sided moral hazard problem in the household education production function. Ashraf (2009) looks at the effect of spousal observability, communication and control on financial choices of married individuals in the Philippines. Our paper adds to the literature by providing direct experimental evidence on the importance of intergenerational agency issues, in particular with respect to the schooling decision.

On the theoretical side, this paper relates to the literature on intergenerational incentives within the family. Many of the current models can be traced back to Becker’s (1974) “Rotten Kid Theorem”. The famous result is that, absent informational asymmetries, an altruistic parent can control her child’s actions indirectly through transfers if the child’s actions affect the level of household income. The result does not necessarily hold under assumptions of moral hazard (Bergstrom, 1989, Weinberg 2001, and Gatti, 2005). The fact that the vast majority of parents in our setting want to pay for a device to induce their child to go to school provides evidence that the “Rotten Kid Theorem” does not hold for the schooling decision in the environment we study. However, our results also indicate that conditional cash transfers such as the ones under the Bolsa-Escola program might reestablish the conditions for the theorem to hold and that parents’ beliefs in our study are consistent with the theorem.

Finally, our paper adds to the literature on conditional cash transfers. Conditional cash transfer

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7 Several of them address decision-making processes across spouses and genders, such as Duflo (2003), Duflo and Udry (2004), and Rangel (2006). Some papers have also addressed empirically or experimentally issues on intergenerational decision-making, such as Li et al. (2008).

8 Furthermore, Becker’s theorem is limited to cases in which the utility of the parents and children are entirely driven by monetary outcomes, as shown by Bernheim et al. (1985). Banerjee (2004) provides a review of alternative ways to model education decision-making by families.

9 For the impact of such programs on school attendance, see Parker, Rubalcava and Teruel (2008), Schultz (2004), Bourgignon et al. (2003), Glewwe and Olinto (2004), Finan and Bobonis (2009), Angelucci, De Giorgi, Rangel, and Rasul (forthcoming), and specifically for Brazil, de Janvry, Finan and Sadoulet (2007), and Glewwe and Kassouf (2008). For the effect on child labor, see Cardoso and Souza (2004). For the impact of incentivizing the child directly, see Kremer, Miguel and Thornton (2004), Angrist and Lavy (2009), and Jackson (2008). For indirect effects on the consumption of non-eligibles’ consumption, see Angelucci and De Giorgi (2009). For the analysis of the effect variations on the design of a conditional cash transfer program and intrahousehold externalities of the program, see Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2008).
programs are a widespread phenomenon in developing countries: in 2009, over thirty countries currently adopt this type of social program. The literature usually values conditional cash transfers to the extent that they achieve more and better-targeted redistribution when compared to an exclusive public goods provision policy\(^\text{10}\) or because they might commit parents to keep their children in school.\(^\text{11}\) We extend the literature by looking at the family’s demand for the conditionality and find an alternative rationale for adopting CCT programs: parents actually prefer conditional to unconditional payments because the conditionality provides parents with the ability to induce school attendance by their children.

The remainder of the paper is organized as follows: in Section 2, we present background on public education and conditional cash transfers in Brazil. In Section 3, we introduce our theoretical framework. We present our experimental approach in Section 4. In Section 5, we present the results from our experimental treatments, and Section 6 presents our results on the measurement and decomposition of parental valuation of education. Section 7 concludes.

2 Public education and conditional cash transfers in Brazil

Education is compulsory in Brazil for children aged six to fifteen, but the law is loosely enforced. In fact, according to the 2006 Brazilian National Household Survey (PNAD), over 9% of fourteen year-old children from the bottom quartile of the distribution of household per capita income report not being enrolled at the time of the survey. This hides an even larger attendance problem, since only enrollment is compulsory in Brazil. Large numbers of children drop out of school during the school year and re-enroll in the following year as required by law (de Janvry et al, 2007).

The problem of school attendance in Brazil is particularly acute for poor children aged thirteen to fifteen. Although working is only legal at the age of sixteen, over 15% of fifteen year-old children from the bottom-quartile households in the income distribution were not enrolled in school in 2006, and over 22% of them reported to have a job during the week they were interviewed for the 2006 PNAD.

Since 1995, both local and federal governments in Brazil have implemented different conditional

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\(^\text{10}\)See Gahvari and Mattos (2007), following arguments made by Besley and Coate (1992) and Zeckhauser (1971).

\(^\text{11}\)See the World Bank report by Fiszbein et al. (2009) for a summary of the arguments in favor of conditional cash transfers. See de Janvry and Sadoulet (2006) for a discussion of unconditional versus conditional cash transfer programs.
cash transfer (CCT) programs aimed at reducing income inequality and increasing school attendance. The idea of those programs is to make payments to families that meet some eligibility criteria (typically having a low level of per capita income in the household) but only if they meet some conditionality (usually a minimum level of monthly school attendance of their children). The first such program was Bolsa Escola, which was introduced in 1995 in the Distrito Federal state, which surrounds the Brazilian capital, Brasilia. It has been running with a few changes ever since then (it was renamed Bolsa-Escola Vida Melhor in 2009). In 1998, the federal government implemented the Bolsa-Escola program nationwide. In 2003, the federal program was redesigned and renamed Bolsa-Família, targeting poor families with children aged six to fifteen.

The available evidence suggests that the federal program has indeed stimulated schooling among its beneficiaries. De Janvry et al (2007) estimate that in 261 municipalities in the Northeast of Brazil the program reduced dropout rates by 8% on average. They estimate that if the beneficiary children were not in the program, the dropout rate would have been 12% instead of the 4% currently observed among them. The program is thus estimated to have induced a 66% decline in dropout. The impact on school attendance rates is likely to have been higher since enrollment itself is compulsory (and therefore very high) regardless of the program.

Our analysis focuses on the Bolsa-Escola program in the areas surrounding Brasilia, and which, in 2009, is still administered separately by the local government.

At the time of our study, the eligibility criterion for the Bolsa-Escola program was a monthly household per capita income less then R$ 100 (about US$ 50). Under this CCT program, the mother of a beneficiary household receives R$ 120 per month if one child between the ages of six and fifteen attends a minimum of 85% of classes that month.\(^\text{12}\) If the child misses more than 15% of the classes in any month (unjustified absences), payments are suspended for the next month onwards. Absences are reported by teachers to the school principals and from them to the local government. The program has a strong concern for enforcing the conditionality. The local government does random visits schools to enforce the compliance to the rules. If the family has more than one child within this age range, they receive R$ 120 per month for the first child, R$ 30 for the second, and R$ 30 for the third. The maximum payment per month is R$ 180 per family.

\(^{12}\)If the household has no mother, the payment is made to the father or another adult responsible for the children.


3 Theoretical framework

To organize our analysis, we develop a simple model of the schooling decision in the household, which is composed of one child and one parent in a one-shot sequential game.

3.1 The model

In the game, the child decides whether or not to go to school \((S = 1,\) if she goes, and \(S = 0\) otherwise). If the child goes to school, the parent observes it with certainty. If the child misses school, the parent observes the child’s action with probability \(\pi\).\(^{13}\)

3.1.1 Sequence of play

1. The parent announces a binding contract before the child chooses \(S\) with an income transfer \(x_0\) if the parent observes that the child misses school, and \(x_1\), otherwise. We assume the parent is able to commit to an ex-ante optimal announced contract.

2. The child decides whether or not to go to school.

3. Transfers \(x_0\) and \(x_1\) are made from the parent to the child. We assume that the child consumes the transfer entirely on a private good of price one, and that the parent allocates what is left from household income \(Y\) after the transfer to the consumption of the same good.

3.1.2 Preferences

For simplicity, we assume that the child’s utility, \(U_{\text{child}}\), is linear in the consumption of the private good. The child’s utility has an additional term in case she attends school: her net benefit from going to school. This benefit corresponds to the difference between the child’s perceived gross benefit from attending school \((V_{\text{child}})\) and her perceived cost \((\gamma)\). We assume that this difference is negative so the child faces a net private cost of attending school, \(c \equiv \gamma - V_{\text{child}} > 0\). As we will see in the results section, one of our experimental treatments provides evidence consistent with this last assumption.

\(^{13}\)We assume that lack of observability only occurs when the child misses school because we think this assumption is more realistic than supposing lack of observability in both school attendance and absence actions. We believe that the child is more likely to try to hide from her parent that she missed school than that she went to school. Our results hold if we assume lack observability in both actions instead.
Letting $I_{S=j}$ be the indicator that the child chooses $S = j$, we can write the expected utility of the child as:

$$E[U_{child}] = I_{S=1} \{x_1 - c\} + I_{S=0} \{\pi x_0 + (1 - \pi)x_1\}$$  \hspace{1cm} (1)$$

The child will choose to go to school if (child’s incentive compatibility constraint):

$$x_1 - x_0 \geq c/\pi$$  \hspace{1cm} (2)$$

The parent is assumed to be altruistic toward her child, but discounts her child’s utility by $\alpha \in [0, 1)$. Finally, we assume that the parent derives a net private benefit $V > 0$ from her child going to school. One reason why the parent faces a net private benefit of the child attending school while the child faces a net cost could be that the child faces a higher private cost of attending school than the parent (which could be combined with a lower perception of the gross benefit of attending school by the child).\(^{15}\)

One of our experimental treatments provides evidence consistent with the assumption that $V > 0$.

We can write the parent’s utility expected utility as:

$$E[U_{parent}] = I_{S=1} \{(Y - x_1) + \alpha [x_1 - c] + V\} + I_{S=0} \{\pi[(Y - x_0) + \alpha x_0] + (1 - \pi)[(Y - x_1) + \alpha x_1]\}$$

\hspace{1cm} (3)$$

Hence, if the child goes to school ($S = 1$), the parent pays $x_1$ to the child and derives $V$ from her child attending school. The child in this case pays her private cost of school attendance $c$. If the child does not go to school ($S = 0$), with probability $\pi$ the parent pays $x_0$ to the child, and with probability $(1 - \pi)$ the parents pays $x_1$. In either case, the parent does not derive benefit from school attendance and the child does not pay the cost of school attendance.

### 3.1.3 Equilibrium outcomes - Does the child attend school?

We now solve for the equilibrium outcomes, working backwards. We compute the optimal transfers by the parents for the two possible choices of the child ($S = 1$ and $S = 0$) assuming that the child is

\(^{14}\)We impose that the parent is not perfectly altruistic because $\alpha = 1$ would imply that the parent is indifferent between her own and her child’s consumption, leaving no role for the provision of incentives through contingent transfers to the child.

\(^{15}\)In our results section, we provide evidence that in our sample children have significantly lower perceptions of the returns to schooling than their parents.
behaving optimally. We first note that if $Y < c/\pi$, the parent is never able to meet the child’s incentive compatibility constraint and therefore the child never goes to school. We now look at the cases in which $Y \geq c/\pi$.

The maximum (expected) utility level the parent can achieve if the child does not go to school and is behaving optimally (i.e. $S^* = 0$) is derived by solving the following problem:

$$\max_{x_1, x_0} \{ \pi [Y - (1 - \alpha)x_0] + (1 - \pi) [Y - (1 - \alpha)x_1] \}$$

s.t. $x_1 - x_0 \leq c/\pi$

$$x_0, x_1 \geq 0$$

This implies the optimal transfers of $\{x_0^* | S^* = 0\} = \{x_1^* | S^* = 0\} = 0$. The maximum level of parental utility in this case is therefore $\{U_{\text{parent}*} | S^* = 0\} = Y$.

The maximum utility level the parent can achieve if the child goes to school and is behaving optimally ($S^* = 1$) is found by solving the following problem:

$$\max_{x_1, x_0} \{ Y - (1 - \alpha)x_1 + V - \alpha c \}$$

s.t. $x_1 - x_0 \geq c/\pi$

$$x_0 \geq 0$$

This implies the optimal transfers of $\{x_0^* | S^* = 0\} = 0$ and $\{x_1^* | S^* = 1\} = c/\pi$. The maximum level of parental utility in this case is therefore $\{U_{\text{parent}*} | S^* = 1\} = Y - (\alpha + (1 - \alpha)/\pi)c + V$.

**Proposition 1** Let $\overline{Y} \equiv c/\pi$ and $\overline{V} \equiv (\alpha + (1 - \alpha)/\pi)c$. Then:

1. If $Y < \overline{Y}$, then $x_1^* = 0, x_0^* = 0$ and $S^* = 0$

2. If $Y \geq \overline{Y}$, then:

   (a) If $V \geq \overline{V}$, then $x_1^* = c/\pi, x_0^* = 0$ and $S^* = 1$

   (b) If $V < \overline{V}$, then $x_1^* = 0, x_0^* = 0$ and $S^* = 0$
If the level of household income is below the minimum level of transfers to the child that induces school attendance, the child will not attend school and the parent will not transfer any resources to the child. If the level of household income is above that minimum level, the parent can induce school attendance, but only chooses to do so if it is ex-ante optimal for herself (i.e. \( V \geq \bar{V} \)).

**Proposition 2 The role of monitoring:** \( \partial V / \partial \pi < 0 \)

The lower the probability of observation of the child’s action is, the higher has to be the transfer \( x_1 \) in order to induce school attendance.

### 3.2 The experiment

We offer parents choices between two policy instruments: conditional cash transfers and (unconditional) cash transfers both of varying sizes. Conditional cash transfers are only paid to the parent if the child attends school whereas cash transfers are paid with certainty.

**Definition 1** A conditional cash transfer \( CCT(T_{CCT}) \) is a payment scheme that transfers \( T_{CCT} \) units of income to the parent if \( S = 1 \) and zero otherwise. Therefore, under this scheme, \( \pi = 1 \), since the parent observes with certainty the child’s action.

**Definition 2** A cash transfer \( CT(T_{CT}) \) is a payment scheme that transfers \( T_{CT} \) units of income to the parent for any value of \( S \). Therefore, under this scheme, the parent’s level of observability is kept at \( \pi \).

In our experiment, we want to derive the size \( T_{CT} \) of a cash transfer that would make the parent indifferent to a conditional cash transfer of size \( T_{CCT} \).

**Proposition 3** Suppose \( \pi < 1 \). Hence \( c < \bar{V} \) and the following holds:

1. If \( V \leq c - T_{CCT} \), then \( T_{CT} = 0 \). The child does not go to school in either case, so the parent is strictly better off under a cash transfer of any positive size.

2. If \( c - T_{CCT} < V < c \), then \( T_{CT} = T_{CCT} + (V - c) < T_{CCT} \). The child goes to school under the conditional cash transfer but not under the cash transfer, and the parent is strictly better off with the child not going to school if the parent receives the same level of payments from the two schemes.
3. If \( V = c \), then \( T_{CT} = T_{CCT} \). The child goes to school under the conditional cash transfer but not under the cash transfer and the parent is indifferent regarding school attendance if the parent receives the same level of payments from the two schemes.

