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

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This paper experimentally analyzes the schooling decisions of poor households in urban Brazil. We elicit parents' choices between monthly government transfers conditional on their adolescent child attending school and guaranteed, unconditional transfers of varying sizes. In the baseline treatment, an overwhelming majority of parents prefer conditional transfers to larger unconditional transfers. However, few parents prefer conditional payments if they are offered text message notifications whenever their child misses school. These findings suggest important intergenerational conflicts in these schooling decisions, a lack of parental control and observability of school attendance, and an additional rationale for conditional cash transfer programs—the monitoring they provide.

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I. 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 his or 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 the parents' ability to monitor their children's school attendance behavior can be a key factor in household decisions.

To study these issues, we look directly inside the household "black box." We analyze the preferences of adolescents and parents in poor, urban Brazil and the decision-making process that leads to real schooling choices. We model the schooling decision as a moral hazard problem between an adult and the adult's child, in which the child is the agent of the decision and the adult cannot perfectly observe school attendance behavior. When the adult wants the child to attend school and the child's preferences are not aligned with the adult's, imperfect observability reduces the adult's ability to provide incentive for school attendance via payments to the child. In this case, the adult may be willing to pay for devices that improve monitoring of the child's actions, thus attenuating moral hazard. The child may also be willing to pay for monitoring devices, so that, in equilibrium, the child will attend school and the adult will reward the child for school attendance.

We use a novel experimental approach to elicit preferences and to understand the informational structure within households with adolescent children in slums (*favelas*) surrounding the city of Brasilia, Brazil. To provide incentive for the questions, we use the setup of the existing local conditional cash transfer (CCT) program, *Bolsa-Escola Vida Melhor* (school stipend, better life). In the program, at the time

[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, e.g., Psacharopoulos (1985, 1994) and Duflo (2001) 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, e.g., the literature on child labor decisions, e.g., Baland and Robinson [2000]).

of the intervention, families with monthly per capita household incomes below half of the Brazilian level of minimum wage (approximately US$120 per month) received large monthly transfers,[3] at least R$120 (approximately US$60 at the time), conditional on their children attending school 85 percent of the days of that month.[4]

Working with the Secretariat of Education of the Distrito Federal state,[5] we are able to estimate parents' willingness to pay to keep (or drop) the conditionality—which we later show to correspond to a willingness to pay for information—by offering the opportunity to switch to unconditional monthly transfers of varying sizes delivered in the same manner as their current conditional payments. Across parents, we randomly change the conditions under which the choices are made. Five percent of the subjects had one of their choices randomly chosen to be their actual payment 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 and the relevance of moral hazard problems inside the household. Our goal is not to measure the impact of implementing different payment schemes chosen by the parents but rather to understand, from parents' choices, the decision-making process leading to schooling choices in those households. Our outcome variables of interest are therefore directly derived from parents' choices during the experiment across treatments.

In the *baseline* treatment, parents are asked to choose between their current CCT program and unconditional payments, or "cash transfers" (CTs), of varying relative sizes. They are told that their child would be informed of any program change. The vast majority of these parents (over 80 percent) prefer to keep the conditionality to tie the large cash transfer to their child's attendance, even if the unconditional transfers pay strictly more. Furthermore, on average, they are willing to forgo the equivalent of more than 6 percent of their monthly household income to keep the conditionality.

The *text message* treatment is identical to the baseline treatment except that before eliciting parents' choices between CCTs and CTs, parents are offered free cell phone text messages every time their child misses

[3] 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), which had previously been used by the local government.

[4] Although some CCT programs are said to suffer from lack of enforcement, *Bolsa-Escola Vida Melhor* has a strong concern for enforcing the program's conditionality. Such enforcement includes random visits made by government officers to schools to check on attendance and compliance to the rules.

[5] Contract number: Termo de Cooperaçao N. 05/2009, Distrito Federal, Secretaria de Estado de Educaçao. For a digital copy of the contract, see the online supplemental appendix to this paper.

school, regardless of the parents' choices between conditional and unconditional cash transfers.[6] Armed with this monitoring technology, very few parents prefer to keep the conditionality; the proportion willing to pay to keep the conditionality drops from over 80 percent to around 20 percent compared to the baseline treatment. When parents are given the ability to perfectly monitor their children, the vast majority find the conditionality to be unnecessary and of little value. Together the two treatments indicate that parents are willing to pay for information to facilitate giving incentives to their children to go to school.

Albeit without providing incentives, we also elicit children's choices between conditional and unconditional transfers under the design of the baseline treatment, and we find that the majority of the children are willing to pay to keep transfers conditional. Although this may seem counterintuitive, our model provides conditions for which such a preference is rational for the children. In the model, the children may prefer a CCT when a CCT induces schooling because it also induces rewards for the children, rewards that will exceed the children's costs of going to school due to informational rents.

Our findings suggest an additional rationale for CCT programs. These programs are a widespread phenomenon in developing countries: in 2010, over 30 countries employed some version of a CCT program.[7] The CCT programs are usually purported to work by (i) lifting credit constraints and (ii) raising the value of the rewarded behavior, in our case, schooling.[8] Our results illuminate an alternative channel through which CCTs may be operating—by providing information to the parents on their children's school attendance behavior and by making this in-

[6] In this treatment, the child is also aware of the parents' choice.

[7] For the impact of conditional cash transfer programs on school attendance, see Bourguignon, Ferreira, and Leite (2003), Glewwe and Olinto (2004), Schultz (2004), Parker, Rubalcava, and Teruel (2008), Bobonis and Finan (2009), Angelucci et al. (2010), and specifically for Brazil, de Janvry et al. (2007) and Glewwe and Kassouf (2008). For the effect on child labor, see Cardoso and Souza (2004). For the impact of providing incentives to the child directly, see Kremer, Miguel, and Thornton (2004), Jackson (2008), and Angrist and Lavy (2009). For indirect effects on noneligibles' consumption, see Angelucci and De Giorgi (2009). For the effect on sexual behavior of recipient children, see Baird et al. (2009). For a study on the effects of making payments conditional, see Baird, McIntosh, and Ozler (2011). For an analysis of the effects of variations on the design of a conditional cash transfer program and intrahousehold externalities of the program, see Barrera-Osorio et al. (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 et al. (2006).

[8] 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 vs. conditional cash transfer programs. The literature also values conditional cash transfers to the extent that they achieve more and better-targeted redistribution when compared to an exclusive public goods provision policy (see Gahvari and Mattos [2007], following arguments made by Zeckhauser [1971] and Besley and Coate [1992]).

formation provision visible to the children. These information flows facilitate contracting between the parent and child on school attendance. Our results suggest that cash transfers alone, without sufficient monitoring of children's behavior, may not be enough to induce school attendance. In our study, however, monitoring was never offered without a cash transfer, so we cannot say if monitoring alone would be enough to induce attendance.

The results could have other important policy implications, particularly in developing countries such as Brazil, where educational attainment and school attendance are low despite high levels of returns to schooling.[9] In 2008, 10 percent of the Brazilian population were illiterate, and the average number of years of schooling was only 7.1 (PNAD 2008). This could lead an observer to believe, based on a standard model of schooling, that many poor parents do not value education or cannot afford to send their children to attend school because they need the children to provide resources for the household.[10] Our results suggest that there might be a further reason behind these unfortunate facts in the developing world: agency issues in the household.[11] Our findings suggest that providing parents with improved information and monitoring on their children's actions could be an effective policy option for increasing school attendance in areas of urban poverty.[12]

This paper relates to several recent empirical studies on household decision making in the context of developing countries.[13] In particular, Berry (2012) provides evidence of a differential impact of incentives for test scores and attendance depending on whether the recipient is the

[9] 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).

