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

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Abstract

This paper experimentally analyzes the schooling decisions of poor households with adolescent children in urban Brazil. 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' incentivized 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 either (i) their child is not informed that the conditionality has been dropped or (ii) if they are offered to receive free 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 are consistent with parental preferences not just to keep the children in the classroom but also 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.[1] However, economic models typically view intrahousehold dynamics and, particularly, parent-child conflict as a secondary element in the process.[2] This paper examines the extent to which parent-child conflict and intrahousehold agency problems play a central role in schooling decisions of poor households with adolescent children in urban Brazil. In particular, we investigate how parental ability to monitor their children's school attendance behavior and the divergence in preferences for schooling across generations of the same household can be key factors in the decision-making process.

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. Based on the findings of a pilot experiment, 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.[3] 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.

[1] For the impact of schooling on income, see Angrist and Krueger (1991), Card (1995, 2001), and the surveys by Card (1999), Krueger and Lindahl (2001), and Goldin and Katz (2008), as well as Aghion and Howitt (2009) on the macroeconomics side. For schooling externalities, see Acemoglu and Angrist (2000). See Lochner and Moretti (2004) for the effect on criminal incarceration, Milligan, Moretti and Oreopoulos (2004) on political participation, and Lleras-Muney (2005) on mortality rates. In developing countries, see, for instance, Duflo (2001) and Psacharopoulos (1985, 1994), on returns to schooling, and Schultz (1997, 2002), on the effect of schooling on health and fertility.

[2] The 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).

[3] The pilot experiment was intended to glean a general understanding of intrahousehold dynamics in the schooling decision and did not have this particular model in mind. For a discussion of the design and findings of the pilot, see the Supplementary Appendix to this paper.

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 setup of the existing local conditional cash transfer (CCT) program, *Bolsa-Escola Vida Melhor* (roughly, "School Stipend, Better Life"). In the program, at the time of the intervention, families with monthly per capita household incomes below half of the Brazilian level of minimum wage (approximately US$ 120 per month)[4] received a transfer of R$ 120 per month conditional upon one of their children attending school 85% of days in that month.[5]

In working with the Secretariat of Education of the *Distrito Federal* state,[6] we are able to 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 randomly chosen to be their actual payments from the local government between September and December 2009 for that child. By analyzing parents' choices under these real stakes, we elicit their preferences for schooling, the relevance of moral hazard problems inside the household, and shed light on their relative valuation of different benefits provided by school (such as safety and 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

[4]In a previous version of this paper, we referred to a lower level of the per capita income upper bound of R$ 100 (approximately US$ 50), previously used by the local government.

[5]Although some CCT programs are said to suffer from enforcement, *Bolsa-Escola Vida Melhor* has a strong concern for enforcing the conditionality, which includes random visits of government officers to schools to check attendance and compliance to the rules.

[6]Contract number: Termo de Cooperacao N. 05/2009, Distrito Federal, Secretaria de Estado de Educacao. For a digital copy of the contract, see the Supplementary Appendix to this paper.

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 six percent of their monthly household 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 setup, we also shed light on 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 also indicates 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 or perhaps 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 one more additional treatment.

The *Non-classroom* treatment exploits the fact that public schools in the area we study 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 he or 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. In this treatment, the willingness to pay for the conditionality remains high, indicating parents substantially value the non-classroom content of schooling.

These results have important policy implications, particularly in developing countries such as Brazil where educational attainment and school attendance are 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 high levels of returns to schooling in that country)[7] or cannot afford to send their children to attend school because they need the children to provide resources for the household).[8] 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

[7]The 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).

[8]See for instance Basu and Van (1997), and Basu and Tzannatos (2006).

information and monitoring on their children's actions could be an effective policy option to increase school attendance.[9] These findings are consistent with recent work in the literature. Jensen (2010) provides evidence that in the Dominican Republic the perceived returns to schooling by eigth-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 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.[10] Berry (2009) provides evidence of a differential impact of incentives for test scores and attendance depending on whether the recipient is the parent or the child, and depending on characteristics of the parent, such as the parent's level of education and time availability. 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

[9]The 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.

[10]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).

2001, and Gatti, 2005).[11] 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.[12] Conditional cash transfer programs are a widespread phenomenon in developing countries: in 2010, 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[13] or because they might commit parents to keep their children in school.[14] 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.[15]

[11] 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. Chiappori (1992) and Browning and Chiappori (1998) provide a collective model of the household. Lizzeri and Siniscalchi (2008) characterize optimal parenting policies in a model of parental guidance and supervised learning.

[12] 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 effect on sexual behavior of recipient children, see Baird, Chirwa, McIntosh, and Ozler (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), and Baird, McIntosh, and Ozler (2009). For the effect of CCTs on child labor supply when the household is exposed to shocks, see de Janvry, Finan, Sadoulet, and Vakis (2006).

[13] See Gahvari and Mattos (2007), following arguments made by Besley and Coate (1992) and Zeckhauser (1971).

[14] 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.

[15] A related literature studies private responses to public transfers (e.g. Moffitt, 1992, and Rosenzweig and Wolpin, 1994).

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% 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 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-Familia*, 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, was 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.[16] 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.[17] The maximum payment per month is R$ 180 per family.

3 Theoretical framework

To organize our analysis, we develop a simple model of the schooling decision in the household, which is a viewed as binary-choice moral hazard problem involving one child (the agent of the choice) and one parent.

[16] If the household has no mother, the payment is made to the father or another adult responsible for the children.