4. If \( c < V < V \), then \( T_{CT} = T_{CCT} + (V - c) > T_{CCT} \). The child goes to school under the conditional cash transfer but not under the cash transfer and the parent is strictly better off with the child going to school if the parent receives the same level of payments from the two schemes.

5. If \( V \geq V \), then \( T_{CT} = T_{CCT} + (V - c) > T_{CCT} \). The child goes to school in either case but the parent is strictly better off under the CCT because her level of transfers to the child is lower than under the CT.

**Proof:** see Appendix A.

Therefore, if a parent has \( V > c \), we say that she pays for the conditionality as a monitoring device.

Mapping the two policy instruments (CT and CCT) to our first two propositions, if the goal of the implementer of the instruments is to induce school attendance, then:

1. The minimum size of the transfer of either type of policy has to be \( T \equiv V - Y \).

2. Conditional on meeting the minimum required size of the transfer \( T \), increasing monitoring \( \pi \) in either policy instrument will help promote school attendance.\(^{16}\)

We assume that all values of the transfers offered to the parents in our experiment are greater than \( T \).

In the next sections of the paper, we use a set-up in which parents report their choices between conditional and unconditional cash transfers both of varying relative sizes. We provide evidence consistent with our main assumptions and with Propositions 1 and 2 from the model. In order to map theoretical assumptions and predictions to experimental tests over a population, we assume that the families in our study vary according to parental net private benefit of the child going to school, \( V \),

\(^{16}\)If the child cares about the level of household income, another channel other than increasing monitoring through which conditional cash transfers can induce school attendance is given by the fact that payments to the parent are conditional on school attendance. As an example, if the parent consumes her fraction of household income on a household public good which the child can also consume, then the CCT creates wedge \( a \) in the consumption level of the household public good depending on the child's action, which is internalized by the child, thus increasing her incentives for school attendance. We do not detect this specific channel in our experiment and we also provide evidence that the bulk of the preference for conditionality is due to the monitoring feature of the CCT. All the results and predictions from our framework are kept if we assume a model with household public good consumption as just described.
which we suppose to be symmetrically distributed with mean $\hat{V}$ and cumulative distribution function $\psi(.)$. Therefore, $1 - \psi(c)$ is the mass of parents willing to pay for the conditionality. Two treatments designed to test Propositions 1 and 2 are described in the next section of the paper.

In our experiment, we also implement treatments to understand the relative importance of different components of schooling to parental net valuation of the total benefit generated from her child attending school. We assume that attending school provides three types of benefits:

1. **skills**, the skills acquisition in the classroom (the formal education content);
2. **signal**, the gain from the signaling content of schooling, regardless of the acquisition of skills;
3. **nonclass**, the non-classroom content of schooling, such as the decrease in the exposure to crime, drugs and violence, and the gain from interacting with other children outside of the classroom.

We assume that each of these variables equals one if the child acquires the component of schooling it refers to, and zero otherwise. For instance, if the child only acquires the signaling content of schooling, then $skills = 0, signal = 1,$ and $nonclass = 0$.

If the average in our study is such that $c < \hat{V} < \overline{V}$, then she believes that her child will go to school under the conditional cash transfer but not under the unconditional cash transfer. In that case, we can write:

$$T_{CT} - T_{CCT} = V - c = \{U_{parent}^*|S^* = 1, \pi = 1\} - \{U_{parent}^*|S^* = 0, \pi = 1\} \equiv B(skills, signal, nonclass)$$

The difference between the unconditional and conditional transfer levels is equal to the net utility gain in parental utility from having the child attending school under perfect observability of the child’s action ($\pi = 1$). We write it as a function of the three components of schooling: $B(skills, signal, nonclass)$. In the next section, we describe three treatments designed to disentangle the relative importance of those three components to parental total net valuation of schooling.
4 The experiment

4.1 The basic set-up of the intervention

We surveyed 210 families, all enrolled in the CCT program (*Bolsa-Escola Vida Melhor*).\textsuperscript{17} For each family, we interviewed one parent and her child between thirteen and fifteen years of age. Those families were all enrolled in the program and hence at the time were receiving R$ 120 per month conditional on school attendance of that child to at least 85% of classes each month.

We focused on children between between thirteen and fifteen because we believe that at this age range children have already formed individual preferences, some bargaining power in the household and an outside option to schooling.\textsuperscript{18} This is also the age range at which school attendance drops considerably. According to the official Brazilian household survey (PNAD), by age sixteen, dropout rates reach 26% for children in Brazil with a household monthly per capita income less than R$ 100 (the eligibility criterion for the *Bolsa-Escola* program). Finally, these are also the oldest CCT-eligible students, since payments stop when the child turns sixteen.

Families invited to the experiment were randomly chosen among those enrolled in the *Bolsa-Escola* program. First, a district was randomly chosen within all of the school districts in Brasilia. Second, within each chosen district, a number of schools were randomly chosen. Finally, within each chosen school, a number of students were randomly chosen. We interviewed families from eleven schools in four of the existing twenty districts in the *Distrito Federal* state (for a discussion on the representativeness of our sample, see Appendix B).

We also only included children who had no older CCT-eligible siblings to ensure that the invited families were been paid R$ 120 for school attendance of the invited child. Families were recruited with letters distributed to the child by their school’s principal on Thursday or Friday, inviting them to attend a one-hour study at the child’s school over the weekend. Families were randomly offered either R$ 7 or R$ 10 to attend the study, with the potential to earn more. The average show-up rate in our study was 87% (see Appendix B for details on show-up rates).

When participants arrived, each family was randomly assigned into one of the treatments de-

\textsuperscript{17}The agreement with the local government was made possible with the help of the local NGO *Missao Crianca*.

\textsuperscript{18}In the whole country, over 22% of fifteen year-old children from the bottom quartile households in the distribution of household per capita income reported to have a job during the week they were interviewed for the 2006 PNAD.
scribed in the next subsection. The randomization was based on the last two digits of the parent’s Cadastro de Pessoa Fisica (akin to a Social Security Number in the United States). The parent was seated at a computer, and the student was asked to wait. If there were no free computers, the parent would wait as well. One surveyor was assigned to each participant to answer questions, and, in some instances, read the questions for them, and assist them to in using the computers. In every treatment, parents would complete their portion of the experiment, then the children. In some cases described in the next subsection, there was a joint decision-making portion, which followed the children’s part.

The experiment began with the surveyor offering the parent the opportunity to choose a new cash transfer program. The parent answered twenty-five questions, each one a choice between a cash transfer conditional on a behavioral outcome of their child (e.g. like their current CCT program) or an unconditional transfer, also paid monthly in the same manner. Each treatment varied the specifics of the conditionality or the informational features of the choices, but the structure and sequence of the questions was always the same. Each question varied the relative size of the conditional and the unconditional transfers. That is, subjects were offered series of binary choices—a CCT worth R$ X or a CT worth R$ Y—and X and Y were varied for each choice.

The minimum amount was always R$ 120, ensuring the family could not leave with a transfer that paid less than their original one. The first screen held constant the amount of the CCT at R$ 120 and increased the CT from R$ 120 to R$ 180 in increments of R$ 5, as presented below:

<table>
<thead>
<tr>
<th>Which Monthly Payment Would Your Prefer?</th>
</tr>
</thead>
<tbody>
<tr>
<td>R$ 120 Conditional on Attendance OR R$ 120 Unconditionally</td>
</tr>
<tr>
<td>R$ 120 Conditional on Attendance OR R$ 125 Unconditionally</td>
</tr>
<tr>
<td>...</td>
</tr>
<tr>
<td>R$ 120 Conditional on Attendance OR R$ 180 Unconditionally</td>
</tr>
</tbody>
</table>

The second screen held constant the amount of the CT at R$ 120 and increased the CCT from R$ 125 to R$ 180 in increments of R$ 5, as presented below:

<table>
<thead>
<tr>
<th>Which Monthly Payment Would Your Prefer?</th>
</tr>
</thead>
<tbody>
<tr>
<td>R$ 120 Conditional on Attendance OR R$ 120 Unconditionally</td>
</tr>
<tr>
<td>...</td>
</tr>
<tr>
<td>R$ 120 Conditional on Attendance OR R$ 180 Unconditionally</td>
</tr>
</tbody>
</table>

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19 If there was a long wait, subjects would play BINGO for small prizes.
20 Surveyors were all undergraduate students from the University of Brasilia.
21 It is possible this ordering may have an effect within one treatment, but assuming this does not interact with treatment effects, this will not affect the analysis across treatments.
Which Monthly Payment Would You Prefer?

<table>
<thead>
<tr>
<th>R$ 125 Conditional on Attendance OR R$ 120 Unconditionally</th>
</tr>
</thead>
<tbody>
<tr>
<td>R$ 130 Conditional on Attendance OR R$ 120 Unconditionally</td>
</tr>
<tr>
<td>...</td>
</tr>
<tr>
<td>R$ 180 Conditional on Attendance OR R$ 120 Unconditionally</td>
</tr>
</tbody>
</table>

Each treatment used these same twenty-five conditional versus unconditional transfer questions. Parents were asked to report their preferred choices for each of the twenty-five questions and were informed that 5% of participants would have one of their decisions implemented and that the decision would be randomly chosen from all of their decisions. Any change would last through the end of the current school year, for four months (from September to December 2009) and would only apply to the child present at the experiment.

All sessions were performed between July and September 2009. The experiment was conducted at computer terminals using a web-based survey. Subjects were not allowed to interact with each other in the computer lab. No communication within or across families was allowed during the entire experiment. For each family, total participation took no longer than one hour.

### 4.2 Experimental treatments

Table 1 reports the summary and comparison of all treatments.

#### 4.2.1 Baseline Treatment

The sixty-one families in this group made the sequence of choices just described in which the conditional transfers has exactly the same conditionality as in the Bolsa-Escola program: class attendance on 85% of the days every month. The choices were offered first to the parent, then the child, then jointly,

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22See the actual set of twenty-five questions in Portuguese used in the Baseline treatment group attached to the end of this document.

23Individuals were informed that they would face twenty-five binary choices. This is the Becker DeGroot Marschak elicitation procedure, which incentivizes truthful reporting of willingness to pay.

24We performed an experimental pilot with thirty-five families in March-April 2009 mostly focused on surveying the families, with no treatments. A discussion on the pilot experiment design and results can be found in Appendix C.

25The experiment was performed using Qualtrics’ web-based survey platform.
but were only financially incentivized for the parents.\textsuperscript{26} The child was informed of the choices made by the parent. This is our baseline treatment group, since it regards choices over the program the families were enrolled in. This treatment enables us to estimate $1 - \psi(c)$, i.e., the mass of parents willing to pay for the conditionality.

4.2.2 Do parents need a device to control the child? \textit{Don’t Tell Treatment}

This treatment tests Proposition 1 of the model. We know that under the model the minimum required level of parental net private valuation of schooling $V$ under a CCT with payment $T_{\text{CCT}}$ is $c < V$, where the second term is the minimum required level of $V$ under a CT that pays the same amount and has the same monitoring level.

This treatment, assigned to forty-seven families, is identical to the baseline except the CCT-CT question for the parents was preceded by a short disclaimer saying that we would not tell the child if their transfer program was changed, and that the child would not be offered a CCT-CT decision.\textsuperscript{27} Thus, the children would not have any reason to believe they are leaving with anything other the CCT program with which they came. Therefore, if the parent chooses a CT now, she will have a lower minimum required level of parental net private valuation $c < V$; whereas, under the \textit{Baseline} treatment, she would have $V$. If the parent pays for the conditionality, it is because $V > c$. Therefore, according to Proposition 1, shifting $V$ to $c$ should eliminate the willingness to pay for the conditionality. It follows that $1 - \psi(c)$ should go to zero, unless some parents believe they are unable to hide from their child the change from the CCT to the CT.

In summary, if parents believe that the conditionality induces school attendance and want the CCT as a device to commit the child to attending school, then when offered to choose between conditional and unconditional cash transfers, parents could choose the larger of the two transfers and allow the child to believe the transfers are still conditional on attendance.

\textsuperscript{26}We do not present the results from the joint and child decisions in the sections of the paper. The analysis of those decisions can be found in Appendix D.

\textsuperscript{27}In this treatment, the child would only answer questions about demographics, preference parameters, etc.
4.2.3 Monitoring and parental control: Text message Treatment

This treatment is designed to address Proposition 2 of our model. If monitoring is a problem in the household, then providing the child with a level of monitoring $\pi$ comparable to the one provided by the CCT should decrease the need to pay to keep the conditionality on the payments. It follows that $1 - \psi(c)$ should go to zero, unless some parents believe the CCT still provides more monitoring.

This treatment, assigned to fifty-two families, is identical to the baseline except the CCT-CT question for the parents was preceded by offering the option of receiving a free text message sent to their cell phone every day their child misses class. Parents were greeted with a screen offering them the free service and asking them for their cell phone number if they would like to sign up.\textsuperscript{28} The rest of the experiment proceeded identically to the baseline.

The three subsequent treatments are designed to understand the driving forces of parental net total valuation of schooling, by analyzing separately the preference for conditionalities tied to the acquisition of different components of schooling.

4.2.4 School as a bunker: the Non-classroom treatment

This treatment addresses parental valuation of the non-classroom content of school, such as keeping the child off the streets.\textsuperscript{29} Children only attend a half-day of classes in Brasilia, either in the morning (from eight to twelve) or the afternoon (from two to six). They are welcome (but not required) to attend school during the other half, but do not have any classes. In this treatment, to which fifty-three families were randomly assigned, parents were asked to choose between a cash transfer conditional on their child’s attendance at school in the half of the day they are not in class (still at 85%) and unconditional payments. These choices are binary decisions, identical in sequence and monetary value to those in the other treatments. For these decisions, the parents were told there was no conditionality regarding attending classes. This means that if the child misses every day of class, the parent will still be paid the transfer so long as the child attends the other half of the day. There are no requirements regarding

\textsuperscript{28}Only two parents in this treatment did not have a cell phone. Everyone else accepted the offer.

\textsuperscript{29}Katz et al (2001) report that 53% of disadvantaged parents who applied for housing vouchers did so to “get away from drugs and gangs” while only 18% were driven by the opportunity for “better schools for [their] children.” Kling et al (2005) further show that the child’s neighborhood may be a good predictor of whether the child is involved in crime. Goldin and Katz (2008) provide historical evidence that child labor laws generally exempted older youths, constrained by the compulsory schooling law, but only conditional on them going to work.
the activities performed in school during the no-class shift. There are no classes and the child can do whatever he or she wants, as long as it occurs inside the school limits. The child’s attendance during the no-class shift is monitored by a school guard. The child is still welcome to attend class during the usual class shift. Children in this treatment group were informed about the choices made by their parents. To make sure parents did not leave with a less preferred cash transfer program than their current program, they were later asked to choose between their current program (i.e. a CCT of R$ 120) and the alternative they had just chosen (but they were not informed that they would be offered that option while they were making their choices between conditional and unconditional payments).