[10] See, e.g., Basu and Van (1998) and Basu and Tzannatos (2006).

[11] The 2006 Brazilian National Household Survey (PNAD) asked 15–17 year-old adolescents about their main reason for not attending school: 39.1 percent reported their own pure lack of interest in going to school, 20.7 percent mentioned working or looking for a job, 3.7 percent reported having to help at home, and only 1.5 percent reported that they were prevented from attending by their parents.

[12] 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 eighth-grade students are significantly lower than the actual returns. Moreover, when provided with information on actual returns to schooling, the least-poor students in Jensen's analysis were significantly less likely to drop out of school in subsequent years. Attanasio and Kaufmann (2009) use 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. Dinkelman and Martínez (2011) find significant decreases in absenteeism from providing Chilean eighth graders and a subset of their parents with information about financial aid for higher education.

[13] Several address decision-making processes across spouses and genders, such as Duflo (2003), Duflo and Udry (2004), and Rangel (2006). Bobonis (2009) tests whether the allocation of resources in households is efficient, using experimental variation in Mexico. Some papers have also addressed empirically or experimentally issues on intergenerational decision making, such as Li et al. (2010).

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 theorem states 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. However, the result does not necessarily hold under assumptions of moral hazard (Bergstrom 1989; Weinberg 2001; Gatti 2005).[14] 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 suggests that the "Rotten Kid Theorem" would not hold for the schooling decision in the environment we study. However, our results also indicate that conditional cash transfers such as those 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.

The remainder of the paper is organized as follows. In Section II, we present background information on public education and conditional cash transfers in Brazil. In Section III, we introduce our theoretical framework. We present our experimental design in Section IV. In Section V, we present the results from our experimental treatments. Section VI concludes.

II. Public Education and Conditional Cash Transfers in Brazil

Education is compulsory in Brazil for children ages 6–15, but the law is loosely enforced. In fact, according to the 2006 Brazilian National Household Survey (Pesquisa Nacional por Amostra de Domicilios; PNAD), over 9 percent of 14-year-old children from the bottom quartile of the distribution of household per capita income reported not being enrolled at

[14] 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, Shleifer, and Summers (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. Cherchye, De Rock, and Vermeulen (2009) test general collective consumption models and reject the standard unitary model. Mazzocco (2007) studies household intertemporal behavior and commitment, modeling households as groups of agents making joint decisions.

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 reenroll in the following year as required by law (de Janvry, Finan, and Sadoulet 2007).

The problem of school attendance in Brazil is particularly acute for poor children ages 13–15. Although working is only legal at the age of 16, over 15 percent of 15-year-old children from the bottom quartile households in the income distribution were not enrolled in school in 2006, and over 22 percent reported having a job during the week they were interviewed for the 2006 PNAD. If one looks at children of high school age instead, the situation is even more concerning: in 2006, 42.6 percent of 18-year-old individuals from the bottom quartile of the household per capita income distribution had dropped out of school before completing a high school education.[15]

Since 1995, both local and federal governments in Brazil have implemented different conditional cash transfer programs aimed at reducing income inequality and increasing school attendance. The idea of CCT 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 (e.g., 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. 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 ages 6–15. The federal program later increased the cutoff age for children enrolled in the program up to its current level, age 17. At the time of our study, the Distrito Federal state CCT program, renamed *Bolsa-Escola Vida Melhor* in 2009, was still administered separately by the local government. Our experiments only involved recipients of the Distrito Federal state CCT program.

The available evidence suggests that the federal program has indeed stimulated schooling among its beneficiaries. De Janvry, Finan, and Sadoulet (2010) estimate that in 261 municipalities in the Northeast of Brazil, if the beneficiary children were not in the program, their dropout rate would have been 12 percent instead of the 4 percent it was under the CCT program, a 67 percent decline. The program's impact on school

[15] Although the comparison is imperfect, we can put these dropout rates in context by relating them to high school dropout rates in poor, urban areas in the United States. The high school dropout rates for the class of 2011 in Detroit and Indianapolis, two of the public school districts with the lowest graduation rates among the 50 largest US cities (Swanson 2009), were both around 20 percent (sources: State of Michigan, Center for Educational Performance and Information, and Indiana Department of Education.)

attendance rates could very well have been even higher if enrollment were not compulsory (and therefore quite high on paper).

At the time of our study, the eligibility criterion for the *Bolsa-Escola Vida Melhor* program was a monthly per capita household income below half of the Brazilian level of minimum wage (approximately US$120 per month). Under this CCT program, the mother of a beneficiary household receives R$120 per month if one child between the ages of 6 and 15 attends a minimum of 85 percent of classes that month.[16] If the child misses more than 15 percent of the classes in any month (unjustified absences), payments are suspended for the next month onward.[17] Absences are reported by teachers to the school principals and from principals to the local government. Although anecdotal evidence suggests that in the national program the conditionality is not strongly enforced, the local program in the Distrito Federal state is known for having a strong concern for enforcing the conditionality. The local government does random visits to schools to enforce the compliance to the rules. If the family has more than one child within this age range, it receives R$120 per month for the first child, R$30 for the second, and R$30 for the third.[18] The maximum payment per month is R$180 per family.

III. Theoretical Framework

In this section, we develop a simple model of the schooling decision in the household.[19] The model will deliver an alternative rationale for CCT programs—providing information to parents on the attendance behavior of their children.

Suppose that there are N pairs, $n = 1, 2, 3, \ldots, N$. Each pair has a risk-neutral child c and a risk-neutral adult a. The pairs play a two-period game. In period 1, the child chooses $e_n \in \{0, 1\}$. If the child chooses $e_n = 1$, the child goes to school and incurs a cost, c_n. In period 2, the adult and the child receive payoffs (V_n^a, V_n^c), with probability p if the child chose $e_n = 1$, and both receive zero otherwise (the payoffs above are expressed in present value).[20] Hence, adult n's utility is

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

[17] The only requirement that the child has to meet for the payment to be made to the family is the school-attendance requirement.

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

[19] We are particularly grateful to the editor and to Florian Ederer for suggestions on the theoretical framework.

[20] The payoffs are realized with probability p capturing the uncertain nature of the returns to schooling.

$$U_n^a = \begin{cases} pV_n^a & \text{if } e_n = 1, \\ 0 & \text{if } e_n = 0. \end{cases}$$

Child n's utility is

$$U_n^c = \begin{cases} pV_n^c - c_n & \text{if } e_n = 1, \\ 0 & \text{if } e_n = 0. \end{cases}$$

If $c_n \leq pV_n^c$, then the child will find it privately optimal to attend school.

A. *Perfect Information*

A central planner with perfect information instructs child n to choose $e_n = 1$ if and only if

$$pV_n^a \geq c_n - pV_n^c, \tag{1}$$

and $e_n = 0$ otherwise.

Alternatively, under this condition, an adult who is able to commit to an ex ante optimal contract could transfer $c_n - pV_n^c$ to the child and provide incentive for school attendance as an equilibrium outcome. Henceforth, for each pair n, we focus on the case of an adult using transfers to provide incentive for school attendance to the child. We also restrict ourselves to the more interesting case where $c_n > pV_n^c$ (as discussed before, when $c_n \leq pV_n^c$, the child will find it privately optimal to go to school without transfers).