[17] A family would receive R$ 0 if any child missed more than 15% of days that month. Our experiment only potentially removed a conditionality worth R$ 120 from the child present.

3.1 The model

In the model, 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 so with certainty. If the child misses school, the parent observes the child's true action with probability π.[18]

3.1.1 Timeline

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.[19]

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^{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^{child}) and her perceived cost (γ). We assume that this difference is negative so the child faces a net private cost of attending school, $c \equiv \gamma - V^{child} > 0$. In the results section, we will provide experimental evidence consistent with this last assumption.

[18] 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. Our results hold if we assume lack observability in both actions instead.

[19] One of the experimental treatments, the *Don't tell* treatment, provides evidence consistent with this assumption.

Letting $I_{S=j}$ be the indicator that the child chooses $S = j$, we can write the expected utility of the child as:

$$E[U^{child}] = I_{S=1}\{x_1 - c\} + I_{S=0}\{\pi x_0 + (1-\pi)x_1\}. \quad (1)$$

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

$$x_1 - x_0 \geq c/\pi. \quad (2)$$

The parent is assumed to be altruistic toward her child but discounts her child's utility by $\alpha \in [0,1)$.[20] 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).[21] One of our experimental treatments provides evidence consistent with the assumption that $V > 0$.

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

$$E[U^{parent}] = I_{S=1}\{(Y - x_1) + \alpha(x_1 - c) + V\} + I_{S=0}\{\pi[(Y - x_0) + \alpha x_0] + (1-\pi)[(Y - x_1) + \alpha x_1]\}. \quad (3)$$

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 π the parent pays x_0 to the child, and with probability $(1-\pi)$ the parent pays x_1. In either case, the parent does not derive benefit from school attendance

[20] 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.

[21] In our results section, we provide evidence that in our sample children have lower perceptions of the returns to schooling than their parents do.

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 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 (rearranging terms to facilitate the interpretation):

$$\max_{x_1,x_0} I_{S=0}\{\pi[Y-(1-\alpha)x_0]+(1-\pi)[Y-(1-\alpha)x_1]\}$$

$$\text{s.t. } x_1 - x_0 \leq c/\pi,$$

$$x_0, x_1 \geq 0.$$

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 $\{U^{parent*}|S^*=0\} = Y$.

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:

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

$$\text{s.t. } x_1 - x_0 \geq c/\pi,$$

$$x_0 \geq 0.$$

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

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. $V\geq\overline{V}$).

Proposition 2 The role of monitoring: $\partial\,\overline{V}/\partial\pi<0$.

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.[22]

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* π *is unchanged.*

In our experiment, we want to derive the size $\overline{T}_{CT}$ of a cash transfer that would make the parent indifferent to a conditional cash transfer of size $\overline{T}_{CCT}$.

Proposition 3 *Suppose* $\pi < 1$. *Hence* $c < \overline{V}$ *and the following holds:*

1. *If* $V \leq c - \overline{T}_{CCT}$, *then* $\overline{T}_{CT} = 0$. *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* $c - \overline{T}_{CCT} < V < c$, *then* $\overline{T}_{CT} = \overline{T}_{CCT} + (V - c) < \overline{T}_{CCT}$. *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* $\overline{T}_{CT} = \overline{T}_{CCT}$. *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* $c < V < \overline{V}$, *then* $\overline{T}_{CT} = \overline{T}_{CCT} + (V - c) > \overline{T}_{CCT}$. *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.*

[22] In the actual setup that we study, the conditionality is a monthly 85% attendance rate. However, given the structure of the model, we assume for simplicity that the parent observes the child's action with certainty under the CCT scheme.

5. *If $V \geq \overline{V}$, then $\overline{T}_{CT} = \overline{T}_{CCT} + (\overline{V} - c) > \overline{T}_{CCT}$. 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 the Supplementary Appendix.

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 policy-maker is to induce school attendance, then:

1. The minimum size of the transfer of either type of policy has to be $\underline{T} \equiv \overline{Y} - Y$.

2. Conditional on meeting the minimum required size of the transfer $\underline{T}$, increasing monitoring π in either policy instrument will help promote school attendance.[23]

We assume that all values of the transfers offered to the parents in our experiment are greater than $\underline{T}$.

In the next sections of the paper, we use a setup 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, which we suppose to be symmetrically distributed with mean $\hat{V}$ and cumulative distribution function

[23] 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.

$\psi(.)$. 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.

4 The experiment

4.1 The setup

We surveyed 210 families, all enrolled in the CCT program (*Bolsa-Escola Vida Melhor*).[24] For each family, we interviewed one parent and one 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 these children may have already formed individual preferences, some bargaining power in the household and an outside option to schooling.[25] 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 representative-

[24]The agreement with the local government was made possible with the help of the local NGO *Missao Crianca*.

[25]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.

ness of our sample, see the Supplementary Appendix). We only included children who had no older CCT-eligible siblings to ensure that a family would only be invited once. 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 offered either R$ 7 or R$ 10 to attend the study.[26] The average show-up rate in our study was 87% (see the Supplementary Appendix for details on show-up rates).

When participants arrived, each family was randomly assigned into one of the treatments described in the next four subsections. 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 in a separate room. If there were no free computers, the parent would wait as well.[27] One surveyor was assigned to each participant to read the survey questions.[28] Only clarifying questions asked by participants were answered by surveyors. All clarifying questions regarding the treatment questions were addressed by the author conducting the experiment. Surveyors were randomly ordered at the beginning of the day and assigned according to availability throughout the rest of the day. In every treatment, the parents would complete their portion of the experiment first. The children would go second, with the parents leaving the room. In some of the treatments (as described below), there was a joint decision-making portion, which followed the children's part.