4.2.5 Schooling as signaling/social promotion: the Signaling treatment

This treatment addresses parental valuation of the non-classroom content of school combined with promotion to the next grade, and still no conditionality on class attendance. For that, we ran a follow-up, phone call interview experiment with thirty-one randomly chosen parents that were originally in the Baseline treatment group. Subjects were called and told that the lottery to determine which experimental subject’s CT-CCT decisions would be implemented had not yet been run and that they now, if they wanted, would be offered the opportunity to make new choices, perhaps among different transfer programs they were not offered previously.

The Signaling treatment is very similar to the Non-classroom treatment, except parents were told their child would automatically pass to the next grade at the end of the school year for any conditional cash transfer option they chose, no matter the child’s attendance or performance in school. The child was not informed about the automatic promotion. With this guarantee in place, parents first made the same twenty-five CT-CCT choices, where the conditionality was on the child attending school in the half of the day the child was not in class. At the end, they were asked if they would prefer a transfer program from these twenty-five choices or one of the twenty-five choices they made in the baseline treatment.
4.2.6 Total valuation of schooling: the *All components treatment*

We ran a final phone follow-up experiment with the thirty parents that were originally assigned to the *Baseline* treatment and subsequently not allocated to the *Signaling* treatment. These were offered the same menu of choices as in the *Baseline* treatment, without being informed of their original choice. However, they were also offered guaranteed promotion to the next grade for their child in case they chose the conditional transfer. The child was not informed about the automatic promotion. This treatment therefore addresses parental valuation of the three analyzed components of schooling combined.

In the experiment, we also elicited various other parameters of the participants. Both parents and children answered questions about demographics and intrahousehold dynamics. These parameters were not elicited in an incentivized manner—subjects were simply directly asked. Perceptions on financial returns to schooling and time preferences, however, were elicited in an incentivized fashion (for details on these questions, see Appendix E).

4.3 Experimental Outcomes and Empirical Specification

We are interested in parents’ choices between CT’s and different types of CCT’s. Consequently, we focus on three outcome variables:

1. A dummy variable that is equal to one if the parent prefers a R$ 120 CCT to a R$ 120 CT (*the parent is demanding the conditionality*), and zero otherwise.

2. A dummy variable that is equal to one if the parent prefers a R$ 120 CCT to a CT that pays strictly more (*the parent is paying for the conditionality*), and zero otherwise.

3. The level of CT payment that makes the parent indifferent to a R$ 120 CCT (*the parent’s threshold*).

To estimate the effects of the treatments described previously, we make mean comparisons of these three outcome variables across treatments. Although the assignment to treatments was random, we estimate treatment effects controlling for observables. To that end, we use each one of these three measures separately as our dependent variables and run the following OLS regression in our empirical analysis:

\[ Y_i = \alpha + \nu X_i + \phi_1 I_{Don't \ tell,i} + \phi_2 I_{Text \ message,i} + \phi_3 I_{Non-classroom,i} + e_i \]
where $Y$ is one of the three measures mentioned above, $X$ is a vector of controls, and $I_j$ are the dummies for whether the parent received a treatment other than the Baseline treatment: the Don’t tell treatment, the Text message treatment, and the Non-classroom treatment. Therefore, the treatment dummies measure the effect of each treatment compared to the baseline. In our main specification, we do not look at the effect of the Signaling and All components treatments, since they were implemented as follow-up treatments. In estimating these last two treatment effects, we run a new set of regressions in which the observations from the Signaling and All components treatment groups appear twice: once as part of one of these two treatment groups and once in the Baseline treatment. That second specification has therefore five different treatment dummies and has sixty-one observations placed twice.

In our main specifications, we include the following covariates: treatment dummies, marital status (parent), log of household income, male indicator (parent and child), age (parent and child), employed parent indicator, employed parent spouse indicator, religion dummies, schooling (parent and child), number of children, beta [a measure of time inconsistency discount factor] (for the parent and her child), delta [weekly discount factor] (parent and child), afternoon school shift indicator, race dummies (parent and child), dummy for higher show-up fee, school and surveyor dummies, with the standard errors clustered by school in our baseline specification.

5 Treatment Results

5.1 Summary statistics and motivating evidence

Table 2 presents summary statistics for our variables of interest across treatment groups. With very few exceptions, the means are not significantly different from the baseline group. This suggests that the randomization was successful.

To motivate our analysis of the parent-child conflict, Table 3 presents the means and medians of parents’ and children’s perceptions of current wages the children could earn if they decided to drop out and work instead, and the wage premia from additional years of schooling (and the average yearly returns to schooling). Regarding beliefs of wage premia and returns to schooling, the means between

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30 For a discussion on the construction and measure of time preference parameters, see Appendix E.
31 For robustness, we also reproduced the regressions clustering the standard errors by surveyor and by school*surveyor, and with Probit regressions when the dependent variable was binary. The results are available upon request.
parents and children seem similar (and are insignificantly different), due to two children reporting very high expectations.\textsuperscript{32} The medians however are highly significantly different. We only have measures of the perceived \textit{return}s to schooling, however. We do not have measures of the perceived \textit{cost} of schooling for parents and children. Since the child is the agent actually going to school, it seems plausible that she might have a higher private cost of attending school. This would exacerbate the divergence in preferences within each household. Unfortunately, we cannot address this concern.

We also present in Table 3, the means and medians of both parents and children’s beliefs about the average wage of someone with a high-school or college degrees, together with the actual empirically observed average in Brazil (using the data from PNAD 2007 and updating the values using the Brazilian consumer price index for 2007 and 2008). As we can observe, the parents surveyed are not mis-estimating the actual wage levels in Brazil.\textsuperscript{33}

Finally, we find evidence that parents are underinformed of their child’s school attendance behavior. Only 5.6\% of the children in the sample report that they commute to school with the company of their parents. In most families, parents have to leave home extremely early in order to be in downtown Brasilia in the morning, to either work or look for a job. Table 4 reports the parent’s and the child’s answers to questions regarding school attendance by the child. First, parents report on average lower school absences by the child than their child. Also, parents are more likely to cite sickness as a reason for absence while the children are much more likely to report that “they missed class because they did not want to go.”\textsuperscript{34}

### 5.2 Treatment effects

In Table 5, we compare the means of our outcome variables across treatment groups. As explained before, we define a parent’s “threshold” as the minimum amount of unconditional transfer that makes the parent indifferent to a conditional cash transfer of R$120. Since the questionnaires only offered the

\textsuperscript{32}If we exclude the two outlier children, for most measures of returns to schooling, the difference is again significant for the means.

\textsuperscript{33}We found no significant differences in time-preference parameters between parents and children. This is consistent with experimental results by Bettinger and Slonim (2007) that show that by the age of sixteen, discount rates are fairly similar between parents and their children. Children in our sample are also on average more educated than their parents, and the literature suggests that more educated people are more patient (e.g. Lawrance, 1991).

\textsuperscript{34}We should not forget that all these children were receiving a conditional cash transfer at the time of the experiments, which could set a bound on the number of classes they could miss.
option up to R$ 180 in either direction (increasing the CCT or the CT), our measure of the threshold is censored. We thus use two threshold measures: one based on the original, censored data (henceforth parents’ “Censored threshold”) and another based on data extrapolated using a Tobit regression of parents’ threshold on the set of covariates of our baseline specification (presented before), henceforth their “Extrapolated threshold”. Details relating to the construction of these thresholds are in Appendix F.

Figures 1-4 show the mean levels of our four outcome variables of interest for each of the treatment groups. Table 5 presents the mean levels of the four outcome variables across treatments. Table 6 presents the results from regressions of the four outcome variables on the set of covariates described in the previous sections. As expected, the estimates of the treatment effects controlling for observables are very similar to the treatment effects derived from the comparison of means across treatment groups without controlling for observables.

5.2.1 Baseline treatment

Within that group, 88.5% of the parents choose a conditional transfer over an unconditional transfer of equal or greater amount (they “demand” the conditionality) while 82% prefer at least one conditional transfer that paid strictly less than an unconditional transfer (they “pay” for the conditionality).

The parent’s average censored threshold is R$ 157.3. This is a lower bound of the true average due to censoring (63.3% of the parents in the Baseline treatment group prefer a CCT of R$ 120 to a CT of R$ 180). That is, parents, on average, are willing to forego at least R$ 37.6 to keep the conditionality. This is over one third of their pre-CCT level of monthly per capita income. The mean level of the extrapolated threshold in this group is R$ 201. Due to our censoring problem, we look at quartiles in the distribution of the censored thresholds: the first quartile is at R$ 127.5 and the median is already at R$ 180. We plot the cumulative distribution for the censored threshold in the Baseline treatment group in Figure 5.

In Table 6, we observe that the show-up fee does not significantly affect the demand for conditionality or the size of the threshold. As the show-up rate to the experiment was already very high for the lower show-up fee (show-up rate of 85%), selection does not seem to be driving our findings (see

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35 We also reproduced our analysis using median regressions to deal with the censoring problem. The results are very similar to the ones reported in the paper and are available upon request.
Appendix B for a discussion on the show-up rates of our study).³⁶

There is also concern that social desirability and experimenter demand effects may be driving our results. Although the respondents are choosing payment schemes under high stakes, they might be tempted to choose what they feel to be socially or experimenter-approved decisions. As we will see in the next subsection, the fact that most subjects did not have a problem reporting that they would be willing to lie to their child (or hide something from them), an implicit result from the Don’t tell treatment already suggests that social desirability might not a main force. Also, the interaction terms in some of the regressions we present in the subsequent sections are informative. If the results were highly affected by social desirability effects, we would not expect the treatment effects to vary according to variables such as district-level crime rates, perceptions of observability of the child’s action, and many other factors, as we find and will in subsequent sections. In addition to that, experimenter demand effects should be absorbed by the surveyor dummies.

In the main specifications, we do not control for beliefs of returns to schooling. If we add the beliefs of average wage premium from an additional year of schooling, the coefficients for the children’s beliefs are significantly negative for three of the four LHS variables (the exception is the demand for conditionality variable, where the coefficient is negative but not significant). If we use instead the difference in the perceptions of average wage premium between each parent and her child, the coefficients are positive for all four LHS measures and significant for the willingness to pay for the conditionality.³⁷

5.2.2 Conditional cash transfers to control the child: Don’t tell treatment

This treatment is aimed at testing Proposition 1 of the model. In particular, if the child believes that the conditionality still holds (and is is therefore incentivized to attend school), the parent should be

³⁶To further address the possibility of selection bias, we used the set-up of a political poll that interviewed random subjects in Brasilia in August 2009. Of the 2,003 surveyed individuals, 226 were recipients of the local Bolsa-Escola program. These were asked in a non-incentivized way to rank their preferences between a R$ 120 CCT and a R$ 120 CT and between a R$ 120 CCT and a R$ 125 CT. The numbers are extremely similar to the ones we obtain in our experiment: 88.1% demand the conditionality and 78.7% are willing to pay for it. Interestingly, the percentages are virtually identical for both genders of the respondents (half of the sample was male). Finally, in our experiment pilot, thirty-five families were offered to choose between the two types of payments: thirty-three of them preferred a CCT to a CT of equal size; thirty-two of them preferred a CCT to a CT of strictly greater size.

³⁷Three reasons why these measures of beliefs are not always significant are: (i) endogeneity issues; (ii) these are measures of the child’s beliefs of returns to schooling, rather than parents’ beliefs of the child’s beliefs of returns to schooling (which should be the variable with predictive power); (iii) we do not observe parents’ perceptions of the child’s private cost of going to school.
less willing to pay to keep the conditionality. Both in the comparisons of means and the regression results, we observe a substantial drop in the preference for conditionality when compared to the Baseline treatment.

The treatment effects in Table 6 indicate that a large portion of the preference for the CCT is therefore explained by the need to control the child. The probability of demanding the conditionality goes down from 0.88 to 0.44 and the probability of paying for the conditionality drops from 0.82 to 0.24, when compared to the Baseline treatment. Furthermore, the extrapolated threshold is reduced by R$55.9 (the censored measure drops by R$30.1). For all four outcome variables, the treatment coefficients are significant at 1%. Figure 6 illustrates the treatment effect by plotting the cumulative distribution for the censored threshold in both the Baseline and Don’t Tell treatment groups. The residual preference for conditionality after the Don’t tell treatment is imposed could be explained by the inability of the parents to hide information from the child and by other reasons that are addressed in Appendix G.

Jointly, the results from the Baseline and Don’t tell groups provide evidence that in our setting, on average: (i) parents and children have different preferences regarding schooling; (ii) parents positively value their child going to school ($\hat{V} > 0$); (iii) children face a positive net cost of attending school ($c > 0$). We need all these three conditions in order for most parents, all else equal, to be willing to pay to keep the conditionality, and most of them not paying for it anymore when their child believes that the conditionality is still binding.

5.2.3 Monitoring and parental control: Text message treatment

This treatment is designed to address Proposition 2 of our model. Our previous results suggest that the vast majority of the parents in our study might not be able to enforce school attendance by themselves and are willing to pay for an external device (the conditionality on the payments) for that purpose. If our story is correct, making information parents have on their children’s attendance behavior close to perfect if they choose an unconditional cash transfer (i.e., shifting $\pi$ close to one) should reduce significantly the preference for conditionality.

Both in the comparison of means and in the regression analysis, we observe a substantial decrease in parental preference for conditionality, when compared to the Baseline treatment. The treatment effects in Table 6 attest that observability is an important problem and that an increase in the degree of
information parents have about their child’s action regarding school attendance reduces drastically the necessity for the conditional cash transfer as monitoring device. When offered a free monitoring device (text messages), parents do not need to spend money to keep the conditionality on their cash transfers. The probability of demanding the conditionality is reduced from 0.88 to 0.35, and the likelihood of paying for the conditionality drops from 0.82 to 0.28 when compared to the Baseline treatment. Also, the extrapolated threshold drops by R$ 54.8 (the censored measure decreases by R$ 31.7). For all but one outcome variable, the treatment coefficients are significant at 1% (for the censored threshold, the p-value is 0.012). Figure 7 illustrates the treatment effect by plotting the cumulative distribution for the censored threshold in both the Baseline and Text message treatment groups.