B. *Imperfect Information*

We now assume that the child's action is not observable by the adult. Therefore, the adult and the child cannot contract on e_n.[21] In particular, we assume that in each pair n, the adult receives a signal, s_n, that $e_n = 1$ and that the adult can contract based on the signal. The signal technology is as follows: $\Pr(s_n = 1|e_n = 1) = \Pr(s_n = 0|e_n = 0) = \pi$, where $\pi \in (1/2, 1]$. Hence, π is the quality of the monitoring technology as it measures the precision of the signal. Note that we assume that the same signal technology is available to all pairs.

1. The Child

To implement school attendance, the adult needs to make a payment to the child, w_n, when the parent observes $s_n = 1$. We assume limited liability on behalf of the child, $w_n \geq 0$. The child will attend school if and only if

[21] This is equivalent to a standard moral hazard model assumption that effort is observable but not verifiable and thus cannot be contracted upon.

$$w_n \geq \frac{c_n - pV_n^c}{2\pi - 1} \equiv \overline{w_n}. \tag{2}$$

Equation (2) is child n's incentive-compatibility (IC) constraint. We define $\overline{w_n}$ as the minimum size of the payment that will induce school attendance.

2. The Adult

Adult n's problem is to maximize U_n^a subject to the IC constraint and limited liability, $w_n \geq 0$. The adult in pair n will either choose $w_n = 0$ and have the child not go to school or choose $w_n = \overline{w_n}$ and have the child go to school. The adult will choose the latter if

$$pV_n^a \geq \frac{\pi}{2\pi - 1}(c_n - pV_n^c). \tag{3}$$

The first factor on the right-hand side of the above equation, $\pi/(2\pi - 1)$, is a measure of the inefficiency caused by imperfect information. It is easy to show that as π approaches one, the above condition becomes equivalent to the first-best condition (eq. [1]). On the other hand, as π decreases, the requirements on V_n^a for the parent to choose to provide incentive for school attendance become stronger. From equation (3), we can establish the following proposition.

PROPOSITION 1. If $c_n > pV_n^c$, for every pair n, there exists a threshold $\pi_n^* \equiv pV_n^a/(2pV_n^a + pV_n^c - c_n)$ such that, in equilibrium, $e_n = 1$ if $\pi \geq \pi_n^*$ and $e_n = 0$ if $\pi < \pi_n^*$.

If the monitoring technology is precise enough, in equilibrium, the parent will induce school attendance with a payment. If the signal is too imprecise, in equilibrium, the parent will not make a payment, and the child will not go to school.

3. Equilibrium Payoffs

For each pair n, let us define U_{n,e_n}^{a*} as the adult's equilibrium payoff and U_{n,e_n}^{c*} as the child's equilibrium payoff. It can be shown that $U_{n,1}^{a*} \geq 0$ (and strictly positive if $\pi > \pi_n^*$), $U_{n,0}^{a*} = 0$, $U_{n,1}^{c*} \geq 0$ (and strictly positive if $\pi < 1$), and $U_{n,0}^{c*} = 0$. Furthermore, we have $\partial U_{n,1}^{a*}/\partial\pi > 0$, $\partial U_{n,0}^{a*}/\partial\pi = 0$, $\partial U_{n,1}^{c*}/\partial\pi < 0$, and $\partial U_{n,0}^{c*}/\partial\pi = 0$.

The intuition behind the derivative signs is straightforward: for every adult-child pair n, when the child attends school (hence, $\pi \geq \pi_n^*$), a higher π reduces the size of the payment the adult has to make to meet the child's IC constraint, so the adult is better off. At the same time, a higher π reduces the informational rents of the child, thus making the child worse off. Figure 1 plots the equilibrium payoffs of

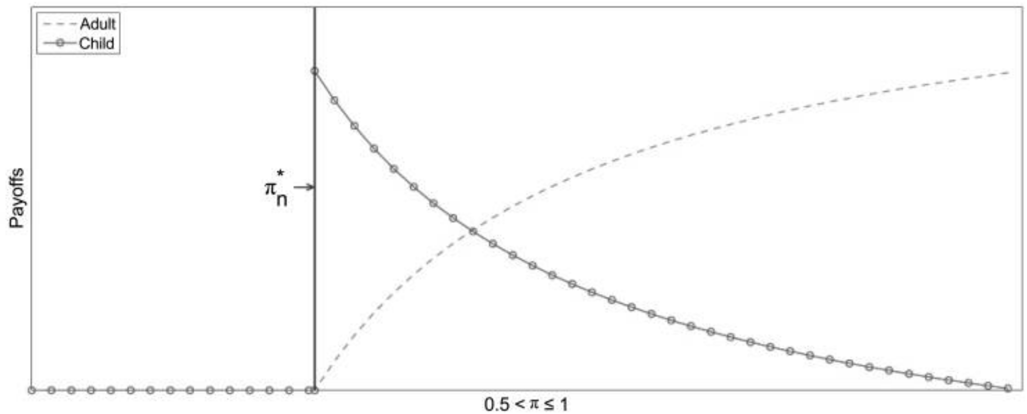

FIG. 1.—Adult's and child's equilibrium payoffs under optimal strategy implementation

an adult and a child of pair n as a function of π, under optimal strategy implementation.

C. *The Experiment*

Suppose an adult in pair n is offered choices between two policy instruments: conditional cash transfers (CCT) and (unconditional) cash transfers (CT), both of varying sizes. Conditional cash transfers are paid to the adult only if the child attends school; cash transfers are paid to the adult unconditionally, regardless of e_n.

DEFINITION 1. A conditional cash transfer $\mathrm{CCT}(T_{\mathrm{CCT}})$ is a payment scheme that transfers T_{CCT} units of income to the adult only if the child attends school at least 85 percent of the days that month.

Under the conditional cash transfer scheme, the probability of an adult receiving the signal of the child's true action is π_{CCT}. Since the parent has the same monitoring technology as before (π) plus the monitoring added by the CCT, we assume $\pi \leq \pi_{\mathrm{CCT}} < 1$. These inequalities capture the monitoring provided by real-world CCT programs; they provide good, but not quite perfect (not every day), signals of children's attendance.

DEFINITION 2. A cash transfer $\mathrm{CT}(T_{\mathrm{CT}})$ is a payment scheme that transfers T_{CT} units of income to the adult for any value of e_n.

Therefore, under this scheme, the parent's level of observability is unchanged: $\pi_{\mathrm{CT}} = \pi$.

For simplicity, we assume that, for every pair n, the adult privately consumes the entire transfer, regardless of the transfer scheme. Further, we assume that the monitoring technology provided by a CCT scheme is greater than the threshold for monitoring required under a CCT for the parent to induce school attendance.[22] That is, we assume that, for every pair, the child will attend school in equilibrium under a CCT program with a transfer of any positive size.

In our experiment, we are mainly interested in deriving the size of an unconditional cash transfer, $\hat{T}^a_{\mathrm{CT},n}(T_{\mathrm{CCT}})$, that would make the adult in pair n indifferent to a conditional cash transfer of size $T_{\mathrm{CCT}} > 0$. Under the conditions listed in the previous paragraph, we can establish the following two propositions and the corollary for every pair n.

PROPOSITION 2. $\hat{T}^a_{\mathrm{CT},n}(T_{\mathrm{CCT}}) > T_{\mathrm{CCT}}$ for all $T_{\mathrm{CCT}} > 0$ if and only if $\pi_{\mathrm{CCT}} > \pi$ and $\pi_{\mathrm{CCT}} > \pi^*_n$.

Proof. See the online supplemental appendix.