In any treatment, the experiment began with the surveyor offering the parent the opportunity to choose a new cash transfer program. There were 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, to the same parent. 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

[26] See a sample invitation letter and its English version in the Supplementary Appendix.
[27] If there was a long wait, subjects would play BINGO for small prizes.
[28] Surveyors were all undergraduate students from the University of Brasilia.

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. First, the questions 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:

Which Monthly Payment Would Your Prefer?		
R$ 120 Conditional on Attendance	OR	R$ 120 Unconditionally
R$ 120 Conditional on Attendance	OR	R$ 125 Unconditionally
⋮		
R$ 120 Conditional on Attendance	OR	R$ 180 Unconditionally

Secondly, the questions 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:[29]

Which Monthly Payment Would Your Prefer?		
R$ 125 Conditional on Attendance	OR	R$ 120 Unconditionally
R$ 130 Conditional on Attendance	OR	R$ 120 Unconditionally
⋮		
R$ 180 Conditional on Attendance	OR	R$ 120 Unconditionally

To minimize time spent on this procedure in these time-intensive surveys, we made one important design decision: as soon as a parent indicated a switch in preference from a conditional transfer to an unconditional transfer (or vice-versa), the research assistant would presume the parent similarly preferred all unconditional (or conditional) transfers worth more. That is, if for instance the parent states a preference for R$ 130 unconditional to R$ 120 conditional, then the surveyor would assume

[29]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.

the parent would also prefer any amount from R$ 135 to R$ 180 unconditional to R$ 120 conditional. In many experiments revealed choices are not necessarily monotonic; thus, reported levels of willingness to pay for conditional transfers may be understated. However, the results section relies on cross-treatment analyses rather than emphasizing point estimates. The concern would only hold if the assumption interacted with the treatment effects.

Each treatment used these same twenty-five conditional versus unconditional transfer questions.[30] Parents were informed that 5% of participants would have one of their decisions implemented and that decision would be randomly chosen from the 25 questions.[31] 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 June and July 2009.[32] The experiment was conducted at computer terminals using a web-based survey.[33] 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.

[30] See the actual set of twenty-five questions and text in Portuguese used in each treatment and their translated versions in English in the Supplementary Appendix.

[31] Hence, this is a version of the Becker DeGroot Marschak elicitation procedure, which incentivizes truthful reporting of willingness to pay.

[32] We 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 the Supplementary Appendix. Two additional experimental treatments designed to further analyze what drives parental valuation of schooling were performed over the telephone after the implementation of the main experiment, in September. Here, we do not report the results from these two treatments since these treatments were implemented in a different format (via telephone) and because their interpretations are more speculative then the remaining of the analysis. The description and results from these treatments are however reported in the working paper version of this paper.

[33] In all but one school, *CEF 20 Ceilandia* , the experiment was performed using Qualtrics' web-based survey platform. In that school, since the internet connection was slow during the intervention, an identical (content-wise), though visually different, pdf computer survey was used. Although a pdf-survey fixed effect cannot be disentangled from the school fixed effect, the data from this school and survey are very similar to the data collected in the other schools. The final results do not change if the data from that school are excluded, so they are not. The results excluding that school are available from the authors upon request.

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 have exactly the same conditionality as in the *Bolsa-Escola* program: class attendance on 85% of the days every month.[34] The choices were offered first to the parent, then the child, then jointly, but were only financially incentivized for the parents.[35] The parent was informed that the child would be informed of the choices made by the parent at the very end of the session. 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? *Don't Tell* treatment

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.[36] Thus, the children would not have any reason to believe the family would be leaving with anything other than 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 $c < \overline{V}$; whereas, under the *Baseline* treatment, she would have $\overline{V}$. If the parent pays for the conditionality, it is because $V > c$. Therefore, according to Proposition 1, shifting $\overline{V}$ 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.

[34] See the entire questionnaire used in the *Baseline* treatment and its English version in the Supplementary Appendix.
[35] The analysis of the child's and joint decisions can be found in the Supplementary Appendix.
[36] In this treatment, the child would only answer questions about demographics, preference parameters, etc.

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.

4.2.3 Monitoring and parental control: *Text message* treatment

If monitoring is a problem in the household, then providing the parent with a level of monitoring π 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.[37] The rest of the experiment proceeded identically to the baseline.

4.2.4 School as a bunker: the *Non-classroom* treatment

We also use the same experimental setup to attempt to understand what drives parental demand for schooling for their children. This treatment addresses parental valuation of the non-classroom content of school, such as keeping the child off the streets.[38] Children only attend a half-day of classes at public schools in Brasilia, either in the morning (from eight to twelve) or the afternoon (from two

[37] Only two parents in this treatment did not have a cell phone. The other fifty accepted the offer. All fifty-two are included in the analysis.

[38] 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, conditional on going to work.

to six).[39] They are welcome (but not required) to be at school during the other half of the day, but they 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 the activities performed in school during the no-class shift. There are no classes being offered and the child can do whatever he or she wants (except attending classes), as long as it occurs inside the school limits. Schools in the areas we study are generally protected by very tall fences or walls.[40] 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—a CCT of R$ 120 conditional on attendance during the classroom shift—and the alternative they had just chosen. They were not informed that they would be offered that option while they were making their initial choices in this treatment.