These results indicate that a significant portion of the preference for conditionality is due to the higher monitoring level of the child’s action offered by the CCT. When it is no longer higher, preference for conditionality is considerably reduced. However, some parents still pay for the conditionality when they are endowed with better information (text messages). We do not address experimentally the exact motive behind that residual preference.38

5.2.4 Why do parents want the child to go to school? (1) - Non-classroom treatment

Table 6 shows that the Non-classroom treatment reduces preference for conditionality when compared to the Baseline treatment group, but the demand for conditionality is not eliminated. When compared to the Baseline group, the treatment reduces the likelihood of paying for the conditionality from 0.88 to 0.74 (not statistically significant) and the probability of paying for the conditionality from 0.82 to 0.68 (significant at 1%). The treatment decreases the threshold measured by the extrapolated data by R$ 34.6 and the censored threshold by R$ 16.5 (both significant at 1%). This suggests that a non-negligible portion of parental desire to control their child is to make sure the child attends school, and

38Three possible groups of reasons for those parents are: (i) the parent does not think that text messages imply better monitoring than the CCT: the parent might not be willing to move away from a mechanism that is certain to work (CCT); the parent might think she could lose her cell phone, change her number or have an income shock that forces her to sell the device for instance; (ii) the parent needs the conditionality to control someone other than the child: the parent might want the CCT to prevent someone else from not letting the child attend school or to commit herself to let the child attend school. Only one of the nine parents that still pay for the conditionality in the Text message treatment group has time-inconsistent preferences. Therefore, we believe that the remaining preference is not a problem of parental self-control. For more discussion on the role of time-inconsistent preferences, see Appendix G; (iii) the child cares about household income: if the child is altruistic toward the parent’s consumption level or if the parent spends a share of her consumption on a household public good also enjoyed by the child, then the CCT provides an additional incentive for the child to attend school, which a CT with perfect monitoring would not provide.
not necessarily classes. We address this result in more depth in the next sections of the paper.\footnote{After choosing their payment schemes, parents were asked whether they would prefer to revert back to their current CCT program or if they would rather abandon it and take the one based on attendance during the shift in which the child has no classes; 42.3% of the parents reported they would prefer to be part of the new program.}

5.2.5 Why do parents want the child to go to school? (2) - Signaling treatment

The Signaling treatment was implemented as a follow-up phone treatment with parents originally assigned to the Baseline treatment and therefore is not included in Table 6.\footnote{After eliciting their preferences over the new choices, parents were reminded about their original choice during the first implementation and were asked which set of decisions they would prefer to be implemented; 41.9% preferred to keep the new choice over the original one.} We can however do within-subject comparisons, comparing their choices under the Baseline and the Signaling treatments. The fraction of parents demanding the conditionality stays constant at 90.3\%, the fraction paying for the conditionality drops from 80.6 to 71\%, the average censored measure of the threshold is reduced from R$ 156.5 to R$ 155.7, whereas the extrapolated one is reduced from R$ 195.5 to R$ 184.8. None of these differences is statistically significant, using t-tests. The picture is almost identical if we compare instead the means of the outcome variables with the ones in the original Baseline treatment, or with the remaining parents in the Baseline treatment not included in the Signaling treatment.

In our regression analysis, in order to estimate the effects from the Signaling and the All components treatments, the observations from these two treatment groups now appear twice, once as part of one of these two treatment groups and once in the Baseline treatment. We now have five different treatment dummies in the specification, as shown in Table 7. The coefficients on the Signaling treatment dummies are only significant for the outcome variable “pay for conditionality” (at 10\%), and the point estimates are also fairly small. This suggests that the bulk of parental preference for the conditionality is explained by the combination of the non-classroom and the labor-market signaling components of schooling, without any conditionality on class attendance.

There is a concern that the phone treatment could have a differential effect because it was implemented in a different day, using a different tool, and it followed a previous experimental experience. In Appendix H, we present several robustness checks which address this concern.
5.2.6 Why do parents want the child to go to school? (3) - *All components* treatment

It may be the case that in the *Baseline* treatment, parents do not think that the probability of the child passing to the next grade equals one, and therefore the *Signaling* treatment would be also capturing the value of insurance against failing the grade. We address this issue by offering automatic promotion to the conditional schemes of the *Baseline* treatment, under the *All components* follow-up treatment.

If we do within-subject comparisons of the parents in this group with their choices when in the *Baseline* treatment, we observe no significant differences. The fraction of parents demanding the conditionality goes up from 90 to 93.3 percent; the fraction paying for the conditionality increases from 83.3 to 90 percent percent. The censored threshold is reduced from R$ 158 to R$ 157.8, whereas the extrapolated measure increases from R$ 194.8 to R$ 204.8.

These results are confirmed by the regressions in Table 7, which indicate that adding automatic promotion to the current set-up of the *Bolsa-Escola* program does not have a significant impact on parents’ preference for the conditionality.

5.2.7 Interaction effects

To address the robustness of our findings and test our predictions in more depth, we add interaction terms to our main specifications.

- *Text message* treatment interactions

For parents that already had a high level of information about their children, we would expect a weaker treatment effect (the increase in observability of the child’s action from the treatment is lower). We test this by interacting the *Text message* treatment dummy with a measure of how much the parent already observes her child’s actions regarding school attendance. In the study, parents were asked to estimate in a scale from 0 to 5 how much they think they know about what her child is doing during her day. Table 8 shows the results of the interaction of the *Text message* treatment dummy with this measure of (beliefs about) observability, where we use a standardized measure (zero mean and unit variance) of the 0 to 5-scale variable.\(^{41}\) The results suggest that for parents who report better abilities

\(^{41}\)With the standardized measure, the coefficient on the treatment dummy indicates the effect evaluated at the mean level of the scale variable, and the coefficient on the interaction term indicates the change in the treatment effect when moving that variable by one standard deviation.
to monitor their child, the *Text message* treatment had a weaker effect.

- **Non-classroom** treatment interactions

We would like to check whether the parent is keeping the conditionality when offered the new program in order to control a child that is more likely to be on the streets when not in class. We do not have an exact measure of how much parents think their child is likely to be on the streets, but we can use our measure of how much the parent believes she knows about what the child is doing during her day. The interaction effects are reported in Table 9.

The results indicate that the demand for a device to make the child be in school when the child does not have classes is much higher when parents do not know what their children are doing during their day. The interaction term is significant in all four specifications. More interestingly, the results point that, for parents that knew what their children are doing one standard deviation less than the mean of that variable, the preference for conditionality and the level of the threshold would be almost identical to the ones in the *Baseline* treatment under this new CCT program that only requires attendance when the child is not in class.

We present a second set of interactions to understand what is driving the demand for schooling when the classroom content is taken away. Our hypothesis is that the bulk of the non-classroom benefit from school in the studied context is given by the safety it provides by keeping the child off the streets. To test this hypothesis, we interact the *Non-classroom* treatment dummy with a district-level measure of violence: the number of criminal occurrences of any type in 2008 per 1,000 inhabitants, collected by the local Department of Security. We also want to know if the problem of violence is more important whether the child is male or female, which could tell us in general if parents are more worried about their child selecting into crime (more likely for boys) or being a victim of crime (more likely for girls). In general, we also want to see if, regardless of the local crime rate, the importance of the non-classroom component of schooling differs depending on the gender of the child. We interact the *Non-classroom* treatment dummy with a male child dummy and add a triple interaction term of the treatment dummy, the local crime rate and the male child dummy. To facilitate interpretation, we standardize the district-level crime rate measure (zero mean and unit variance).
The results are reported in panel B of Table 9. We observe that for a girl living in a mean-crime rate district, the preference for conditionality and the indifference thresholds are no longer lower in this treatment when compared to the Baseline group. We also see that the average threshold under this treatment increases significantly in more violent districts, consistent with the idea that in more dangerous areas, the value of the non-classroom component of schooling is higher. In fact, given the size of the coefficients, this treatment suggests that the bulk of parental valuation of the non-classroom component of schooling is a concern for the child’s safety. Also, the average threshold under the treatment is significantly lower for male than for female children: parents value the non-classroom component more for girls than for boys. This suggests the concern for violence is more related to protecting children from danger rather than avoiding them selecting into crime. We further address this hypothesis by looking at the triple interaction term. We observe that the difference of the treatment effect between genders increases when the crime rate increases: the non-classroom component is even more valuable for girls when the district is more violent.

6 Parental Valuation of Schooling

In this section we focus on measuring parental total net valuation of schooling, \( B(\text{skills}, \text{signal}, \text{nonclass}) \) and attempt to disentangle its different components.

6.1 Baseline results

In the Baseline group, 82% of the parents chose to pay for the conditionality. It seems safe to assume that the average parent in that group has \( \hat{V} > c \), i.e., she is paying for the conditionality. We now make one further assumption: \( \hat{V} < \bar{V} \). In terms of our model, this is equivalent to assuming that the level of monitoring that the parent has available without the CCT is sufficiently low (formally, \( \pi < [(1 - \alpha)c/(V - \alpha c)] \)) so that, absent the conditionality tied to the transfers, the child would not attend school. In that case, the parent’s willingness to pay for the conditionality (the distance between the parent’s threshold and the level of R$ 120) will measure her net total valuation of having the child attending school. Given the degree of the problem of observability of the child’s actions described in the previous sections, the assumption we make seems reasonable. But we can still ask ourselves: how
much more are the students in our experiment likely to attend school as a result of being part of the Bolsa-Escola program?

Unfortunately, we do not have a precise measure of the impact the conditional cash transfer has on the likelihood of attending class. We could not get official attendance rates for beneficiaries and non-beneficiaries of the Bolsa-Escola program. However, we were able to get, for two of the schools in our sample, attendance rates in August 2009 for 106 students aged thirteen-fifteen and that have never been enrolled in the program. The average monthly attendance rate for these students was 53.1%. This is an overestimated measure of actual attendance rates, since it excludes from the analyzed classrooms the students that dropped out before August, making it a biased sample. Also, those students are on average richer than the ones enrolled in the Bolsa-Escola program, since having a low enough per capita income is the criterion of eligibility. If one believes that attendance rates are lower for poorer families, the numbers would be even smaller.

We could not get actual attendance rates for children enrolled in the program. We have provided evidence suggesting that the children in our sample have a net cost of attending school and that parents have a problem of observing their attendance behavior. Since the requirement of the program is an 85% attendance rate, we assume this level as a predictor of the average attendance level among beneficiary students in the thirteen-fifteen age range. Hence, our estimate of the average increase in school attendance rates under the Bolsa-Escola program is about 31.9%.

More than actual attendance rates for Bolsa-Escola beneficiary and non-beneficiary students, we are interested in parents’ beliefs about the change in attendance rates due to tying the transfers to class attendance. In a follow-up survey, we asked thirty parents originally assigned to the Baseline group to estimate the number of class days they believe their child would miss in the next four weeks if the transfers from the government were no longer conditional on class attendance. The average expected attendance rate among the respondents is 30.9% of days. In our experiment, we also asked parents about the number of days of classes they believe their child missed in the last two months. The average belief of monthly attendance rate is 96%. In this case, the estimated increase in the beliefs of attendance rates due to imposing a conditionality on transfers from the government is 65.1%.

Using the estimated thresholds for the Baseline treatment group, we can make inference on parents’ valuation of that increase in the probability of attending school. If we take into account the extrap-
olated measure of the threshold, the average estimated valuation is R$ 81 for the individuals in the Baseline group. This level corresponds to 14% of the average current post-CCT monthly household income estimated for that group (R$ 569.8). If instead we use the censored measure of the threshold, the average estimated equivalent compensation is R$ 37.3 for the individuals in the Baseline group (still 6% of their average household income).

### 6.2 Decomposing the value of schooling

In our model, we defined $B(skills, signal, nonclass)$ as the parent’s total net valuation of schooling (i.e., the change in parental utility from having the child going to school relative to not going). In this subsection, we use our experimental set-up to disentangle the relative contribution of each of the three arguments to parental overall valuation of schooling. Of course, we do not claim to infer exact magnitudes from this exercise, but rather to have a sense of the relative importance of different benefits of school in the total valuation. We use the estimated thresholds from three treatments, Non-classroom, Signaling and All components for our decomposition.

We make the following assumptions for our estimation:

**Assumption 1** The parent believes that the probability of the child attending the class shift with no CCT is the same regardless of the existence of CCT-incentives for the no-class shift.

This assumption is made to enable us to compare the parent’s utility in each of the three treatment groups above to the same benchmark level with no conditionality on the payments.

It is possible that if the child is already coming to the no-class shift, then the child could just stay at school for the class shift. Alternatively, it is possible that if the child is already at school for one shift, then the child is more likely to be tired of being there and less likely to attend the class shift. To address the validity of the assumption, we ran a follow-up survey with twenty-five parents originally allocated to the Non-classroom treatment group and asked them to compare the two probabilities above. Twenty-five of them said the two probabilities are the same, one said the likelihood of attending the class-shift is higher when the child is incentivized to attend the no-class shift and one reported that the other probability is higher.
Assumption 2 The parent believes the benefit from being at school with no classes is the same regardless of the shift.

To check the validity of this assumption, we use the fact that the children in our sample study in different shifts and interact the Non-classroom treatment dummy with an indicator of the shift during which the child has class. The interaction is never significant, providing evidence that parents value the non-classroom component of school equally regardless of the shift.

Assumption 3 The parent believes that if there is no class-CCT, the child does not acquire the skills nor the signaling component of schooling. The parent also believes that if there is no CCT for either shift, the child does not acquire the non-classroom component of schooling. The parent believes that an 85% attendance rate in class provides the skills-acquisition component of schooling, and that an 85% attendance rate in any shift provides the non-classroom component.

This assumption is made for simplicity, in order to avoid dealing with fractions of the different components of schooling and having to impose functional forms on the function $B(\cdot)$.

Finally, if the parent believes that in the absence of any conditional payments the child is more likely to attend school in the class shift than in the no-class shift, our empirical measure of parental valuation would overestimate the importance of the non-classroom component of schooling. In the status quo, the children that we analyze are welcome to attend their school during the no-class shift, though their attendance is not required. As mentioned before, in a follow-up survey, we asked thirty parents originally assigned to the Baseline group to estimate the number of days they believe their child would miss in both shifts in the next four weeks if the transfers from the government were no longer conditional on any behavioral outcome. Interestingly, parents’ beliefs are that the child is more likely to attend the no-class than the class shift. The average expected attendance rate is 30.9% of days for the class shift and 44.8% of days for the no-class shift. This difference is significantly different from zero (at the 1% level) using a t-test of equality in matched data. This finding suggests that the studied children might prefer to be at school with no classes (and play sports, for instance) than to be there in the classroom. To be conservative, we make one last assumption:

Assumption 4 The parent believes that in the absence of any conditional payments the child is equally likely to
attend school in the class and no-class shifts.

Our empirical variable “threshold” can help us estimate parental net (total) valuation of having the child going to school relative to not attending school in the case in which the parent perfectly observes the child’s action ($\pi = 1$).