Proposition 2 establishes two jointly necessary and sufficient conditions

[22] Adapting notation from before, we define, for every pair n and for every $\mathrm{CCT}(T_{\mathrm{CCT}})$, $\pi^*_{n,\mathrm{CCT}}(T_{\mathrm{CCT}}) \equiv (pV^a_n + T_{\mathrm{CCT}})/(2pV^a_n + 2T_{\mathrm{CCT}} + pV^c_n - c_n) < \pi^*_n$ for all $T_{\mathrm{CCT}} > 0$. We therefore assume that for all n, $\pi_{\mathrm{CCT}} \geq \pi^*_{n,\mathrm{CCT}}(T_{\mathrm{CCT}})$ for all $T_{\mathrm{CCT}} > 0$.

under which, for any pair, the adult prefers a CCT to a CT even though the CT pays strictly more. The first condition is that the CCT strictly improves the monitoring technology compared to the CT scheme. Second, the CCT improves the monitoring technology beyond π_n^*. Adult n will prefer a CCT to a CT with a larger transfer if and only if both of those conditions hold. In this case, we say that the adult is willing to pay for information.

COROLLARY 1. If $\pi_{\text{CCT}} > \pi_n^*$, then $\hat{T}^a_{\text{CT},n}(T_{\text{CCT}}) = T_{\text{CCT}}$ for all $T_{\text{CCT}} > 0$ if and only if $\pi_{\text{CCT}} = \pi$.

In a situation in which an adult is willing to pay for information, if one sets $\pi = \pi_{\text{CCT}}$, then that adult will be indifferent between a CCT and a CT of equal sizes.

As we will describe in the next section, in one of our experimental treatments, we will exogenously vary the monitoring technology provided to adults; we will greatly increase the information they have about their children's school attendance. In this experimental treatment, not only will we be increasing π greatly, we will be equating the monitoring technology across the CCT and CT schemes. In that treatment, we will be able to test the following prediction, which follows immediately from the preceding results.

TESTABLE PREDICTION. In a sample of adult-child pairs, suppose that there initially exists a fraction $\mu > 0$ of adults who prefer a CCT to a CT that pays strictly more. If we add a superior monitoring technology to both the CT and CCT environments such that π and π_{CCT} are now larger and equal, then μ will become zero.

PROPOSITION 3. The child strictly prefers a conditional cash transfer (CCT) to a cash transfer (CT) if and only if $\pi < \pi_n^*$ (hence, $e_n = 0$ in equilibrium) under the CT.

Proof. See the online supplemental appendix.

That the child may prefer a CCT to a CT that pays a larger transfer may seem counterintuitive since the child has a cost of going to school. Proposition 3 says that the child would prefer a CCT to a CT if and only if, in equilibrium, the child does not go to school in the CT scheme. The intuition is that the child prefers the CCT to the CT because, under a CCT, the adult will make payments to the child, and these payments will more than offset the child's cost of attending school because of the child's informational rents.

IV. The Experiment

A. *The Setup*

We conducted the experiment with 210 families.[23] All 210 were already enrolled and benefiting from the local CCT program (*Bolsa-Escola Vida Melhor*).[24] For each family, we interviewed one parent and one child between the ages of 13 and 15. Those families were all enrolled in the program, and hence at the time they were receiving R$120 per month conditional on school attendance of a child to at least 85 percent of classes each month.

We focused on children of ages 13–15 because these children may have already formed individual preferences, some bargaining power in the household, and an outside option to schooling. This is also the age range at which school attendance drops considerably. According to the official Brazilian household survey (PNAD), by age 16, dropout rates reach 26 percent for children in Brazil with a household monthly per capita income less than R$100 (the average level of household monthly per capita income among beneficiaries of the *Bolsa-Escola* program was about R$65 in 2009). Finally, these were also the oldest CCT-eligible students since payments would stop when the child turned 16.

Families invited to the experiment were randomly chosen among those enrolled in the *Bolsa-Escola* program. First, four districts were 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 11 schools in four of the existing 20 districts in the Distrito Federal state (for evidence of the representativeness of our sample, see the online supplemental 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 1-hour study at the child's school over the weekend. Families were offered either R$7 or R$10 to attend the study.[25]

[23] This experiment would fall under "framed field" according to the nomenclature presented by Harrison and List (2004) and List (2008). A sample size of around 210 observations came from a crude, but useful, power calculation. In our pilot experiment, 94 percent of families (32 out of 34) preferred to have the conditionality over no conditionality for equally-sized transfers. We were hoping for treatment effects of at least 20 percent in our main experiment (hence dropping demand for conditionality to 75 percent), power of 80 percent, and significance at 10 percent. This called for 54 subjects per cell (for a two-sample comparison of proportions).

[24] The agreement with the local government was made possible with the help of the local nongovernmental organization *Missão Criança*.

[25] See a sample invitation letter and its English version in the online supplemental appendix.

The average show-up rate in our study was 87 percent (see the online supplemental appendix for details on show-up rates).[26]

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. Next, the children would make their decisions after the parents had left 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. That is, each parent came in to the experiment with the standard, local CCT program. In the experiment, they could potentially change this program. There were 25 questions, each one a choice between a cash transfer conditional on a behavioral outcome of their child (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 were always the same. Each question varied the relative size of the conditional and the unconditional transfers. That is, subjects were offered a 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 that the family could not leave with a transfer that paid less than their current program. 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.

[26] The show-up fee was varied to grant the ability to potentially estimate bias in the selection of what type of adult-child pairs participated in our study. Our show-up rates were 85 percent under the R$7 fee, so we do not attempt to estimate selection bias in our sample.

[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.

Which Monthly Payment Would You 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

Second, 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 You 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 the time spent on this procedure in these time-intensive surveys, 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. In practice, this meant that if the subject chose the CCT (CT) in the first choice—R$120 versus R$120—then the next question would increase the CT (CCT) transfer by R$5 and continue to do so until they switched to a CT (CCT). Thus, we induced monotonicity in the decisions. In previous studies, decisions are frequently, but not always, monotonic.[30] If the parents would have otherwise submitted nonmonotonic preferences, the reported levels of willingness to pay for conditional transfers may be understated in our experiment (since most subjects have a willingness to pay greater than zero, our elicitation ticks up the price, and we are only observing the first switch). However, the results section relies on cross-treatment analyses rather than emphasizing levels. Hence, concern

[29] It is possible that this ordering may have an effect within one treatment, but assuming it does not interact with treatment effects, this will not affect the analysis across treatments.

[30] For example, using a standard subject pool, Holt and Laury (2002), who use a modified Becker-DeGroot-Marschak procedure (BDM), find that 5.5 percent and 6.6 percent of their subjects revealed nonmonotonic preferences in their high- and low-stakes risk preference elicitation treatments. Although it remains an open question, we are not aware of any evidence that nonmonotonic behavior in a BDM would be more pervasive in a developing nation with a less educated subject pool. Guiteras and Jack (2012) use a BDM to elicit the willingness to accept different levels of piece-rate pay for real labor in Malawi. In their protocol, at each offered piece rate level, the surveyor asked three times if the subject was sure that they in fact would or would not receive a contract and be expected to work at that level if the level was randomly drawn. The authors find zero instances of nonmonotonic behavior.

would only arise if nonmonotonicities interacted with the treatment effects. We have no reason to suspect it would, especially to drive treatment effects the size of which we observe.[31]

Earlier versions of this paper reported that subjects explicitly responded to all 25 questions. This was the original design and intention; however, our team of surveyors decided during training that inducing monotonicity seemed reasonable and would save time. Only after we queried our research assistants, in summer 2010, as to why we found 100 percent monotonicity were we made aware of the change. All subsequent drafts have reported the protocol as it was implemented rather than how it was designed. As discussed in the previous paragraph, we do not believe the change affected the data in a meaningful way.