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:

[39] In a reduced set of public schools, children at the age range we study may attend school during an evening shift instead. In our sample, two children attend school during that shift. However, for both cases, the parent is not in the *Non-Classroom* treatment group (one parent is in the *Text message* group, and the other, in the *Don't Tell* group).

[40] See pictures of some of the schools in the Supplementary Appendix.

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 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 in the household, number of children in the household for whom the parents receive CCT payments, beta [a measure of time inconsistency discount factor] (for the parent and her child), delta [weekly discount factor] (parent and child),[41] afternoon school shift indicator, race dummies (parent and child), dummy for higher show-up fee, school and surveyor dummies, with the

[41] For a discussion on the construction and measure of time preference parameters, see the Supplementary Appendix.

standard errors clustered by school in our baseline specification.[42,43]

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 monthly wages the children could earn if they decided to drop out and work instead, and the monthly 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 parents and children seem similar (and are insignificantly different), due to two children reporting very high expectations.[44] The medians however are highly significantly different. We only have measures of the perceived *returns* to schooling, however. We do not have measures of the perceived *cost* of schooling for parents and children. Since the child is the agent actually going to school, it seems plausible that the child 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

[42]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.

[43]One of the research assistants, who was the hostess for the families that came to the study, would input her name as the surveyor as she sat down some families. However, she only conducted two interviews. For the purposes of our analysis, we consider the research assistant doing the interview to be the surveyor.

[44]If we exclude a few outliers (one child reported R$ 180,000 as the monthly wage increase from having a college degree) for most measures of returns to schooling, the difference is again significant for the means.

the average monthly 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 misestimating the actual wage levels in Brazil.[45]

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 many families, parents have to leave home very 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 does. 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."[46]

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 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),

[45] 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).

[46] 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.

henceforth their "Extrapolated threshold".[47] To further deal with the censoring problem, in the next subsection, we also present the results from quartile regressions.

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 six percent of their pre-CCT level of household monthly income. The mean level of the extrapolated threshold in this group is R$ 200.9. Due to the 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 R$ 180. We plot the cumulative distribution for the censored threshold in the *Baseline* treatment group in Figure 1.

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.[48] There

[47] Details relating to the construction of these thresholds are in the Supplementary Appendix.
[48] See the Supplementary Appendix for a discussion on the show-up rates of our study.

could also be a concern that social desirability and/or experimenter demand effects may be driving the results. We address these issues in subsection 5.2.6.

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.[49]

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 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, compared to the *Baseline* treatment. Furthermore, the extrapolated threshold is reduced by R$ 56.1 (the censored measure drops by R$ 30.4). For all four outcome variables, the treatment coefficients are significant at 1%. Figure 2 illustrates the treatment effect by plotting the cumulative

[49]Three 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.

distribution for the censored threshold in both the *Baseline* and *Don't Tell* treatment groups.[50]

In Table 7, we present the results from quartile regressions of the censored threshold. As expected, for parents with low levels of willingness to pay for the conditionality in the *Baseline* group (censored thresholds equal or lower than R$ 125), the *Don't tell* treatment would have a smaller effect (since the parents did not need the CCT device to start with). However, for parents with higher willingness to pay for the conditionality, the *Don't tell* treatment has a stronger effect, similarly to the mean regressions presented before.

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 this story is correct, providing near-perfect information to parents on their children's attendance behavior if they choose an unconditional cash transfer (i.e., shifting π 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

[50]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 the Supplementary Appendix.

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 school attendance drastically reduces the necessity for the conditional cash transfer as a monitoring device. When offered another free monitoring device (text messages), most 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$ 54.9 (the censored measure decreases by R$ 31.8). For all but one outcome variable, the treatment coefficients are significant at 1% (for the censored threshold, the p-value is 0.012). Figure 3 illustrates the treatment effect by plotting the cumulative distribution for the censored threshold in both the *Baseline* and *Text message* treatment groups. Finally, Table 7 presents the treatment effects from quartile regressions of the censored threshold variable. Here again, the treatment effects are comparable to the mean effects, with the exception of the first quartile (where parents would not demand conditional payments anyway).

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.[51]

The findings above therefore suggest that parents are willing to pay substantial sums of money for monitoring devices informing them about their child's attendance behavior. It is important to

[51]Three 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 the Supplementary Appendix); (iii) *the child cares about household income*: if the chid 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.

notice that under the *status-quo*, parents have been receiving conditional cash transfers from the local government since 1995. Parents are therefore requiring high sums of money to *drop* the conditionality. The conditionality could be thought of as an equilibrium outcome for parents' demand for more control over their child's behavior. Finally, in our study, parents were asked to report their choices privately and in the lab, without knowing who else was going to be offered to make the same choices. This could also help explain why many of them are willing to pay such large amounts for monitoring devices, since those parents were not able to coordinate and maybe develop an alternative, private solution for their common monitoring problem.

5.2.4 Schools as bunkers? - *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$ 34.3 and the censored threshold by R$ 16.3 (both significant at 1%). This suggests that a non-neglible portion of parental desire to control their child is to make sure the child attends school, and not necessarily classes.[52] Figure 4 illustrates the treatment effect by plotting the cumulative distribution for the censored threshold in both the *Baseline* and *Non-classroom* treatment groups. Finally, Table 7 presents consistent results from quartile regressions of the censored threshold variable.

[52] 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 Treatment effects and parents' perception of observability of the child's behavior

Our treatment effects suggest an informational story: parents in our study highly value monitoring devices that could help them control their child's behavior. To address the robustness of our findings and test our predictions in more depth, we study how parents' perception of their ability to observe their child's behavior affect our treatment effects.