In our experiment, for those with $TCT > 120$, we have:

$$TCT - 120 = \{U^{\text{parent}} | S^* = 1, \pi = 1\} - \{U^{\text{parent}} | S^* = 0, \pi = 1\} \equiv B(\text{skills, signal, nonclass})$$

Under the aforementioned assumptions, if we compare the levels of the average threshold (controlling for the set of covariates from our main regressions) in each of the three treatment groups to the baseline level of the CCT (R$ 120), we can measure the relative value of the different components of schooling for the average parent in the study. If we use the extrapolated data, we get:

- From the Non-classroom treatment: $B(0, 0, \text{nonclass})$=R$ 34.6$
- From the Signaling treatment: $B(0, \text{signal}, \text{nonclass})$=R$ 70.8$
- From the All components treatment: $B(\text{skills}, \text{signal}, \text{nonclass})$=R$ 83.8$

where, for notation simplicity, we write $B(\text{skills} = 0, \text{signal} = 0, \text{nonclass} = 1) \equiv B(0, 0, \text{nonclass})$ and keep the same convention for all arguments of $B(.)$.\(^{42}\)

Conditional on having the non-formal and the labor-market signaling content of schooling, adding the formal education content (skills) does not drastically increase the perceived total net valuation of schooling by the parents in our study.\(^{43}\)

Finally, even if we cannot give an exact value for each of the components separately, we can put bounds on their sizes, if we make the following two assumptions of weak super-modularity (complementarity):

\(^{42}\)The numbers for the censored measure of the thresholds are: $B(0, 0, \text{nonclass})$=R$ 16.7$, $B(0, \text{signal}, \text{nonclass})$=R$ 37.1$, $B(\text{skills}, \text{signal}, \text{nonclass})$=R$ 40.5$.

\(^{43}\)An observation is important here. The Signaling and All components treatment also provide families with the chance to have the option of the child still being in school in one year, when she could be more mature and decide to study on her will. This feature is not related to the labor-market signaling content of schooling. It implies an under-estimation of the importance of the non-classroom content of school.
1. \( B(\text{skills}, \text{signal}, \text{nonclass}) \geq B(\text{skills}, \text{signal}, 0) + B(0, 0, \text{nonclass}) \)
   
   (the classroom and non-classroom components are not substitutes)

2. \( B(0, \text{signal}, \text{nonclass}) \geq B(0, 0, \text{nonclass}) + B(0, \text{signal}, 0) \)
   
   (the signaling and non-classroom components are not substitutes)

Under these assumptions, if we use the extrapolated data, we get the following bounds:

\( B(\text{skills}, 0, 0) \leq R\$ 13 \), \( B(0, \text{signal}, 0) \leq R\$ 36.2 \), and \( B(0, 0, \text{nonclass}) \geq R\$ 34.6 \).

Although these are rough estimates,\(^{44}\) our evidence suggests that skills acquisition might not be the driving force of the demand for schooling in the context we study.\(^{45}\)

These results can be mapped to recent findings in the economic literature. Hanushek and Woessmann (2009) show that although children in Latin America attain high levels of schooling, they do not develop the cognitive skills one would expect, and argue that this partly explains the lack of growth of these economies. The parents’ preferences in our study fit into a story in which the added value of the formal educational content to the overall value of schooling is low.

### 7 Conclusion

Using a high-stakes field experiment, we study schooling decisions of poor households with adolescent children in urban Brazil. We provide evidence that the following aspects, treated as secondary in the standard economic approach to schooling, can play a crucial role in the actual decision-making process:

First, in the setting we study, intergenerational conflict is central to the schooling decision. There is a divergence in preferences across generations within each household: parents highly value their children’s being in school and more than the children themselves do.

\(^{44}\)The numbers for the censored measure of the thresholds are: \( B(\text{skills}, 0, 0) \leq R\$ 3.4 \), \( B(0, \text{signal}, 0) \leq R\$ 20.4 \), and \( B(0, 0, \text{nonclass}) \geq R\$ 16.7 \).

\(^{45}\)One might expect the value of the skills content to be larger the better the school is at providing it. To address that, we interact the \textit{All components} treatment dummy in our regressions with a measure of the quality of education made at the school level by the federal government of Brazil. The measure we use is the INDEB, an index of the educational content of each school in the country made in 2007 by comparing the average grades each school got in the same standardized test. The coefficients on the interaction terms are always positive, and significant for the dependent variable “demand the conditionality,” suggesting that the skills acquisition content is more valuable in the better performing schools.
Second, we observe that informational asymmetries can play a crucial role: parents reveal inability to observe the actions of their children and are willing to pay substantial sums for mechanisms that can increase their monitoring and control over their children’s school attendance.

Third, we analyze parents’ willingness to pay for three different components of schooling. In the slums that we study, parents seem to substantially value schooling for the safety that it provides to children and for its signaling/social promotion feature. Skills acquisition, traditionally viewed as the quintessential role of schooling, does not seem to be central in parental valuation of schooling.

These results depart from the usual approach to schooling in economics, where family conflict and agency issues play, at most, a secondary role, and where schooling is viewed primarily as a means of skills acquisition. Taken together, our results suggest that parents in poor households may not be able to control their child’s decisions on schooling investment and safety. The consequence may be overexposure to violence and underinvestment in education, both being consistent with unfortunate current trends in the geographical area we study.

These findings have important implications in terms of policy design, particularly in developing countries. The puzzle of low levels of school attainment in countries such as Brazil, where returns to schooling are very high, is usually understood according to the standard approach (in which parents are assumed to make the decision for their children and where intrahousehold information asymmetries are absent) as evidence that parents either underestimate the actual returns, or that they cannot afford to have a child not working.\textsuperscript{46} In our sample, parents have accurate beliefs about the actual returns to schooling. Also, for the average family in the study, income does not seem to be a constraint that forces parents to prefer an unconditional payment. Preference for the conditionality persists even for the poorest households in our sample.\textsuperscript{47}

This paper provides evidence in favor of a different explanation to the aforementioned puzzle, based on informational issues inside the household: parents want their children to go to school but cannot directly enforce their desire. According to our approach, policies designed to promote school attendance might be more effective if they target the child or the household information structure.

\textsuperscript{46}Edmonds (2006) and Edmonds and Schady (2009) provide evidence that increasing household income through transfers reduce the allocation of children to labor and increase school attendance.

\textsuperscript{47}Our regression results suggest that poorer parents are willing less to pay for the conditionality. This is consistent with the results in Bursztyn (2009) which show that poorer voters in Brazil are less likely to favor public educational spending relative to increases in cash transfers.
instead of focusing on parents, as most do in practice. If parents in our study are correct in their beliefs, all that is necessary for many of them to promote school attendance is to provide them with more information (and therefore increased control) regarding their children’s actions. If the goal of policy is to ensure school attendance alone, with no concerns for redistribution, then sending text messages is much more cost-effective than conditional cash transfers.

On the other hand, we also provide *prima facie* evidence that conditional cash transfers (CCTs) can be a preferred alternative to unconditional cash transfers (CTs). It has been argued that CT programs may be superior to the current global trend of CCTs, since families know better how to optimize for their household, and that with a sufficiently large CT, families will be free to invest optimally in education. In the context of schooling that we study, recipients of these transfers surprisingly prefer the CCT as the conditionality provides a solution to their intrahousehold, intergenerational contracting problem. This gives a new rationale for adopting this type of program. In an environment in which providing parents direct information about their children’s attendance is not available or feasible, CCTs can induce school attendance by the child, even when the parent is not able to enforce it by herself.

Additionally, in the context we analyze, we observe a strong demand for policies that increase the safety provided to children. Low-cost policies such as after-school programs, even without skills acquisition components tied to them, could have a significant impact on parents’ welfare. The relatively low importance attributed to the skills formation content of schooling might also suggest that there is room for a great deal of improvement in the educational quality of Brazilian public schools.

In conclusion, we note the limitations of this study. First, much of the discussion herein centered on the parents’ beliefs. We focus on preferences, not on actual outcomes. We have reason to expect that many of parents’ beliefs are rational, but we cannot observe this directly. Second, we only focus on adolescents. There is significant evidence that skills acquisition is most important in the early years of childhood, and that these skills work as a complement to those gained during adolescence (Cunha and Heckman, 2007). Parents most likely have more control over younger children, but we have no data on how control may decay over time. Finally, our findings concerning the key role of intergenerational conflict and agency problems in schooling decisions relate to a sample of impoverished families in urban Brazil. Further work is needed to address the extent to which such factors are central to schooling choices in other contexts.
References


Appendix

A Theory Appendix

Proof of Proposition 3:

1. \( V \leq c - T_{CCT} \).
   
   In this case, parental utility under a CCT that pays is \( T_{CCT} \) is \( U^{parent|CCT, T_{CCT}} = Y \) and parental utility under a CT that pays \( T \) is \( U^{parent|CT, T} = Y + T \).
   
   Hence, \( T_{CT} = 0 \).

2. \( c - T_{CCT} < V < c \).
   
   In this case, parental utility under a CCT that pays is \( T_{CCT} \) is \( U^{parent|CCT, T_{CCT}} = Y + T_{CCT} + V - c \) and parental utility under a CT that pays \( T \) is \( U^{parent|CT, Y} = Y + T \).
   
   Hence, \( T_{CT} = T_{CCT} + (V - c) < T_{CCT} \).

3. \( V = c \).
   
   In this case, parental utility under a CCT that pays is \( T_{CCT} \) is \( U^{parent|CCT, T_{CCT}} = Y + T_{CCT} \) and parental utility under a CT that pays \( T \) is \( U^{parent|CT, Y} = Y + T \).
   
   Hence \( T_{CT} = T_{CCT} \).

4. \( c < V < V \).
   
   In this case, parental utility under a CCT that pays is \( T_{CCT} \) is \( U^{parent|CCT, T_{CCT}} = Y + T_{CCT} + V - c \) and parental utility under a CT that pays \( T \) is \( U^{parent|CT, Y} = Y + T \).
   
   Hence \( T_{CT} = T_{CCT} + (V - c) > T_{CCT} \).

5. \( V \geq V \).
   
   In this case, parental utility under a CCT that pays is \( T_{CCT} \) is \( U^{parent|CCT, T_{CCT}} = Y + T_{CCT} + V - c \) and parental utility under a CT that pays \( T \) is \( U^{parent|CT, Y} = Y + T + V - V \).
   
   Hence \( T_{CT} = T_{CCT} + (V - c) > T_{CCT} \).

B Representativeness, Randomization and Show-Up Rates

The families in the experiment are representative of the population of all families in Brasilia eligible for the Bolsa-Escola program. Table A.1 shows that the families in our study do not differ greatly from other eligible families in Brasilia on a number of important observables (we report their mean levels). The data in the first and second columns are those collected by the government when determining eligibility for the program. The data in the furthest right column were collected in our experiment. Since there are discrepancies in the data on the families in our study, the best comparison is the first and second columns since these were collected in the same manner.\(^{48}\)

\(^{48}\)The largest notable discrepancy comes from reported household income. Families have incentive to under-report income to the government, and this would explain our discrepancy. However, it is also possible, though unlikely, that these households are now making R$ 22 more per month on average than what they did at the time of the program measurements in the beginning of the year.
Additionally, the show-up fees promised in the recruitment letters were significant enough to yield a large turnout for each session. Table A.2 shows that for a promised show-up fee of R$ 7, we had an 85% show-up rate, and for R$ 10, this increased slightly to 94%. This eliminates fears that selection drives our results.

C Pilot design and results

To get a glimpse of the dynamics and preferences of the household and inform our main experimental designs, we ran a pilot in March of 2009. Here we briefly describe the design and results.

C.1 Pilot design

The CCT-CT trade-off also began the pilot experiment, and was similarly incentivized using the Becker DeGroot Marschak elicitation procedure. For this set of questions, we held the size of the CT constant at R$ 120 while we increased the value of the CCT in increments of R$ 5, from R$ 110 to R$ 170.

We also elicited parents’ and children’s time preferences, beliefs of returns to schooling, beliefs of child’s attendance, how the child gets to school, and how nurturing and disciplinary the parent was. The first two sets of questions were elicited in the same manner as in the final study.

We randomly selected fifteen year-old students from one randomly chosen school each from three separate randomly chosen districts surrounding Brasilia. As in the final study, students were handed recruitment letters from the principal. One school had a R$ 7 show-up fee and had nine of twelve invited families show up while in the two others we offered R$ 5 and had average show-up rates of 62%. In total, thirty-five families attended.

C.2 Pilot results

The results from the pilot are quite similar to those in the baseline of the final study. 94.8% of parents were willing to pay for the conditionality. Parents’ beliefs were directionally higher than the children’s for all measures of the returns to schooling and were significantly higher for both the expected wages given a secondary degree or a college degree. Similar to our current study, there was a considerable information asymmetry in the child’s attendance. Parents reported significantly fewer absences and also cited “The child not want to go” significantly less frequently as a reason for any absences.

D The child’s decision and the joint decision

D.1 The child’s decision

According to the model, the child’s utility level is independent from the choice of conditional or unconditional transfers by her parent. If the parent has enough information to induce her to go to school, then the parent will only transfer resources to the child up to the point in which the child is indifferent between attending or not attending school (or slightly better off by attending).

Therefore, if the child believes that her parent is going to transfer her just the amount enough to make her indifferent between going or not going to school, the child should be indifferent between any
levels of conditional and unconditional cash transfers. If the child believes that her parent will transfer her more to ensure that she attends school, then she will prefer the conditionality to the extent that she expects that difference to be large.

In addition to that, if the child has self-control problems, and if the type of the cost to go to school faced by the child is a small, everyday cost that makes her procrastinate and not go to school, then if the child is sophisticated, she will be willing to pay for the conditionality as a commitment device.

In three of our four original treatments (the exception being the Don’t Tell treatment), the child was offered the same choices as the parent between conditional and unconditional payments, but always with the conditionality of the Baseline treatment. We therefore have 161 children who had their preferences between payments elicited. However, their choices were not incentivized, so we examine the results with skepticism. We observe preference for the conditionality among children, albeit at abated levels: 70% demand the conditionality, while 54% pay for the conditionality. Though substantial, these are significantly less than the respective numbers for the parents in the Baseline treatment (88% and 82%) at the 1% level using either a t-test or rank sum test of equality. Furthermore, the median level of the censored threshold for children is between R$ 120 and R$ 125, compared to R$ 180 by parents in the Baseline treatment.49 We did not implement specific treatments to understand the exact structure of the cost to go to school faced by the child and we leave this as a path for future research.

D.2 The joint decision

In the Baseline treatment, the experiment concluded with the parent-child pairs performing a joint decision-making task over the same CT-CCT decision they both faced as individuals. Since these questions were not incentivized, we examine the results with skepticism.