Each treatment used these same 25 conditional versus unconditional transfer questions.[32] Parents were informed that 5 percent of participants would have one of their decisions implemented and that decision would be randomly chosen from the 25 questions.[33] Any change would last through the end of the current school year, for 4 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.[34] The experiment was conducted at computer terminals using a web-based survey.[35]

[31] Nonetheless, we can get an extreme upper bound on how nonmonotonicities may affect the data. The most pessimistic approach would be to assume there is only nonmontonicity among parents in the treatment arms (not the baseline), thus reducing our observed treatment effects. Moreover, we consider the very extreme case in which the individuals with the lowest willingness to pay to keep the conditionality on the transfers (five parents in the text message treatment, or 14 percent of parents in that group) and seven in the don't tell treatment (11 percent) are the subjects who would, given the opportunity, exhibit nonmonotonic preferences, and we assume that their actual willingness to pay is the maximum price offered in the experiment. Our main treatment effects from this extreme scenario decrease slightly but not dramatically, and their coefficients are still statistically significant. This analysis can be found in the online supplemental appendix.

[32] See the actual set of 25 questions and text used in each treatment in Portuguese and their translated versions in English in the online supplemental appendix.

[33] Hence, this is a version of the BDM elicitation procedure that provides respondents with an incentive for truthful reporting of willingness to pay.

[34] We performed an experimental pilot with 35 families in March–April 2009, consisting largely of surveys and a version of the baseline treatment. A discussion on the pilot experiment design and results can be found in the online supplemental 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. The description and results from these treatments are reported in the working paper version of this paper.

[35] In all but one school, CEF 20 Ceilandia, the experiment was performed using Qualtrics's 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

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 1 hour.

B. Experimental Treatments

1. Baseline Treatment

The 60 parents in this cell made the sequence of choices just described.[36] They chose between a CCT and a CT with transfers of varying relative sizes. Other than the size of the transfer, the CCT, in this treatment, is the same as the one with which they came to the experiment: they get paid the transfer only if their child attends 85 percent of class days each month. The parent was informed that, at the very end of the session, the child would be made aware of the choices made by the parent. This treatment enables us to compute the fraction μ of parents in our sample who prefer a CCT to a CT that pays strictly more (and who are therefore willing to pay for information), according to the "testable prediction" established in the theory section.

2. Demand for Monitoring: Text Message Treatment

If monitoring is a problem in the household, then providing the parent with a level of monitoring π sufficient to enable the parent to induce school attendance already under the CT scheme should reduce demand for the CCT.

This treatment, randomly assigned to 51 families, is identical to the baseline except that the CCT-CT question for the parents was preceded by an offer to receive a free text message sent to their cell phone every day their child misses class. All parents in the text message treatment group who reported having a cell phone accepted the offer to start

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are excluded, so they are not. The results excluding that school are available in the online supplemental appendix.

[36] See the entire questionnaire used in the baseline treatment and its English version in the online supplemental appendix. Earlier versions of this paper reported one or two more subjects in three of the treatments, including the baseline. Two hundred and fourteen surveys were pulled up on computers, but four were never initiated; hence, here we will report 210 parents. Two hundred and eight families completed the survey (two left before the children started); hence some data analysis will necessarily report two fewer data points. As mentioned before, we relied on combinations of digits (which are randomly assigned to citizens) of the Brazilian equivalent to the Social Security number to determine the treatment status of parents. However, due to our sample sizes, different treatment groups ended up with different cell sizes. In particular, the baseline treatment ended with more observations than the other treatments. The assignment to treatment was based on the last two digits of the *Cadastro de Pessoa Fisica* of the parent: {even, even} for the baseline treatment, {odd, even} for the text message treatment, {even, odd} for the don't tell treatment, and {odd, odd} for the nonclassroom treatment.

receiving text messages. Only the two parents that did not have a cell phone did not accept the offer. All parents in the group are included in the main analysis. We reproduce the main regressions dropping the two parents who did not accept the text message offer. The results hold and are in the supplemental appendix. Parents were greeted with a screen offering the free service and asking for their cell phone number if they would like to sign up.[37] The rest of the experiment proceeded identically to the baseline treatment.[38]

According to the testable prediction from the theory section, the fraction μ of parents in our sample who prefer a CCT to a CT that pays strictly more should be zero when parents have access to text message notifications, unless some parents think the CCT still provides better monitoring than the level monitoring offered in the text message system (e.g., if they think that the system might not work well or that they might not have a cell phone number in the future).

3. Additional Treatments

The *don't tell* treatment, when combined with the results of the baseline treatment, serves to provide evidence consistent with two main elements of our theoretical framework: (i) there is a divergence between the adult's and the child's preferences within one pair, and (ii) the child is the agent of the schooling decision.[39]

The treatment, assigned to 47 families, is identical to the baseline except that the CCT-CT question for the parents was preceded by a short disclaimer saying that we would not tell the child if the transfer program was changed and that the child would not be offered a CCT-CT decision.[40] Thus, the children would not have any reason to believe

[37] All parents in the text message treatment group who reported having a cell phone accepted the offer to start receiving text messages. Only the two parents that did not have a cell phone did not accept the offer. All parents in the group are included in the main analysis. We reproduce the main regressions dropping the two parents who did not accept the text message offer. The results hold and are in the online supplemental appendix.

[38] Hence, in the text message treatment group, parents were making the decision to start receiving free text message notifications at the very beginning of the experiment, before the CT-CCT decision, and not knowing anything about the CT-CCT decision that would follow.

[39] As a result, the parent is precisely seeking to control the child when she pays for information (or the conditionality). It may be the case that the parent pays to keep payments conditional to control herself, or perhaps her spouse, whomever she views as the agent of the decision, and the behavioral problem. Further, it may be that she views the information provided by the text messages as a sufficient form of control of her spouse. This treatment is designed to make it clearer that parent-child conflict is the key to school attendance for this population.

[40] In this treatment, the child would only answer questions about demographics, preference parameters, etc.

that the family would be leaving with anything other than the CCT program with which it came.

This treatment makes two important changes from the baseline treatment. First, the child does not see if a change has been made to the transfer scheme the family is in. As a result, if parents want the CCT as a device to induce the child to attend school, then even those who would be willing to pay for the conditionality as a monitoring device if offered the set of choices from the baseline treatment could now choose the larger of the two transfers and allow the child to believe that the transfers are still conditional on attendance.[41] Second, the child does not see what the parent chooses. It may be that parents in the baseline treatment are using their decisions to signal to the child that schooling is valuable. Since the child cannot observe the choice, the don't tell treatment precludes the possibility of sending an externally verified signal. In either case, an observed drop in the willingness to pay to keep the conditionality compared to the baseline treatment would indicate that the parent believes that the child is the agent of the decision in the moral hazard school attendance problem. We further discuss the implications of this "signaling interpretation" of the preference for conditional payments in Section V.B.5.

The second additional treatment, the *nonclassroom treatment*, was implemented to help shed light on whether parental demand for schooling in the environment we study is also driven by parental valuation of the nonclassroom content of school, such as keeping the child off the streets. Fifty-two families were randomly assigned to this treatment group. We describe the design and analyze the results of this treatment in the supplemental appendix.[42] We therefore exclude the observations from this treatment in the analysis that follows.

4. Child's Choices

All children who participated in the experiment, with the exception of those whose families were assigned to the don't tell treatment, had their CT-CCT choices elicited under the design of the baseline treatment. There were not incentives provided for the children's choices, however.