- *Text message* treatment and perception of observability

For parents that believe they 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.[53] The results suggest that for parents who report better abilities to monitor their child, the *Text message* treatment has a weaker effect.

- *Non-classroom* treatment and perception of observability

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.

[53] 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.

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, but now under this new CCT program that only requires attendance when the child is not in class.

5.2.6 Experimenter demand effects and social desirability

Although the respondents are choosing payment schemes under real stakes, they might be tempted to choose what they feel to be socially or experimenter-approved decisions. We can shed light on these ubiquitous experimental concerns in a few ways. First, we had seventeen surveyors with varying levels of experience and knowledge. Some helped with only a few parents while some did as many as thirty-eight. Some came to our training session while others (unfortunately) had minimal training. While we never told any surveyor the hypotheses of the experiment, one could reasonably assume that with more experience and training, they could more readily infer the goal of the study. If we consider experience or training as measures of surveyor knowledge, and hence the "double-blindedness" of an interview, we can address these issues. If we linearly interact the treatment dummies with an indicator of whether the surveyor received training and include these in the main regression specifications used before, as presented in Table 10, we observe that training never significantly affects the treatment effects, and the direction of the interaction is generally towards attenuating our main findings. We also compute a measure of surveyor experience, by calculating, for each experiment session, the number of interviews that each surveyor had conducted before that session. In Table 11, we report the interaction analysis of the treatment effects with our measure of surveyor experience. We observe that experience, also, if anything, seems to attenuate the treatment effects.

Although it is not immediately clear in which direction social desirability should be pushing the willingness to pay in each treatment—Should "good parents" declare they have no child control problems or rather be willing to pay for an external control device?—it is always a legitimate concern in experiments, especially involving face-to-face interactions. It is difficult to directly measure social desirability, but there seems to be little evidence indicating that it is driving the results. First, the parents are, on average, 40 years-old, and the surveyors are all college-aged. Second, the stakes are real and potentially large. Third, most subjects report stigmatized behaviors and beliefs, such as the fact that they would be willing to lie to their child (or hide something from the child), an implicit result from the *Don't tell* treatment. Also, the interaction terms in some of the regressions presented before 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 perceptions of observability of the child's action by the parent.

6 Concluding remarks

Using a real-stakes experiment, we study the 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 being in school and do so more than the children themselves do.

Second, we observe that informational asymmetries can play a crucial role: parents reveal an 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 show that in the slums that we study, parents seem to substantially value schooling even if the child is not in the classroom, and thus learning. Skills acquisition, traditionally viewed as the quintessential role of schooling, does not seem to comprise wholly parents' demand for 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 underinvestment in education and overexposure to violence, both being consistent with unfortunate current trends in the geographical area we study, with the population 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 high and where there is no evident shortage of schools, 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.[54] 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.[55]

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 this approach, policies designed to promote school attendance might be more effective if they target the child or the household information structure

[54] 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.

[55] Our regression results suggest that poorer parents are willing less to pay for the conditionality. This is consistent with the results in Bursztyn (2010) 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 many of them 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 could be 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 could increase the safety provided to children. After-school programs, even without skills acquisition components tied to them, could have a significant impact on welfare. Local governments in Brazil (including the *Distrito Federal* government) have recently been implementing longer shifts with extra-curricular activities in public schools that are not space-constrained. Finally, the relatively low importance attributed to the classroom content of schooling might also suggest that there is room for a great deal of improvement in the educational quality of Brazilian public schools, or perhaps how the quality is viewed by the populace.

In conclusion, we note the limitations of this study. First, much of the discussion herein centered on the parents' beliefs and 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.

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Figures and tables

Figure 1: **Cumulative probability for the required unconditional transfer (censored threshold) in the Baseline treatment group**