Joint decisions also show high demand and high willingness to pay for the conditionality: 74% of joint decisions reveal demand for the conditionality, and 62% pay for the conditionality. On average, parents and children are jointly willing to give up R$ 23.8 to keep the conditionality (using the censored measure of the threshold). Though these levels are high, they are all significantly lower than the parents’ ones, all at 1% level of significance using a t-test or rank sum test of equality.

Parents do not seem to wield a lot more bargaining power than the children in the joint decision. If we look at non-trivial joint decisions (in which the parents and children did not have equal thresholds in the individual decisions), and only consider joint decisions that fall in the interval of the child’s and parent’s individual decisions, on average the joint decision is very close to the midpoint of the interval. Using the censored data, if we think of the child’s bargaining weight as $\alpha$ in the equation

$$\text{Joint Threshold} = \alpha \cdot \text{Child Threshold} + (1 - \alpha) \cdot \text{Parent Threshold},$$

the average $\alpha$ is 0.44. Also, $\alpha = 0$ thirteen of thirty-two times, and $\alpha = 1$ eleven of thirty times. Thus, overall, parents seem to have slightly higher bargaining power in the joint decision. Understanding the dynamics of intergenerational bargain over this type of decision is another promising area for future research.

E Measurement of preference parameters

We measured parents’ and children’s time preference over now versus one week and one week versus two weeks. There has been an increasing body of literature indicating that lab measures of time preference indicate...

We elicited time preference using a standard Becker DeGroot Marschak procedure. First, subjects were asked a series of binary choices, between R$ 5 in one week and R$ Y in two weeks. Y started at 5 and increased to 15 in increments of R$ 0.05. The resulting discount factor between one week and two weeks is our variable “delta”. The choice was then repeated for R$ 5 now versus R$ Y in one week. The ratio of the discount factor between now and one week to the discount factor between one and two weeks is the measure of time inconsistency, our variable “beta”. Subjects were told there would be a 5% chance they would be paid for one of their randomly chosen responses. If the payment was to be made in the future, delivery occurred via the school principal in an envelope to either the child or the parent.

The other important preference parameter that was elicited in a financially incentivized fashion was beliefs of returns to schooling. For 24.5% randomly chosen families, subjects were asked to guess the empirical averages for monthly wages for someone from their city who had completed (i) one more year of schooling than the child, (ii) two more years of schooling than the child, (iii) a secondary degree, and (iv) a university degree. Every time their guess was within R$ 20 of the empirical average (according to the Pesquisa Nacional por Amostra de Domicílios - PNAD 2007 data set), they were awarded R$ 1. Since there is a concern that subjects might not think their child or themselves are average, for the remaining 75.5% of the families we asked these questions in a non-incentivized way: how much the parent and the child think the child would earn as monthly wage if she completed each of the levels of schooling mentioned above. We found no effect of asking the questions in an incentivized fashion (the means and standard deviations are almost identical). Therefore we collapse both incentivized and non-incentivized answers into one measure for each question.

F Construction of the threshold variables

As defined in the model, we also look at the amount that made a parent choose an unconditional transfer over their current CCT of R$ 120, which we call her threshold. A few observations should be mentioned here. First, the way the question was asked in the experiment was through first holding fixed the CCT at R$ 120 and increasing the amount for the CT (in R$ 5 increments up to R$ 180), and then by keeping the amount of the CT fixed at R$ 120 and increased the amount for the CCT (in R$ 5 increments up to R$ 180). Therefore, for those who preferred a CT of R$ 120 to a CCT of the same amount, we need to make an approximation in order to map their answer to a threshold of preference over a R$ 120 CCT. For that purpose, we follow the model and make a linearity assumption in the construction of our threshold variable: if the required unconditional transfer to make a parent indifferent to a R$ X CCT is R$ T, then the required unconditional transfer for a R$ X+Y CCT is R$ T+Y.50

The second observation we make is that even though the question was asked in R$ 5 increments, we treat it as a continuous variable in our regressions, to facilitate the interpretation of the coefficients.

Finally, it is important to mention that our data display a censoring problem for the threshold variable. The choices between different amounts of money in both the CT and CCT payment schemes went from R$ 120 to R$ 180. However, for twenty-five parents, the maximal size of the CCT was not

50The results throughout the paper are indistinguishable if we assume instead an approximation based on logarithmic utility. The correlation of the measures of threshold is above 0.99.
enough to induce them to choose it over a R$ 120 CT, whereas for seventy-one parents, the maximal CT was not enough to induce them to choose it over a R$ 120 CCT. To take that into account, we ran Tobit regressions with censored data outside the two limits. We also used these regressions to predict the true value of the threshold for those whose value was censored.

G Residual preference for conditionality in the Don’t Tell treatment group

As he have seen, there is still some demand for the conditionality even after the Don’t Tell treatment is imposed. We suggest three main reasons for that. The first one is the inability of the parent to lie to their child or to hide a change in the cash transfer scheme from the child. Either one would attenuate the treatment effect. The second reason is that the parent might want the CCT to control someone else, such as her spouse. Over 95% of the parents that came to our study were mothers. They might want the conditionality to control their husband would could want the child to work or help at home instead of study. These two reasons are not corroborated by the data, though this may be a result of noisy measures. For aversion to lying, we asked the families whether they would ever lie to their children, and the interaction of the answer with the treatment indicator is not significant. For spousal control, the interaction of the treatment indicator with a dummy for married parent is not significant.

Finally, as a third explanation, the parent could prefer the CCT because it works as a commitment device to control herself. This motive is not included in our model since, as we have seen, the conditionality is mainly driven by a need to control the child. We can nevertheless address the intuition behind it. In the Don’t Tell Treatment, the child’s willingness to attend school is no longer a problem because she believes she is still incentivized to attend it. But a parent with time-inconsistent preferences and facing a small cost every day for the child to go to school (for instance, an effort cost) might procrastinate and not pay it. This can occur even if at the lab stage (when the CT versus CCT choice is made) she would be better off if the child went to school. If the parent is sophisticated, she will anticipate that: she will still prefer the CCT to the CT even if the child is incentivized under both regimes. We find some evidence in favor of that motive. When we look at the interaction term, the effect of the treatment is significantly weaker the more hyperbolic the parent is (i.e., lower time-inconsistency discount factor, “beta”), as seen in Table A.3. To facilitate interpretation, we standardize our measure of beta (zero mean and unitary variance).

H Robustness checks for the follow-up treatments

There is a concern that the follow-up phone treatments could have a differential effect because they were implemented in a different day, using a different tool, and they followed a previous experimental experience. To take that into account, we also treated one week later by phone the remaining thirty families from the original control group that had not been assigned to the Signaling treatment. eighteen of those parents were randomly allocated to a phone version of the Non-classroom treatment and the remaining twelve were asked again the same baseline questions. None of these families were reminded about their original choice before the treatments were implemented by phone.

The first piece of evidence we present are within-subject comparisons for the twelve parents that were in the Baseline group in both implementations. The percentage of parents demanding the conditionality decreases from 100% to 91.7%; the percentage of them paying for it drops from 83.3% to 67.7% when asked asked the same question by phone; the average censored threshold goes down from R$
156.2 to R$ 155, whereas the average for the extrapolated threshold increases from R$ 196.1 to R$ 199.5. These differences are never statistically significant.

As an additional robustness check, we re-run the regressions with the two follow-up treatments (Signaling and All components, repeating the thirty observations originally assigned to the Baseline. Twelve observations were added to the Baseline treatment and eighteen to the Non-classroom treatment. We also add a dummy on whether the treatment was implemented by phone and an interaction of that dummy with the Non-classroom treatment dummy (to consider a differential effect of a phone interview on the effects of that treatment). The results are reported in Table A.4. For all four dependent variables, both coefficients are statistically insignificant.
Figures and Tables

Figure 1: Fraction of parents demanding the conditionality across treatments

Figure 2: Fraction of parents paying for the conditionality across treatments
Figure 3: **Average required unconditional transfer ("threshold") across treatments—extrapolated measure**

Figure 4: **Average required unconditional transfer ("threshold") across treatments—censored measure**
Figure 5: Cumulative probability for the required unconditional transfer (censored threshold) in the Baseline treatment group

Figure 6: Cumulative probability for the required unconditional transfer (censored threshold): Baseline and Don’t Tell treatment groups (with 95% bootstrap confidence intervals - 1,000 bootstrap samples)

Notes: We re-sampled with replacement from the empirical distribution 1,000 times. From these 1,000 bootstrap samples, we computed the confidence intervals for each point on the cumulative distribution.
Figure 7: Cumulative probability for the required unconditional transfer (censored threshold): Baseline and Text message treatment groups (with 95% bootstrap confidence intervals - 1,000 bootstrap samples)

Notes: We re-sampled with replacement from the empirical distribution 1,000 times. From these 1,000 bootstrap samples, we computed the confidence intervals for each point on the cumulative distribution.
Table 1: **Treatments description**

<table>
<thead>
<tr>
<th>Treatment group</th>
<th>Type of conditionality</th>
<th>Additional features</th>
<th>Does the child know the choice?</th>
<th>Obs.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>85% attendance of classes</td>
<td>-</td>
<td>Yes</td>
<td>61</td>
</tr>
<tr>
<td>Don’t tell</td>
<td>85% attendance of classes</td>
<td>-</td>
<td>No</td>
<td>47</td>
</tr>
<tr>
<td>Text message</td>
<td>85% attendance of classes</td>
<td>Text message when child misses (for any choice)</td>
<td>Yes</td>
<td>52</td>
</tr>
<tr>
<td>Non-Classroom</td>
<td>85% attendance in no-class shift</td>
<td>-</td>
<td>Yes, but not autom. promotion</td>
<td>53</td>
</tr>
<tr>
<td>Signaling</td>
<td>85% attendance in no-class shift</td>
<td>Automatic promotion (only if chooses CCT)</td>
<td>Yes, but not autom. promotion</td>
<td>31</td>
</tr>
<tr>
<td>All components</td>
<td>85% attendance of classes</td>
<td>Automatic promotion (only if chooses CCT)</td>
<td>Yes, but not autom. promotion</td>
<td>30</td>
</tr>
</tbody>
</table>

Notes: Signaling and All components were follow-up treatments implemented to the parents originally in the Bolsa-Escola treatment.
Table 2: Means of observables across treatments

<table>
<thead>
<tr>
<th>Variable (mean)</th>
<th>Baseline (N=61)</th>
<th>Don’t tell (N=47)</th>
<th>Text message (N=52)</th>
<th>Non-classroom (N=53)</th>
<th>Signaling (N=31)</th>
<th>All components (N=30)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age (parent)</td>
<td>40.58</td>
<td>41.23</td>
<td>38.47*</td>
<td>40.10</td>
<td>39.67</td>
<td>41.50</td>
</tr>
<tr>
<td>Male parent</td>
<td>0.03</td>
<td>0.04</td>
<td>0.00</td>
<td>0.08</td>
<td>0.07</td>
<td>0.00</td>
</tr>
<tr>
<td>Male child</td>
<td>0.44</td>
<td>0.38</td>
<td>0.42</td>
<td>0.48</td>
<td>0.43</td>
<td>0.45</td>
</tr>
<tr>
<td>Married</td>
<td>0.49</td>
<td>0.56</td>
<td>0.58</td>
<td>0.42</td>
<td>0.39</td>
<td>0.60</td>
</tr>
<tr>
<td>Single</td>
<td>0.25</td>
<td>0.17</td>
<td>0.17</td>
<td>0.26</td>
<td>0.29</td>
<td>0.20</td>
</tr>
<tr>
<td>Divorced</td>
<td>0.25</td>
<td>0.25</td>
<td>0.23</td>
<td>0.30</td>
<td>0.29</td>
<td>0.20</td>
</tr>
<tr>
<td>Log HH income</td>
<td>6.24</td>
<td>6.23</td>
<td>6.23</td>
<td>6.09*</td>
<td>6.20</td>
<td>6.28</td>
</tr>
<tr>
<td>Employed</td>
<td>0.47</td>
<td>0.43</td>
<td>0.53</td>
<td>0.56</td>
<td>0.57</td>
<td>0.37</td>
</tr>
<tr>
<td>Employed Spouse</td>
<td>0.32</td>
<td>0.34</td>
<td>0.47*</td>
<td>0.15**</td>
<td>0.27</td>
<td>0.37</td>
</tr>
<tr>
<td>Catholic</td>
<td>0.51</td>
<td>0.54</td>
<td>0.54</td>
<td>0.57</td>
<td>0.52</td>
<td>0.50</td>
</tr>
<tr>
<td>Protestant</td>
<td>0.39</td>
<td>0.38</td>
<td>0.40</td>
<td>0.40</td>
<td>0.35</td>
<td>0.43</td>
</tr>
<tr>
<td>No religion</td>
<td>0.05</td>
<td>0.02</td>
<td>0.04</td>
<td>0.02</td>
<td>0.10</td>
<td>0.00</td>
</tr>
<tr>
<td>Beta (parent)</td>
<td>1.00</td>
<td>0.93</td>
<td>1.01</td>
<td>1.08</td>
<td>0.96</td>
<td>1.04</td>
</tr>
<tr>
<td>Beta (child)</td>
<td>1.14</td>
<td>0.93**</td>
<td>1.00</td>
<td>0.97**</td>
<td>1.03</td>
<td>1.25</td>
</tr>
<tr>
<td>Delta (parent)</td>
<td>0.76</td>
<td>0.79</td>
<td>0.67**</td>
<td>0.69</td>
<td>0.77</td>
<td>0.75</td>
</tr>
<tr>
<td>Delta (child)</td>
<td>0.73</td>
<td>0.83</td>
<td>0.78*</td>
<td>0.79</td>
<td>0.74</td>
<td>0.72</td>
</tr>
<tr>
<td>Yrs. schooling (parent)</td>
<td>7.12</td>
<td>6.34</td>
<td>7.18</td>
<td>6.62</td>
<td>6.53</td>
<td>7.70</td>
</tr>
<tr>
<td>Yrs. schooling (child)</td>
<td>7.59</td>
<td>8.02*</td>
<td>7.84</td>
<td>7.54</td>
<td>7.47</td>
<td>7.72</td>
</tr>
<tr>
<td># of children in HH</td>
<td>3.65</td>
<td>3.74</td>
<td>3.63</td>
<td>3.27</td>
<td>4.03</td>
<td>3.27</td>
</tr>
<tr>
<td>Black parent</td>
<td>0.28</td>
<td>0.19</td>
<td>0.19</td>
<td>0.17</td>
<td>0.29</td>
<td>0.27</td>
</tr>
<tr>
<td>Mestizo parent</td>
<td>0.56</td>
<td>0.56</td>
<td>0.56</td>
<td>0.60</td>
<td>0.55</td>
<td>0.57</td>
</tr>
<tr>
<td>White parent</td>
<td>0.13</td>
<td>0.21</td>
<td>0.23</td>
<td>0.19</td>
<td>0.10</td>
<td>0.17</td>
</tr>
<tr>
<td>Black child</td>
<td>0.28</td>
<td>0.19</td>
<td>0.19</td>
<td>0.17</td>
<td>0.29</td>
<td>0.27</td>
</tr>
<tr>
<td>Mestizo Child</td>
<td>0.56</td>
<td>0.56</td>
<td>0.56</td>
<td>0.60</td>
<td>0.55</td>
<td>0.57</td>
</tr>
<tr>
<td>White Child</td>
<td>0.13</td>
<td>0.21</td>
<td>0.23</td>
<td>0.19</td>
<td>0.10</td>
<td>0.17</td>
</tr>
</tbody>
</table>

Notes: We display the means across treatments of the covariates used in the main regressions. We perform t-tests of equality in means, comparing the means of each variable in each treatment to the ones in the baseline treatment.