[41] There might be concern that the parent might not think that she is able to lie to the child or that the child might experiment missing classes beyond 15 percent at a given month and learn that the transfers were no longer conditional. These behaviors would attenuate any treatment effects we might find. Further, the second explanation does not seem likely given the data on attendance rates from the local Secretariat of Education for March and April 2009 for all students ages 14 and 15 enrolled in the CCT program. Only 0.7 percent missed more than 3 days of classes in March and 1.9 percent in April.

[42] We thank the editor for the suggestion to move the nonclassroom treatment analysis to the online supplemental appendix and to focus the analysis of the paper on the agency problem in the household.

In the treatments in which the child was offered to choose between CCT and CT payments, the choices were offered first to the parent, then the child, then jointly.[43]

C. Experimental Outcomes and Empirical Specification

We are interested in the parents' choices between different types of CTs and CCTs. We focus on two outcome variables.

1. The parent is *willing to pay*—a dummy variable that is equal to one if the parent prefers a R$120 CCT to a CT that pays strictly more, and zero otherwise.
2. The parent's *willingness to pay*—equal to the largest difference in transfer sizes, $T_{\text{CT}} - T_{\text{CCT}}$, where the parent chooses the CCT program.[44]

In the baseline treatment group, we will be interested in willing/willingness to pay for the conditionality. Results from the baseline treatment coupled with results from the text message treatment will allow us to discuss willing/willingness to pay for information.

To estimate the treatment effects on the first (dummy) outcome variable of interest described previously, we first make mean comparisons across treatments without controls. Although the assignment to treatments was random, we also estimate treatment effects controlling for observables. To that end, we run the following regression in our empirical analysis:

$$Y_i = \alpha + \nu X_i + \phi_1 I_{\text{text message},it} + \phi_2 I_{\text{dont tell},i} + e_i,$$

where Y is the dummy dependent variable, 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 text message treatment and the don't tell treatment. Therefore, the treatment dummies measure the effect of each treatment compared to the baseline.

In our complete specification, 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's spouse indicator, religion dummies,

[43] The analysis of the joint decisions can be found in the online supplemental appendix.

[44] Note that the willingness to pay could be up to R$5 greater. We code the willingness to pay the same across all treatments and focus on across-treatment differences. For robustness, we recode the willingness to pay differently for the baseline and the other treatments. First, we leave the willingness to pay unchanged in the baseline treatment group and increase it by R$5 in all other treatments; this creates a lower bound on our effects. Second, we increase the willingness to pay by R$5 in the baseline treatment group and leave it unchanged in all other treatments, thus creating an upper bound on our effects. The results are shown in the supplemental appendix. Our results are robust to recoding the willingness to pay variable.

schooling (parent and child), number of children in the household, dummy on whether the household has been receiving CCTs for more than one child, beta (a measure of time inconsistency discount factor; for the parent and her child), delta (weekly discount factor; parent and child),[45] race dummies (parent and child), dummy for higher show-up fee, and school and surveyor dummies.[46]

Since the CCT-CT choice elicitation in the experiment only offered a maximum difference of R$60 between the sizes of the transfers, our measure of the willingness to pay for the conditionality is censored.[47] To deal with this censoring problem, we assess the treatment effects on the second outcome variable—the willingness to pay—by examining directly across treatments the cumulative distribution of the cash transfer that makes the parent indifferent to a R$120 CCT.[48]

V. Treatment Results

A. *Summary Statistics and Motivating Evidence*

Table 1 presents summary statistics for observables across the three treatment groups of interest.[49] With very few exceptions, the means are not significantly different from those of the baseline group. This suggests that the randomization was successful.

To motivate our analysis of the parent-child conflict, table 2 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 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.[50] The medians, however, are highly

[45] For a discussion on the construction and measure of the time preference parameters, see the online supplemental appendix.

[46] 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.

[47] For 34 percent of the respondents, a R$120 CCT was preferred to a R$180 CT, the maximum. Additionally, for 12 percent of the respondents, a R$120 CT was preferred to a R$180 CCT.

[48] We also run quantile regressions using the willingness to pay as the dependent variable and including the set of controls described above. The results are reported in the supplemental appendix. Details relating to the construction of the willingness to pay variable are also found in the online supplemental appendix.

[49] The table including the nonclassroom treatment is presented in the online supplemental appendix.

[50] 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.

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.

Table 2 also reports the means and medians of both parents' and children's beliefs about the average monthly wage of someone with a high school or college degree, 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, on average, the parents surveyed are not misestimating the actual wage levels in Brazil.[51]

We also find evidence that parents are underinformed of their child's school attendance behavior. Anecdotally, 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. Further, only 7 percent of the children in the sample report that they commute to school in the company of their parents. Table 3 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."[52] Finally, in the unincentivized postexperiment survey, 93 percent of parents report rewarding their child for school attendance in some capacity, and 36 percent report using financial rewards.

B. Treatment Effects

In table 4, we present the treatment effects on whether or not the parent prefers a R$120 CCT to a CT that pays strictly more (a dummy variable). In column 1, we present the treatment effects without controlling for observables. In column 2, we include individual-level and household-level covariates and run an ordinary least squares (OLS) regression.[53] In column 3, we also include surveyor and school dummies, and in column 4, we

[51] 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 16, discount rates are fairly similar between parents and their children.

[52] Recall 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.

[53] There are two parents whose children did not complete the entire survey and who thus could not be included in the regression from col. 2. To use the same sample in cols. 1 and 2, we also drop these observations from the specification in col. 1. The results (available upon request) are unchanged if we keep them in the regression from col. 1.

TABLE 1

MEANS OF OBSERVABLES ACROSS TREATMENTS

VARIABLE	TREATMENT Baseline (N = 60)	Text Message (N = 47)	Don't Tell (N = 51)
Age (parent)	40.58	38.47*	41.23
	(7.33)	(5.65)	(10.29)
Age (child)	14.22	14.06	14.28
	(.81)	(.98)	(.74)
Male parent	.03	.00	.04
	(.18)	(.00)	(.20)
Male child	.44	.42	.38
	(.50)	(.50)	(.49)
Married	.50	.59	.57
	(.50)	(.50)	(.50)
Single	.25	.18	.17
	(.44)	(.39)	(.38)
Divorced	.25	.24	.26
	(.44)	(.43)	(.44)
Log household income	6.24	6.23	6.23
	(.49)	(.61)	(.56)
Employed	.47	.53	.43
	(.50)	(.50)	(.50)
Employed spouse	.32	.47*	.34
	(.47)	(.50)	(.48)
Catholic	.52	.55	.55
	(.50)	(.50)	(.50)
Protestant	.40	.41	.38
	(.49)	(.50)	(.49)
No religion	.05	.04	.02
	(.22)	(.20)	(.15)
Beta (parent)[a]	1.00	1.01	.93
	(.32)	(.32)	(.19)
Beta (child)	1.14	1.00	.93**
	(.53)	(.34)	(.20)
Delta (parent)[b]	.76	.67**	.79
	(.24)	(.22)	(.21)
Delta (child)	.73	.78	.83**
	(.25)	(.18)	(.16)
Higher show-up fee	.20	.22	.32
	(.40)	(.42)	(.47)
Years of schooling (parent)	7.12	7.18	6.34
	(3.22)	(3.01)	(3.42)
Years of schooling (child)	6.59	6.84	7.02*
	(1.13)	(1.09)	(1.24)
Number of children in household	3.65	3.63	3.74
	(1.86)	(1.68)	(1.69)
Receiving CCT for more than one child	.52	.71**	.54
	(.50)	(.46)	(.50)
Black parent[c]	.28	.20	.19
	(.45)	(.40)	(.40)
Mixed race parent	.57	.57	.57
	(.50)	(.50)	(.50)
White parent	.13	.24	.21

TABLE 1 (*Continued*)

Variable	Treatment: Baseline ($N = 60$)	Text Message ($N = 47$)	Don't Tell ($N = 51$)
	(.34)	(.43)	(.41)
Black child	.28	.20	.19
	(.45)	(.40)	(.40)
Mixed race child	.57	.57	.57
	(.50)	(.50)	(.50)
White child	.13	.24	.21
	(.34)	(.43)	(.43)

NOTE.—*T*-tests of equality in means were performed, comparing the means of each variable in each treatment to the ones in the baseline treatment.