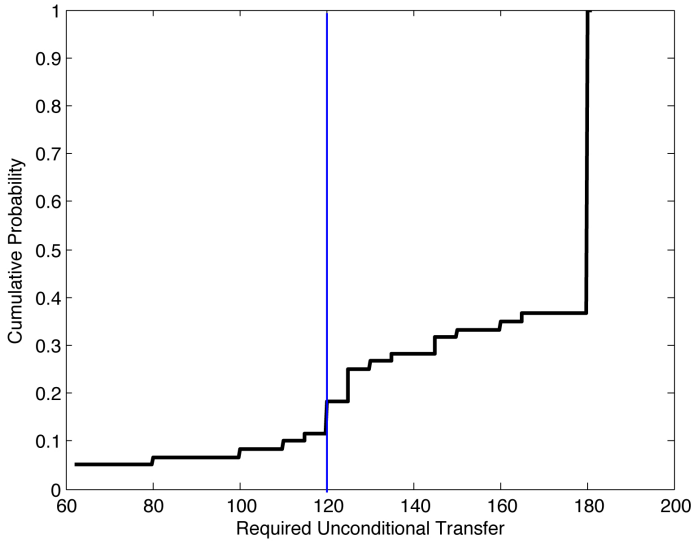
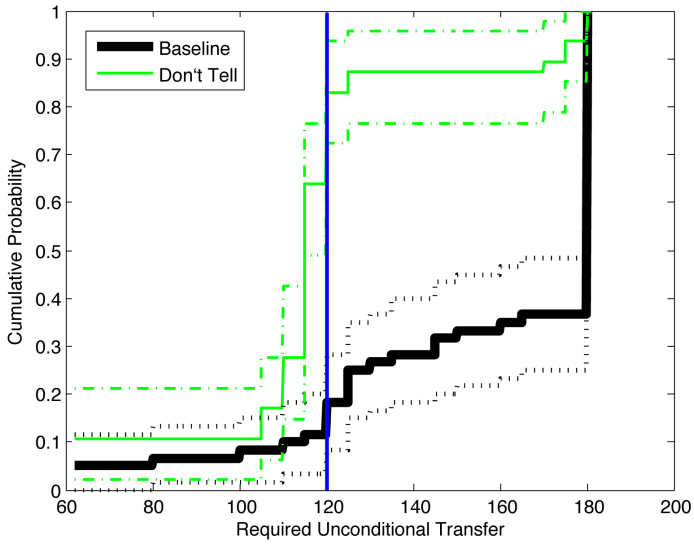
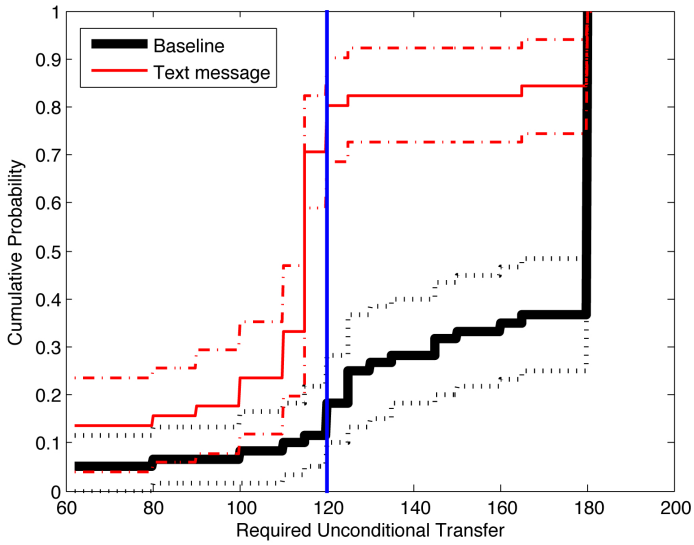


Figure 2: **Cumulative probability for the required unconditional transfer (censored threshold): Baseline and Don't Tell treatment groups** (with 95% bootstrap confidence intervals - 1,000 bootstrap samples)



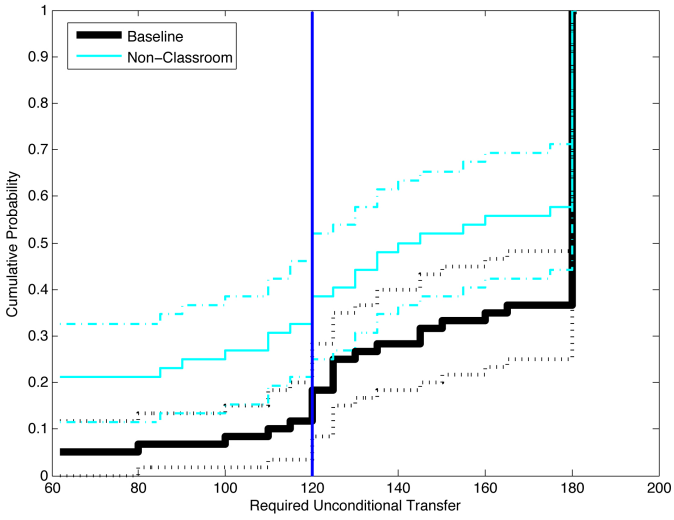
Notes: We re-sampled with replacement from the empirical distribution 1,000 times. From these 1,000 bootstrap samples, we computed the confidence intervals for each point on the cumulative distribution.

Figure 3: **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.

Figure 4: **Cumulative probability for the required unconditional transfer (censored threshold): Baseline and Non-classroom treatment groups** (with 95% bootstrap CI - 1000 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**

Treatment group	Type of conditionality	Additional features	Does the child know the choice?	Obs.
Baseline	85% attendance of classes	-	Yes	61
Don't tell	85% attendance of classes	-	No	47
Text message	85% attendance of classes	Text message when child misses (for any choice)	Yes	52
Non-Classroom	85% attendance in no-class shift	-	Yes, but not autom. promotion	53

Table 2: **Means of observables across treatments**

	Treatment			
	Baseline (N=61)	Don't tell (N=47)	Text message (N=52)	Non-classroom (N=53)
Variable (mean)				
Age (parent)	40.58	41.23	38.47*	40.10
Age (child)	14.22	14.28	14.06	14.29
Male parent	0.03	0.04	0.00	0.08
Male child	0.44	0.38	0.42	0.48
Married	0.49	0.56	0.58	0.42
Single	0.25	0.17	0.17	0.26
Divorced	0.25	0.25	0.23	0.30
Log HH income	6.24	6.23	6.23	6.09*
Employed	0.47	0.43	0.53	0.56
Employed Spouse	0.32	0.34	0.47*	0.15**
Catholic	0.51	0.54	0.54	0.57
Protestant	0.39	0.38	0.40	0.40
No religion	0.05	0.02	0.04	0.02
Beta (parent) [a]	1.00	0.93	1.01	1.08
Beta (child)	1.14	0.93**	1.00	0.97**
Delta (parent) [b]	0.76	0.79	0.67**	0.69
Delta (child)	0.73	0.83	0.78*	0.79
Yrs. schooling (parent)	7.12	6.34	7.18	6.62
Yrs. schooling (child)	7.59	8.02*	7.84	7.54
# of children in HH	3.65	3.74	3.63	3.27
Black parent [c]	0.28	0.19	0.19	0.17
Mestizo parent	0.56	0.56	0.56	0.60
White parent	0.13	0.21	0.23	0.19
Black child	0.28	0.19	0.19	0.17
Mestizo Child	0.56	0.56	0.56	0.60
White Child	0.13	0.21	0.23	0.19

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 the Supplementary Appendix.