* 10% significant difference (for the mean in the treatment group when compared to the mean in the baseline group).
** 5% significant.

a Beta refers to the time-inconsistency discount factor. It is the ratio between the time-discount factor of now vs one week and the discount factor of one week vs two weeks. Therefore beta different from one refers to time-inconsistent preferences.

b Delta refers to the discount factor of one vs two weeks estimated in the experiment. (For a discussion on the construction of Beta and Delta see Appendix E).

c Race is self-reported.
<table>
<thead>
<tr>
<th>Beliefs about child monthly income if she drops out and gets a job</th>
<th>Parent’s belief (in R$)</th>
<th>Child’s belief (in R$)</th>
<th>Diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Beliefs about child monthly income increase with...</td>
<td>Parent’s belief (Std. deviation in parentheses)</td>
<td>Child’s belief</td>
<td>Diff.</td>
</tr>
<tr>
<td>One More Yr. of School</td>
<td>198 (200)</td>
<td>217 (509)</td>
<td>-19</td>
</tr>
<tr>
<td>Two More Yrs. of School</td>
<td>323 (272)</td>
<td>313 (612)</td>
<td>20</td>
</tr>
<tr>
<td>Secondary Degree</td>
<td>576 (388)</td>
<td>561 (774)</td>
<td>15</td>
</tr>
<tr>
<td>College Degree</td>
<td>2,066 (5,677)</td>
<td>2,197 (12,509)</td>
<td>-131</td>
</tr>
<tr>
<td>Yearly average of beliefs about rate of returns to schooling</td>
<td>22% (0.12)</td>
<td>19.2% (0.14)</td>
<td>2.8%**</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>High-school graduate wage</td>
<td>952.5 (424.2)</td>
<td>956.4 (956.9)</td>
<td>-3.9</td>
<td>865 (100)</td>
<td>765</td>
<td>100**</td>
<td>903.8</td>
</tr>
<tr>
<td>College graduate wage</td>
<td>2425.8 (5805.3)</td>
<td>2605.7 (12570.4)</td>
<td>-179.9</td>
<td>1500 (382.5***</td>
<td>1117.5 (1843.9)</td>
<td>382.5***</td>
<td>1843.9</td>
</tr>
</tbody>
</table>

Notes: This table shows the comparison of parents’ and children’s beliefs about wage premia from schooling and the derived average yearly rate of returns to schooling. For differences in means, we use t-tests. For differences in medians, we use signed-rank tests on matched data. *, **, *** indicate respectively 10%, 5%, and 1% level of significance. The national empirical average is the average wage level in Brazil for the two levels of schooling according to the Brazilian National Household Survey (PNAD 2007). The levels are updated using the Brazilian consumer price index (IPCA).
<table>
<thead>
<tr>
<th>Respondent</th>
<th>Parent</th>
<th>Child</th>
<th>Diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Did the child miss any day of school this year? (% answering “yes”)</td>
<td>75.60</td>
<td>85.58</td>
<td>-9.98***</td>
</tr>
<tr>
<td>How many days did the child miss this year?</td>
<td>4.8</td>
<td>5.16</td>
<td>-0.36</td>
</tr>
<tr>
<td>Did the child miss any day of school in the last two months? (% answering “yes”)</td>
<td>50.96</td>
<td>56.04</td>
<td>-5.08</td>
</tr>
<tr>
<td>How many days did the child miss in the last two months?</td>
<td>1.36</td>
<td>1.97</td>
<td>-0.61 **</td>
</tr>
<tr>
<td>Did the child miss any day this year because she was sick? (% answering “yes”)</td>
<td>42.99</td>
<td>31.78</td>
<td>11.21 ***</td>
</tr>
<tr>
<td>...any day because she did not want to go? (% answering “yes”)</td>
<td>8.88</td>
<td>15.42</td>
<td>-6.54 ***</td>
</tr>
</tbody>
</table>

*** 1% significant; ** 5% significant - T-test of equality in means from paired observations (parent and child).
Table 5: Comparison of means and medians of outcome variables across treatments

<table>
<thead>
<tr>
<th>Outcome Variable</th>
<th>Baseline (N=61)</th>
<th>Don’t tell (N=47)</th>
<th>Text message (N=52)</th>
<th>Non-classroom (N=53)</th>
<th>Signaling (N=31)</th>
<th>All components (N=30)</th>
</tr>
</thead>
<tbody>
<tr>
<td>% Demanding conditionality</td>
<td>88.5</td>
<td>37.5***</td>
<td>30.8***</td>
<td>67.9***</td>
<td>90.3</td>
<td>93.3</td>
</tr>
<tr>
<td>% Paying for conditionality</td>
<td>82</td>
<td>18.7***</td>
<td>21.1***</td>
<td>62.3***</td>
<td>71</td>
<td>90</td>
</tr>
<tr>
<td>Average threshold (extrapolated)</td>
<td>201</td>
<td>115.9***</td>
<td>117.9***</td>
<td>146.7***</td>
<td>187.4</td>
<td>204.8</td>
</tr>
<tr>
<td>Median threshold (extrapolated)</td>
<td>185.3</td>
<td>115.5***</td>
<td>117.4***</td>
<td>146.4***</td>
<td>191.5</td>
<td>193.6</td>
</tr>
<tr>
<td>Average threshold (censored)</td>
<td>157.3</td>
<td>117.2***</td>
<td>116.8***</td>
<td>135***</td>
<td>157.7</td>
<td>157.8</td>
</tr>
<tr>
<td>Median threshold (censored)</td>
<td>190.8</td>
<td>115***</td>
<td>115***</td>
<td>142.5**</td>
<td>180</td>
<td>180</td>
</tr>
</tbody>
</table>

For the means, we perform t-tests comparing the means of each outcome variable in each treatment to the ones in the Baseline treatment. For medians, we perform Wilcoxon-Mann-Whitney rank sum tests comparing them to the medians in the Baseline treatment.

*** 1% significant; ** 5% significant (when compared to the Baseline treatment).
Table 6: **Regressions - Treatment Effects**

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Dummy: parent prefers R$ 120 CCT to...</th>
<th>CT indifferent to R$ 120 CCT</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Demands <strong>conditionality</strong></td>
<td>Threshold</td>
</tr>
<tr>
<td></td>
<td>OLS</td>
<td>Tobit</td>
</tr>
<tr>
<td>Don’t tell Treatment dummy</td>
<td>-0.44</td>
<td>-55.934</td>
</tr>
<tr>
<td></td>
<td>[0.102]**</td>
<td>[14.324]**</td>
</tr>
<tr>
<td>Text message Treatment dummy</td>
<td>-0.531</td>
<td>-54.837</td>
</tr>
<tr>
<td></td>
<td>[0.132]**</td>
<td>[15.134]**</td>
</tr>
<tr>
<td>Non-classroom Treatment dummy</td>
<td>-0.143</td>
<td>-34.596</td>
</tr>
<tr>
<td></td>
<td>[0.097]</td>
<td>[9.984]**</td>
</tr>
<tr>
<td>Higher show-up fee dummy</td>
<td>-0.018</td>
<td>13.269</td>
</tr>
<tr>
<td></td>
<td>[0.123]</td>
<td>[30.309]</td>
</tr>
<tr>
<td>Age (parent)</td>
<td>-0.004</td>
<td>-1.357</td>
</tr>
<tr>
<td></td>
<td>[0.006]**</td>
<td>[0.567]**</td>
</tr>
<tr>
<td>Age (child)</td>
<td>0.024</td>
<td>7.524</td>
</tr>
<tr>
<td></td>
<td>[0.037]</td>
<td>[6.197]</td>
</tr>
<tr>
<td>Male parent dummy</td>
<td>0.019</td>
<td>-27.049</td>
</tr>
<tr>
<td></td>
<td>[0.266]</td>
<td>[23.711]</td>
</tr>
<tr>
<td>Male child dummy</td>
<td>0.035</td>
<td>-1.594</td>
</tr>
<tr>
<td></td>
<td>[0.105]</td>
<td>[12.247]</td>
</tr>
<tr>
<td>Weekly discount factor (parent)</td>
<td>0.083</td>
<td>7.761</td>
</tr>
<tr>
<td></td>
<td>[0.137]</td>
<td>[27.224]</td>
</tr>
<tr>
<td>Weekly discount factor (child)</td>
<td>-0.069</td>
<td>-47.944</td>
</tr>
<tr>
<td></td>
<td>[0.165]</td>
<td>[33.914]</td>
</tr>
<tr>
<td>Log Household Income</td>
<td>0.04</td>
<td>15.249</td>
</tr>
<tr>
<td></td>
<td>[0.071]</td>
<td>[4.493]**</td>
</tr>
<tr>
<td>Observations</td>
<td>208</td>
<td>208</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.49</td>
<td>0.52</td>
</tr>
<tr>
<td>Mean of dep. var. in Baseline Treatment</td>
<td>0.88</td>
<td>0.82</td>
</tr>
</tbody>
</table>

Robust standard errors (clustered by school) in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Additional controls: time-inconsistency discount factor [beta] (for parent and child), marital status (parent), employed parent dummy, employed spouse dummy, religion dummies, number of children in the household, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.
<table>
<thead>
<tr>
<th>Dependent Variable Dummy: parent prefers R$ 120 CCT to...</th>
<th>R$ 120 CT</th>
<th>R$ 125 CT</th>
<th>Conditionality</th>
<th>Conditionality</th>
<th>Threshold</th>
<th>Threshold</th>
</tr>
</thead>
<tbody>
<tr>
<td>Demands Pays for conditionality</td>
<td>OLS</td>
<td>OLS</td>
<td>Tobit</td>
<td>OLS (censored)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Don’t tell Treatment dummy</td>
<td>-0.441</td>
<td>-0.584</td>
<td>-56.8</td>
<td>-31.175</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.094]**</td>
<td>[0.068]**</td>
<td>[16.517]**</td>
<td>[10.554]**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Text message Treatment dummy</td>
<td>-0.534</td>
<td>-0.572</td>
<td>-61.468</td>
<td>-35.303</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.124]**</td>
<td>[0.096]**</td>
<td>[16.021]**</td>
<td>[9.662]**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Non-classroom Treatment dummy</td>
<td>-0.169</td>
<td>-0.182</td>
<td>-41.907</td>
<td>-20.823</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.090]*</td>
<td>[0.075]**</td>
<td>[12.588]**</td>
<td>[5.972]**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Signaling Treatment dummy</td>
<td>0.031</td>
<td>-0.117</td>
<td>-5.707</td>
<td>-0.45</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.092]</td>
<td>[0.155]</td>
<td>[22.841]</td>
<td>[10.709]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All components Treatment dummy</td>
<td>0.09</td>
<td>0.123</td>
<td>7.289</td>
<td>2.996</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.060]</td>
<td>[0.058]*</td>
<td>[15.908]</td>
<td>[7.226]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Higher show-up fee dummy</td>
<td>0.034</td>
<td>-0.136</td>
<td>25.94</td>
<td>9.937</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.105]</td>
<td>[0.157]</td>
<td>[28.377]</td>
<td>[13.982]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age (parent)</td>
<td>-0.002</td>
<td>-0.009</td>
<td>-1.275</td>
<td>-0.585</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.006]</td>
<td>[0.003]**</td>
<td>[0.555]**</td>
<td>[0.259]**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age (child)</td>
<td>0.012</td>
<td>0.021</td>
<td>5.671</td>
<td>3.261</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.033]</td>
<td>[0.031]</td>
<td>[4.733]</td>
<td>[2.693]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male parent dummy</td>
<td>0.026</td>
<td>-0.089</td>
<td>-20.342</td>
<td>-8.173</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.212]</td>
<td>[0.118]</td>
<td>[20.110]</td>
<td>[11.698]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male child dummy</td>
<td>0.027</td>
<td>0.006</td>
<td>6.768</td>
<td>4.611</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.098]</td>
<td>[0.120]</td>
<td>[12.037]</td>
<td>[8.126]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Weekly discount factor (parent)</td>
<td>0.113</td>
<td>0.084</td>
<td>-14.533</td>
<td>-6.278</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.123]</td>
<td>[0.187]</td>
<td>[24.126]</td>
<td>[13.705]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Weekly discount factor (child)</td>
<td>-0.011</td>
<td>-0.019</td>
<td>-22.391</td>
<td>-11.245</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.143]</td>
<td>[0.197]</td>
<td>[30.806]</td>
<td>[17.271]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log Household Income</td>
<td>0.039</td>
<td>0.126</td>
<td>17.074</td>
<td>9.193</td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0.074]</td>
<td>[0.067]*</td>
<td>[6.132]**</td>
<td>[3.704]**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>267</td>
<td>267</td>
<td>267</td>
<td>267</td>
<td></td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>0.5</td>
<td>0.48</td>
<td>0.82</td>
<td>0.43</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean of dep. var. in Baseline Treatment</td>
<td>0.88</td>
<td>0.82</td>
<td>196.5</td>
<td>157.3</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Robust standard errors (clustered by school) in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Additional controls: time-inconsistency discount factor ($\beta$) (for parent and child), marital status (parent), employed parent dummy, employed spouse dummy, religion dummies, number of children in the household, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.
Table 8: Text message Treatment Interactions