[a] Beta refers to the time-inconsistency discount factor. It is the ratio between the time-discount factor of now versus 1 week and the discount factor of 1 week versus 2 weeks. Therefore beta different from one refers to time-inconsistent preferences.

[b] Delta refers to the discount factor of 1 week versus 2 weeks estimated in the experiment. (See the online supplemental appendix for the construction of beta and delta.)

[c] Race is self-reported.

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

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

reproduce the analysis of column 3 using probit (and report average marginal effects).[54]

1. Baseline Treatment: Parents' Choices

In the baseline treatment group, parents reveal an overwhelming preference for conditional payments: 82 percent of the parents are willing to pay to keep the conditionality on the transfers. The parent's average (censored) willingness to pay to keep the conditionality is R$37.3. This is likely a lower bound of the true average due to censoring (63.3 percent of the parents in the baseline treatment group prefer a CCT of R$120 to a CT of R$180, the maximum in our protocol). That is, parents, on average, are willing to forgo at least R$37.3 to keep the conditionality, over 6 percent of their pre-CCT level of household monthly income.

[54] In the probit regressions, we lose five observations due to some variables (either surveyor or race dummies) perfectly predicting either success or failure for the outcome variable. These observations are not dropped with OLS regressions. Results with logit regressions are very similar to those with probit regressions and are available upon request. For both our probit and OLS regressions, standard errors are clustered by school. For robustness, we also reproduced the regressions clustering the standard errors by surveyor. The results are available in the online supplemental appendix. We also create a second binary outcome variable, a dummy variable that is equal to one if the parent prefers a R$120 CCT to a R$120 CT, and zero otherwise. The analysis using this outcome variable is in the supplemental appendix.

TABLE 2
BELIEFS ABOUT RETURNS TO SCHOOLING

	MEAN (in R$)			MEDIAN (in R$)			NATIONAL AVERAGE (PNAD 2007)
	Parent's Belief	Child's Belief	Difference	Parent's Belief	Child's Belief	Difference	
Beliefs about child's monthly income if child drops out and gets a job	365	392	−27	460	450	10	
	(150)	(243)					
Beliefs about child's monthly income increase with:							
1 more year of school	198	217	−19	150	100	50*	
	(200)	(509)					
2 more years of school	323	313	10	250	200	50**	
	(272)	(612)					
Secondary degree	576	561	15	500	400	100***	
	(388)	(774)					
College degree	2,066	2,197	−131	1,143	800	343***	
	(5,677)	(12,509)					
Yearly average of beliefs about rate of returns to schooling	22%	19.2%	2.8%**	20%	16%	4%***	
	(12.3)	(13.8)					
Perceived and observed wages:							
High school graduate wage	953	956	−4	865	765	100**	904
	(424)	(957)					
College graduate wage	2,426	2,606	−180	1,500	1,117	383***	1,844
	(5,805)	(12,570)					

NOTE.—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. Standard deviations are in parentheses. 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).

* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

TABLE 3
HOW MUCH DO PARENTS KNOW?

Respondent	Parent	Child	Difference
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	−.36
Did the child miss any day of school in the last 2 months? (% answering "yes")	50.96	56.04	−5.08
How many days did the child miss in the last 2 months?	1.36	1.97	−.60*
Did the child miss any day this year because the child was sick? (% answering "yes")	43.81	32.70	11.12***
Did the child miss any day because the child did not want to go? (% answering "yes")	9.05	15.87	−6.82***

NOTE.—*T*-tests of equality in means from paired observations (parent and child).
* Significant at the 10 percent level.
*** Significant at the 1 percent level.

TABLE 4
REGRESSIONS: TREATMENT EFFECTS
Dependent Variable = Dummy for Parent Prefers R$120 CCT to R$125 CT

	OLS (1)	OLS (2)	OLS (3)	Probit[a] (4)
Text message treatment dummy	−.6136	−.5819	−.4744	−.4634
	(.087)***	(.091)***	(.097)***	(.072)***
Don't tell treatment dummy	−.6433	−.6119	−.5208	−.5409
	(.070)***	(.080)***	(.060)***	(.123)***
Individual and household covariates	No	Yes	Yes	Yes
Surveyor and school dummies	No	No	Yes	Yes
Observations	156	156	156	151

NOTE.—Mean of dependent variable in the baseline group = .82. The sample was restricted to households that answered the entire survey, and thus two observations were lost. Controls in cols. 2–4 include log of household income, employed parent dummy, employed parent's spouse dummy, age (parent and child), male dummy (parent and child), higher show-up fee dummy, weekly discount factor (parent and child), time-inconsistency discount factor [beta] (for parent and child), marital status (parent), religion, dummies, number of children in the household, household is earning CCTs for more than one child, race dummies (parent and child), years of schooling (parent and child). Controls in cols. 3–4 include school and surveyor dummies. In the probit regression, five observations are lost due to some variables (either surveyor dummies or race dummies) perfectly predicting either success or failure for the outcome variable. Standard errors (clustered by school) are in parentheses.
[a] Average marginal effects reported.
*** Significant at the 1 percent level.

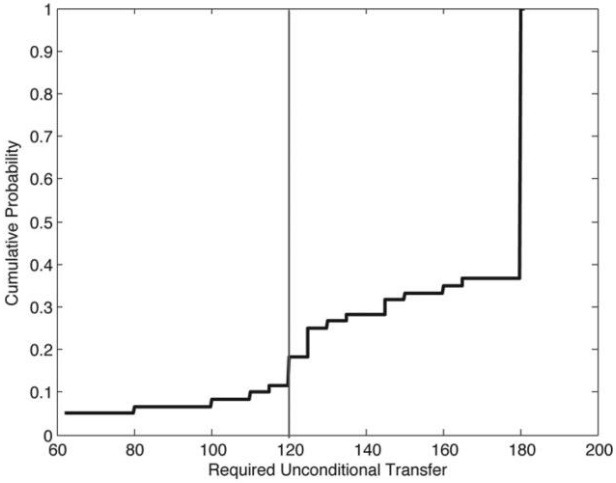

Fig. 2.—Cumulative probability for the cash transfer that makes the parent indifferent to a R$120 CCT—baseline treatment group.

We plot the cumulative distribution for the censored willingness to pay in the baseline treatment group in figure 2. The first quartile is R$7.5, and the median is R$60, the maximum possible willingness to pay.

This preference for the CCT is consistent with the theoretical framework in the previous section. The results reveal that parents value the CCT for reasons beyond slackening short-run credit constraints (as the CT would work similarly). It also reveals that parents highly value their children's education, which is not an insignificant fact. When we analyze the text message treatment, we will provide evidence that the parents highly value monitoring.

There could be a concern that social desirability and/or experimenter demand effects may be driving the results. We will later address these issues.