[c] Race is self-reported.

Table 3: **Beliefs about returns to schooling**

	Mean (in R$) (Std. deviation in parentheses)			Median (in R$)			
	Parent's belief	Child's belief	Diff.	Parent's belief	Child's belief	Diff.	
Beliefs about child monthly income if she drops out and gets a job	365 (150)	392 (243)	-27	460	450	10	
Beliefs about child monthly income increase with...							
One More Yr. of School	198 (200)	217 (509)	-19	150	100	50*	
Two More Yrs. of School	323 (272)	313 (612)	20	250	200	50**	
Secondary Degree	576 (388)	561 (774)	15	500	400	100***	
College Degree	2,066 (5,677)	2,197 (12,509)	-131	1,143	800	343***	
Yearly average of beliefs about rate of returns to schooling	22% (0.12)	19.2% (0.14)	2.8%**	20%	16%	4%***	
Perceived and Observed Wages							
	Parent's belief	Child's belief	Diff.	Parent's belief	Child's belief	Diff.	National Avg. (PNAD 2007)
High-school graduate wage	952.5 (424.2)	956.4 (956.9)	-3.9	865	765	100**	903.8
College graduate wage	2425.8 (5805.3)	2605.7 (12570.4)	-179.9	1500	1117.5	382.5***	1843.9

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 4: **How much do parents know?**

Respondent	Parent	Child	Diff.
Did the child miss any day of school this year? (% answering "yes")	75.60	85.58	-9.98***
How many days did the child miss this year?	4.8	5.16	-0.36
Did the child miss any day of school in the last two months? (% answering "yes")	50.96	56.04	-5.08
How many days did the child miss in the last two months?	1.36	1.97	-0.61 **
Did the child miss any day this year because the child was sick? (% answering "yes")	42.99	31.78	11.21 ***
... any day because the child did not want to go? (% answering "yes")	8.88	15.42	-6.54***

*** 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**

	Treatment			
	Baseline (N=61)	Don't tell (N=47)	Text message (N=52)	Non-classroom (N=53)
Outcome Variable				
% Demanding conditionality	88.5	37.5***	30.8***	67.9***
% Paying for conditionality	82	18.7***	21.1***	62.3***
Average threshold (extrapolated)	200.9	115.9***	117.9***	146.7***
Median threshold (extrapolated)	190.7	115.5***	117.4***	146.4***
Average threshold (censored)	157.3	117.2***	116.8***	135***
Median threshold (censored)	180	115***	115***	142.5**

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**

Dependent Variable	Dummy: parent prefers R$ 120 CCT to…		CT indifferent to R$ 120 CCT	
	R$ 120 CT Demands conditionality OLS	R$ 125 CT Pays for conditionality OLS	Threshold Tobit	Threshold OLS (censored)
Don't tell Treatment dummy	-0.441	-0.574	-56.108	-30.36
	[0.104]***	[0.065]***	[14.485]***	[9.978]**
Text message Treatment dummy	-0.531	-0.535	-54.858	-31.813
	[0.132]***	[0.094]***	[15.204]***	[10.108]**
Non-classroom Treatment dummy	-0.143	-0.138	-34.348	-16.337
	[0.098]	[0.072]*	[9.769]***	[5.069]***
Higher show-up fee dummy	-0.019	-0.208	13	3.438
	[0.124]	[0.149]	[30.051]	[16.739]
Age (parent)	-0.004	-0.01	-1.417	-0.729
	[0.006]	[0.004]**	[0.645]**	[0.405]
Age (child)	0.024	0.037	7.334	4.383
	[0.037]	[0.045]	[6.315]	[3.672]
Male parent dummy	0.019	-0.121	-27.277	-12.647
	[0.274]	[0.166]	[23.703]	[15.330]
Male child dummy	0.035	-0.017	-1.6	1.667
	[0.105]	[0.117]	[12.227]	[8.684]
Weekly discount factor (parent)	0.083	0.174	7.831	1.294
	[0.138]	[0.213]	[27.528]	[17.028]
Weekly discount factor (child)	-0.069	-0.287	-48.282	-22.784
	[0.165]	[0.187]	[34.129]	[19.427]
Log Household Income	0.04	0.124	15.324	8.711
	[0.072]	[0.082]	[4.461]***	[3.091]**
Observations	208	208	208	208
R-squared	0.49	0.52		0.46
Mean of dep. var. in Baseline Treatment	0.88	0.82	200.9	157.3

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, number of children in the household for whom parents receive CCT, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.

Table 7: **Quartile Regressions - Treatment Effects on "Censored Threshold"**

	Treatment effects on censored threshold evaluated at ...		
	First quartile	Median	Third quartile
Don't tell Treatment dummy	-23.258	-41.684	-40.993
	[11.302]**	[11.793]***	[13.481]***
Text message Treatment dummy	-23.969	-39.53	-41.708
	[13.553]*	[11.486]***	[11.392]***
Non-classroom Treatment dummy	-13.781	-7.491	-12.294
	[15.771]	[15.800]	[10.068]
Observations	208	208	208
Quartile level of censored threshold in Baseline group	125	180	180

Robust standard errors (clustered by school) in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%
Additional controls: 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, number of children in the household for whom parents receive CCT, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.