<table>
<thead>
<tr>
<th>Treatment Interactions</th>
<th>$R, 120, CT$</th>
<th>$R, 125, CT$</th>
<th>Threshold (Tobit)</th>
<th>Threshold OLS (censored)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dummy: parent prefers $R, 120, CCT$ to CT indifferent to $R, 120, CCT$</td>
<td>Demands conditionality</td>
<td>Pays for conditionality</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>How much do you know about what your child is doing?</strong></td>
<td><strong>(standardized measure)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment effect</td>
<td>-0.534</td>
<td>-0.54</td>
<td>-54.654</td>
<td>-31.877</td>
</tr>
<tr>
<td></td>
<td>[0.129]**</td>
<td>[0.096]**</td>
<td>[15.291]**</td>
<td>[10.033]**</td>
</tr>
<tr>
<td>How much do you know?</td>
<td>-0.025</td>
<td>-0.048</td>
<td>-4.076</td>
<td>-2.863</td>
</tr>
<tr>
<td></td>
<td>[0.032]</td>
<td>[0.043]</td>
<td>[4.655]</td>
<td>[3.125]</td>
</tr>
<tr>
<td>Interaction term</td>
<td>0.045</td>
<td>0.087</td>
<td>11.412</td>
<td>6.77</td>
</tr>
<tr>
<td></td>
<td>[0.056]</td>
<td>[0.048]</td>
<td>[4.387]**</td>
<td>[2.704]**</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.49</td>
<td>0.53</td>
<td></td>
<td>0.46</td>
</tr>
</tbody>
</table>

Observations: 208 208 208 208
Mean of dep. var. in Baseline Treatment: 0.88 0.82 201 157.3

Robust standard errors (clustered by school) in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Additional controls: Don’t tell and Non-classroom treatment dummies, higher show-up fee dummy, age (parent and child) male indicator (parent and child), weekly discount factor (parent and child), log household income, time-inconsistency discount factor [beta] (for parent and child), marital status (parent), employed parent dummy, employed spouse dummy, religion dummies, number of children in the household, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.
Table 9: Non-classroom Treatment Interactions

<table>
<thead>
<tr>
<th></th>
<th>Dummy: parent prefers R$ 120 CCT to CT indifferent to R$ 120 CCT</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>R$ 120 CT Demands conditionality</td>
</tr>
<tr>
<td>A) How much do you know about what your child is doing? (standardized measure)</td>
<td></td>
</tr>
<tr>
<td>Treatment effect</td>
<td>-0.145 (0.097)</td>
</tr>
<tr>
<td>How much do you know?</td>
<td>0.021 (0.030)</td>
</tr>
<tr>
<td>Interaction term</td>
<td>-0.124 (0.067)*</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.50</td>
</tr>
<tr>
<td>B) District-level crime occurrences per 1,000 hab. (standardized measure)</td>
<td></td>
</tr>
<tr>
<td>And gender of the child</td>
<td></td>
</tr>
<tr>
<td>Treatment effect</td>
<td>-0.059 (0.092)</td>
</tr>
<tr>
<td>District crime rate</td>
<td>-0.069 (0.026)**</td>
</tr>
<tr>
<td>Male child</td>
<td>0.123 (0.089)</td>
</tr>
<tr>
<td>Treatment*District crime rate</td>
<td>0.022 (0.105)</td>
</tr>
<tr>
<td>Treatment*Male child dummy</td>
<td>-0.232 (0.191)</td>
</tr>
<tr>
<td>Treat.<em>Male child dummy</em>Crime rate</td>
<td>0.141 (0.158)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.51</td>
</tr>
<tr>
<td>Observations</td>
<td>208</td>
</tr>
<tr>
<td>Mean of dep. var. in Baseline Treatment</td>
<td>0.88</td>
</tr>
</tbody>
</table>

Robust standard errors (clustered by school) in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Additional controls: Don’t tell and Text message treatment dummies, higher show-up fee dummy, age (parent and child), male indicator (parent and child—for panel A), weekly discount factor (parent and child), log household income, time-inconsistency discount factor [beta] (for parent and child), marital status (parent), employed parent dummy, employed spouse dummy, religion dummies, number of children in the household, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.
A Appendix Tables

Table A.1: Representativeness of Families

<table>
<thead>
<tr>
<th>Mean levels of observables</th>
<th>All eligible families (government dataset)</th>
<th>Families in the experiment (government dataset)</th>
<th>Families in the experiment (expt measures)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Guardian's age</td>
<td>41.1</td>
<td>40.4</td>
<td>40.1</td>
</tr>
<tr>
<td>Female guardian - %</td>
<td>91.9</td>
<td>99</td>
<td>96.2</td>
</tr>
<tr>
<td>Guardian's schooling</td>
<td>4.2</td>
<td>3.6</td>
<td>5.8</td>
</tr>
<tr>
<td>Guardian's race - black - %</td>
<td>7</td>
<td>5.7</td>
<td>19</td>
</tr>
<tr>
<td>Guardian's race - mestizo - %</td>
<td>74.8</td>
<td>77.9</td>
<td>58.1</td>
</tr>
<tr>
<td>Guardian's race - white - %</td>
<td>17.8</td>
<td>15.9</td>
<td>21.4</td>
</tr>
<tr>
<td>HH per capita income</td>
<td>64.9</td>
<td>64.9</td>
<td>86.7</td>
</tr>
<tr>
<td>HH size</td>
<td>5.1</td>
<td>5.4</td>
<td>5.15</td>
</tr>
<tr>
<td>Female child</td>
<td>49.8</td>
<td>53.2</td>
<td>56.7</td>
</tr>
<tr>
<td>Number of Families</td>
<td>62,113</td>
<td>210</td>
<td>210</td>
</tr>
</tbody>
</table>

The first two columns are drawn from the government official dataset of families in the Bolsa-Escola program. The last column refers to measurements of the same variables in the experiment.

Table A.2: Session Show-Up Rates

<table>
<thead>
<tr>
<th>School Name</th>
<th>Show-up Fee</th>
<th>Participated</th>
<th>Invited</th>
<th>Yield</th>
</tr>
</thead>
<tbody>
<tr>
<td>CEF 01</td>
<td>7</td>
<td>46</td>
<td>52</td>
<td>0.88</td>
</tr>
<tr>
<td>CEF 02</td>
<td>7</td>
<td>16</td>
<td>20</td>
<td>0.80</td>
</tr>
<tr>
<td>CEF 03</td>
<td>7</td>
<td>11</td>
<td>13</td>
<td>0.85</td>
</tr>
<tr>
<td>CEF 101</td>
<td>7</td>
<td>13</td>
<td>15</td>
<td>0.87</td>
</tr>
<tr>
<td>CEF 104</td>
<td>7</td>
<td>14</td>
<td>17</td>
<td>0.82</td>
</tr>
<tr>
<td>CEF 13</td>
<td>7</td>
<td>7</td>
<td>10</td>
<td>0.70</td>
</tr>
<tr>
<td>CEF 20</td>
<td>7</td>
<td>32</td>
<td>35</td>
<td>0.91</td>
</tr>
<tr>
<td>CEF Darcy Ribeiro</td>
<td>7</td>
<td>12</td>
<td>14</td>
<td>0.86</td>
</tr>
<tr>
<td>Cedlan</td>
<td>7</td>
<td>14</td>
<td>17</td>
<td>0.82</td>
</tr>
<tr>
<td>CEF 16</td>
<td>10</td>
<td>22</td>
<td>23</td>
<td>0.96</td>
</tr>
<tr>
<td>CEF 17</td>
<td>10</td>
<td>23</td>
<td>25</td>
<td>0.92</td>
</tr>
<tr>
<td>Total</td>
<td>7</td>
<td>165</td>
<td>193</td>
<td>0.85</td>
</tr>
<tr>
<td>Total</td>
<td>10</td>
<td>45</td>
<td>48</td>
<td>0.94</td>
</tr>
</tbody>
</table>
Table A.3: *Don’t Tell* Treatment Interactions

<table>
<thead>
<tr>
<th>Dummy: parent prefers R$ 120 CCT to</th>
<th>CT indifferent to R$ 120 CCT</th>
</tr>
</thead>
<tbody>
<tr>
<td>R$ 120 CT</td>
<td></td>
</tr>
<tr>
<td>Demands</td>
<td></td>
</tr>
<tr>
<td>Pay for</td>
<td></td>
</tr>
<tr>
<td>Threshold</td>
<td></td>
</tr>
<tr>
<td>OLS (censored)</td>
<td></td>
</tr>
</tbody>
</table>

Don’t tell treatment interaction

*(parent’s time-inconsistency discount factor)*

*(standardized measure)*

<table>
<thead>
<tr>
<th>Treatment Effect</th>
<th>-0.496</th>
<th>-0.639</th>
<th>-59.158</th>
<th>-32.774</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>[0.102]***</td>
<td>[0.063]***</td>
<td>[13.557]***</td>
<td>[9.236]***</td>
</tr>
<tr>
<td>Beta</td>
<td>0.032</td>
<td>0.049</td>
<td>9.601</td>
<td>3.804</td>
</tr>
<tr>
<td></td>
<td>[0.053]</td>
<td>[0.043]</td>
<td>[7.444]</td>
<td>[3.762]</td>
</tr>
<tr>
<td>Interaction term</td>
<td>-0.282</td>
<td>-0.345</td>
<td>-18.217</td>
<td>-13.646</td>
</tr>
<tr>
<td></td>
<td>[0.147]*</td>
<td>[0.118]**</td>
<td>[14.376]</td>
<td>[10.515]</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.54</td>
<td>0.54</td>
<td>0.46</td>
<td>0.46</td>
</tr>
</tbody>
</table>

Observations: 208 208 208 208
Mean of dependent var. in Baseline Treatment: 0.88 0.82 192.1 157.6

Robust standard errors (clustered by school) in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Additional controls: Text message and Non-classroom treatment dummies, higher show-up fee dummy, age (parent and child), male indicator (parent and child), weekly discount factor (parent and child), log household income, marital status (parent), employed parent dummy, employed spouse dummy, religion dummies, number of children in the household, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.

Beta refers to the time-inconsistency discount factor. It is the ratio between the time-discount factor of now vs one week and the discount factor of one week vs two weeks. Therefore beta less than one refers to quasi-hyperbolic time preferences.
Table A.4: Robustness Checks Regressions - Treatment Effects adding Follow-up Treatments

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Dummy: parent prefers R$ 120 CCT to...</th>
<th>CT indifferent to R$ 120 CCT</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>R$ 120 CT</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Demands conditionality</td>
<td></td>
</tr>
<tr>
<td></td>
<td>OLS</td>
<td>R$ 125 CT</td>
</tr>
<tr>
<td></td>
<td>OLS</td>
<td>OLS</td>
</tr>
<tr>
<td></td>
<td>Tobit</td>
<td>OLS (censored)</td>
</tr>
<tr>
<td>Don’t tell Treatment dummy</td>
<td>-0.442 [0.088]***</td>
<td>-0.595 [0.065]***</td>
</tr>
<tr>
<td>Text message Treatment dummy</td>
<td>-0.53 [0.116]***</td>
<td>-0.578 0.088***</td>
</tr>
<tr>
<td>Non-classroom Treatment dummy</td>
<td>-0.173 [0.090]*</td>
<td>-0.192 [0.069]**</td>
</tr>
<tr>
<td>Signaling Treatment dummy</td>
<td>0.038 [0.152]</td>
<td>0.023 0.136***</td>
</tr>
<tr>
<td>All components Treatment dummy</td>
<td>0.098 [0.108]</td>
<td>0.256 [0.149]</td>
</tr>
<tr>
<td>Follow-up treatment dummy</td>
<td>-0.008 [0.126]</td>
<td>-0.136 [0.140]</td>
</tr>
<tr>
<td>Follow-up dummy*Non-classroom treatment</td>
<td>0.223 [0.148]</td>
<td>0.275 [0.186]</td>
</tr>
<tr>
<td>Higher show-up fee dummy</td>
<td>-0.003 [0.117]</td>
<td>-0.139 [0.158]</td>
</tr>
<tr>
<td>Age (parent)</td>
<td>-0.003 [0.066]</td>
<td>-0.012 [0.033]***</td>
</tr>
<tr>
<td>Age (child)</td>
<td>0.021 [0.031]</td>
<td>0.033 [0.028]***</td>
</tr>
<tr>
<td>Male parent dummy</td>
<td>0.024 [0.026]</td>
<td>-0.064 [0.114]</td>
</tr>
<tr>
<td>Male child dummy</td>
<td>0.024 [0.097]</td>
<td>0.006 [0.112]</td>
</tr>
<tr>
<td>Weekly discount factor (parent)</td>
<td>0.102 [0.108]</td>
<td>0.059 [0.169]</td>
</tr>
<tr>
<td>Weekly discount factor (child)</td>
<td>-0.02 [0.124]</td>
<td>0.011 [0.158]</td>
</tr>
<tr>
<td>Log Household Income</td>
<td>0.07 [0.081]</td>
<td>0.134 [0.071]*</td>
</tr>
<tr>
<td>Observations</td>
<td>296</td>
<td>296</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.5</td>
<td>0.47</td>
</tr>
<tr>
<td>Mean of dependent var. in Baseline Treatment</td>
<td>0.88</td>
<td>0.82</td>
</tr>
</tbody>
</table>

Robust standard errors (clustered by school) in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Additional controls: time-inconsistency discount factor [beta] (for parent and child), marital status (parent), employed parent dummy, employed spouse dummy, religion dummies, number of children in the household, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.
Dados Pessoais:

Qual é seu nome?

_Sua família atualmente recebe 120 reais por mês e em troca seu filho ou filha é obrigado(a) a ir à escola com um mínimo de 85% de frequência escolar. Sabendo disto, perguntamos: se vocês pudessem escolher um tipo de bolsa para o segundo semestre deste ano, qual seria sua preferência para cada uma das opções abaixo?_

Lembramos que haverá um sorteio. 5% das famílias serão sorteadas, iremos sortear uma das perguntas e você receberá de Setembro a Dezembro de 2009 o que tiver escolhido para ela. Então é melhor para você dizer o que você realmente prefere porque você tem chance de ganhá-lo. Lembrando que o pagamento será feito a quem já o recebe hoje em dia.

Também lembramos que seu filho ficará sabendo de sua escolha e da mudança no pagamento, caso esta ocorra.

- ○ Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
- ○ Receber 125 reais por mês sem obrigação de frequência escolar para o pagamento (recebe o pagamento de qualquer maneira, independente da frequência escolar)

- ○ Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
- ○ Receber 130 reais por mês sem obrigação de frequência escolar para o pagamento

- ○ Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
- ○ Receber 135 reais por mês sem obrigação de frequência escolar para o pagamento

- ○ Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
- ○ Receber 140 reais por mês sem obrigação de frequência escolar para o pagamento

- ○ Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
Receber 145 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou  
  o Receber 150 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou  
  o Receber 155 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou  
  o Receber 165 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou  
  o Receber 170 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou  
  o Receber 175 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o Continuar recebendo 120 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou  
  o Receber 180 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o Receber 125 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou  
  o Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

-  
  o
Receber 130 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 135 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 140 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 145 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 150 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 155 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 160 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 165 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 170 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
○ Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento
- Receber 175 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
- Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento

- Receber 180 reais por mês, mantendo a obrigação de frequência escolar para o pagamento, ou
- Continuar recebendo 120 reais por mês sem obrigação de frequência escolar para o pagamento