Table 8: ***Text message*** **Treatment Interactions**

Dependent Variable	Dummy: parent prefers R$ 120 CCT to...		CT indifferent to R$ 120 CCT	
	R$ 120 CT Demands conditionality OLS	R$ 125 CT Pays for conditionality OLS	Threshold Tobit	Threshold OLS (censored)
Text message treatment interactions				
How much do you know about what your child is doing? (standardized measure)				
Treatment effect	-0.534	-0.54	-54.611	-31.922
	[0.129]***	[0.096]***	[15.296]***	[10.111]**
How much do you know?	-0.025	-0.047	-3.837	-2.66
	[0.033]	[0.047]	[4.696]	[3.173]
Interaction term	0.045	0.088	11.58	6.856
	[0.057]	[0.047]*	[4.464]**	[2.763]**
Observations	208	208	208	208
R-squared	0.49	0.53		0.46
Mean of dep. var. in Baseline Treatment	0.88	0.82	200.9	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, number of children in the household for whom parents receive CCT, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.

Table 9: ***Non-classroom*** **Treatment Interactions**

Dependent Variable	Dummy: parent prefers R$ 120 CCT to...		CT indifferent to R$ 120 CCT	
	R$ 120 CT Demands conditionality OLS	R$ 125 CT Pays for conditionality OLS	Threshold Tobit	Threshold OLS (censored)
Non-classroom treatment interactions				
How much do you know about what your child is doing? (standardized measure)				
Treatment effect	-0.146	-0.144	-33.424	-16.469
	[0.099]	[0.078]*	[10.033]***	[5.245]**
How much do you know?	0.019	0.009	5.974	2.653
	[0.031]	[0.032]	[4.233]	[2.766]
Interaction term	-0.125	-0.121	-26.645	-13.291
	[0.068]*	[0.054]**	[10.330]**	[6.342]*
Observations	208	208	208	208
R-squared	0.50	0.53		0.47
Mean of dep. var. in Baseline Treatment	0.88	0.82	200.9	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 Text message 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, number of children in the household for whom parents receive CCT, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.

Table 10: **Treatment Effects and Surveyors' Training**

Dependent Variable	Dummy: parent prefers R$ 120 CCT to…		CT indifferent to R$ 120 CCT	
	R$ 120 CT Demands conditionality OLS	R$ 125 CT Pays for conditionality OLS	Threshold Tobit	Threshold OLS (censored)
Don't tell Treatment dummy	-0.406	-0.73	-82.734	-39.696
	[0.240]	[0.151]***	[32.646]**	[20.191]*
Text message Treatment dummy	-0.621	-0.713	-83.246	-44.742
	[0.145]***	[0.188]***	[23.045]***	[13.329]***
Non-classroom Treatment dummy	-0.105	-0.102	-38.703	-14.728
	[0.125]	[0.108]	[23.970]	[11.266]
Trained surveyor dummy	0.255	-0.035	35.022	12.944
	[0.235]	[0.253]	[47.839]	[30.482]
Don't tell*Trained surveyor dummy	-0.045	0.21	36.255	12.665
	[0.240]	[0.166]	[31.955]	[18.349]
Text message*Trained surveyor dummy	0.12	0.262	41.668	18.803
	[0.165]	[0.224]	[25.902]	[16.118]
Non-classroom*Trained surveyor dummy	-0.059	-0.057	5.565	-2.651
	[0.134]	[0.101]	[25.600]	[12.595]
Observations	208	208	208	208
R-squared	0.49	0.53		0.46
Mean of dep. var. in Baseline Treatment	0.88	0.82	200.9	157.3

Robust standard errors (clustered by school) in brackets

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

Additional controls: 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, number of children in the household for whom parents receive CCT, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.

Table 11: **Treatment Effects and Surveyors' Experience**

(Experience is measured at each experimental session as the total number of interviews each surveyor has conducted prior to that session)

Dependent Variable	Dummy: parent prefers R$ 120 CCT to…		CT indifferent to R$ 120 CCT	
	R$ 120 CT Demands conditionality OLS	R$ 125 CT Pays for conditionality OLS	Threshold Tobit	Threshold OLS (censored)
Don't tell Treatment dummy	-0.391	-0.626	-67.903	-30.472
	[0.115]***	[0.119]***	[16.375]***	[11.393]**
Text message Treatment dummy	-0.783	-0.703	-87.407	-45.151
	[0.175]***	[0.136]***	[28.756]***	[17.422]**
Non-classroom Treatment dummy	-0.249	-0.179	-58.483	-23.395
	[0.134]*	[0.107]	[17.424]***	[8.785]**
Surveyor experience	-0.009	-0.012	-2.274	-0.944
	[0.017]	[0.018]	[2.452]	[1.881]
Don't tell*Surveyor experience	-0.008	0.005	1.021	-0.066
	[0.012]	[0.014]	[1.034]	[0.796]
Text message*Surveyor experience	0.022	0.015	2.885	1.209
	[0.014]	[0.012]	[1.704]*	[1.189]
Non-classroom*Surveyor experience	0.013	0.006	2.578	0.87
	[0.008]	[0.010]	[1.301]**	[0.847]
Observations	208	208	208	208
R-squared	0.52	0.53		0.47
Mean of dep. var. in Baseline Treatment	0.88	0.82	200.9	157.3

Robust standard errors (clustered by school) in brackets

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

Additional controls: 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, number of children in the household for whom parents receive CCT, race dummies (parent and child), years of schooling (parent and child), school and surveyor dummies.