2. Baseline Treatment: Children's Choices

With the exception of the don't tell treatment, the child was offered the same choices as the parent between conditional and unconditional payments, but their choice environment was always that of the baseline treatment. We elicited the baseline 25 choices between different conditional and unconditional payments for 161 children. However, their choices were not incentived, so we examine the results with reasonable caution. We also observe preference for conditional over unconditional payments when the child is making the choices: 54 percent are willing to pay to keep the conditionality on the transfers, and the median willingness to pay among children is R$5. This is consistent with predictions from our theoretical framework. The child prefers a CCT because she believes that she will be given an incentive to go to school beyond her costs with a CCT and would not be given such an incentive without a CCT (i.e., under a CT).

If we restrict our attention to families in the baseline treatment, where both the parent and the child faced the same choices, we observe a significantly higher willingness to pay among parents than among children (the differences between parents' and children's choices are significant at 1 percent using a *t*-test for the dummy outcome variable and a Wilcoxon signed-rank test for the willingness to pay measure). Regressing the child's choices on our measured observables revealed no pattern of important predictors among the covariates. The results are reported in the online supplemental appendix.

3. Monitoring and Parental Control: Text Message Treatment

This treatment is designed to assess, together with the baseline treatment, the "testable prediction" generated in the theoretical framework. Suppose a proportion $\mu > 0$ of adults are willing to pay to keep the conditionality on the payments, as in the baseline. If we add superior monitoring technology to both the CT and CCT environments, such that the monitoring technologies are now better and equal, then no one will be willing to pay for the CCT, $\mu = 0$. Confirming this testable prediction confirms the thrust of the model: parents are willing to pay for information as a means to allow them to induce their children to go to school.

Both in the comparison of means and in the regression analysis, we observe a substantial decrease in parental willingness to pay to keep the conditionality when compared to the baseline treatment. The treatment effects in table 4 attest that observability of school attendance 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 element of the cash transfer. 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 likelihood of a parent being willing to pay a positive amount to keep transfers conditional is reduced from 0.82 to 0.34 compared to the baseline treatment, when examining the full specification using OLS (col. 3), and from 0.82 to 0.36 when looking at the average marginal effects in the probit regression (col. 4). In all specifications, the treatment effects are significant at 1 percent. Figure 3 illustrates the treat-

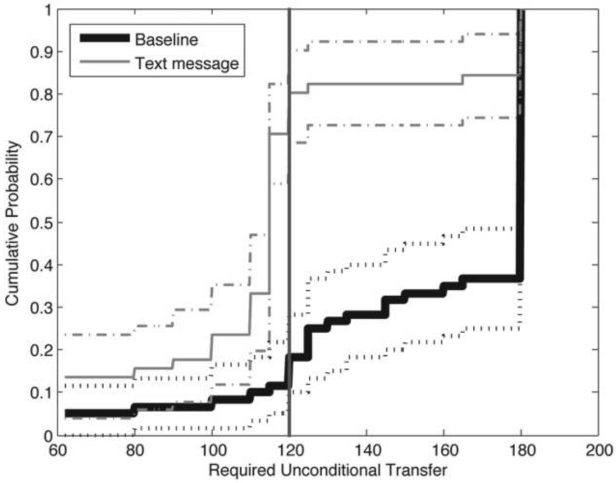

Fig. 3.—Cumulative probability for the cash transfer that makes the parent indifferent to a R$120 CCT—baseline and text message treatment groups (with 95 percent bootstrap confidence intervals and 1,000 bootstrap samples). Resampling with replacement from the empirical distribution was done 1,000 times. From these 1,000 bootstrap samples, the confidence intervals were computed for each point on the cumulative distribution.

ment effects on the willingness to pay variable by plotting the cumulative distribution for the censored willingness to pay in both the baseline and the text message treatment groups.

Taken together, the findings from the baseline and the text message treatments show that parents are willing to pay for information. In the baseline treatment, parents are willing to pay for the conditionality. Once they are provided with good monitoring of their child, however, they are less willing to pay for the conditionality. For many families, the CCT program provides nothing other than cash and monitoring services, and many families are willing to give up some cash to preserve the quality of monitoring.

It is worth noting that the simple fact that the demand for the conditionality is very high in one treatment and all but turned off in another treatment addresses many potential confounding hypotheses for the demand shown in the baseline. Demand for the conditionality in the baseline could have come from multiple sources; for example, parents may think that this is a "referendum" on the CCT program as a whole or that the CCT is the status quo or perhaps parents do not want to look like they wanted a free handout. The fact that demand has been turned off in a very similar setting rules out such confounding explanations.

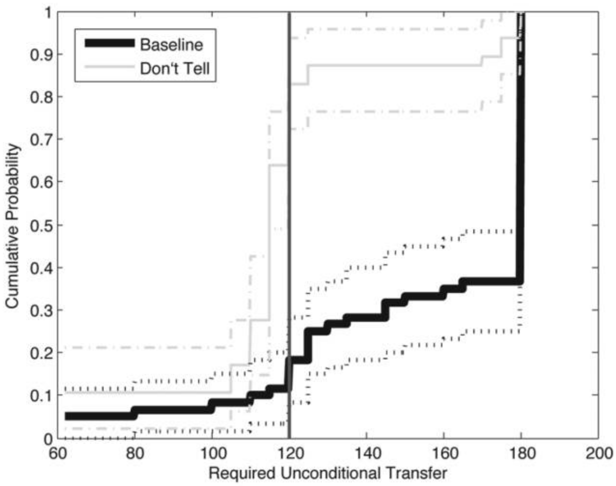

FIG. 4.—Cumulative probability for the cash transfer that makes the parent indifferent to a R$120 CCT—baseline and don't tell treatment groups (with 95 percent bootstrap confidence intervals and 1,000 bootstrap samples). Resampling with replacement from the empirical distribution was done 1,000 times. From these 1,000 bootstrap samples, the confidence intervals were computed for each point on the cumulative distribution.

4. Conditional Cash Transfers to Control the Child: Don't Tell Treatment

As discussed in the experimental design, this treatment tests the assumptions that the parent believes the child is the agent of the decision and that the child does not prefer schooling as strongly as the parent does. Both in the comparisons of means and the regression results, we observe a substantial drop in the willingness to pay to keep the conditionality on the transfers compared to the baseline treatment, as shown in table 4. We can see the treatment effects on the willingness to pay measure in figure 4, which plots the cumulative distribution for the censored willingness to pay in both the baseline and the don't tell treatment groups. We conclude that the parents' willingness to pay for the conditionality is primarily for monitoring of their child's behavior rather than someone else's (e.g., their spouse's).

5. Experimenter Demand Effects, Social Desirability, and Other Concerns

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 17 surveyors with varying levels of experience and knowledge. Some helped with only a few parents, while some did as many as 42. Some came to our training session, while others (unfortunately) had minimal training. Although training and experience are not random, their relation to treatment effects could be illuminating if social desirability or demand effects are significant concerns. 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. We interact the treatment dummies with (i) an indicator of whether the surveyor received training or (ii) a measure of surveyor experience (for each experiment session, we calculate the number of interviews that each surveyor had conducted before that session). The results, reported in the online supplemental appendix, indicate that surveyor training and experience do not significantly affect the treatment effects and that the direction of the interaction is generally toward attenuating our main findings.

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 should they 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 of college age. 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.

One might wonder if our results imply the existence of a market failure—parents are willing to pay significant sums of money for monitoring devices. However, it is important to note that under the status quo, these parents (and low-income households in general) have been receiving conditional cash transfers from the local government since 1995 and therefore already have access to a good monitoring device. As a result, in the experiment, parents are requiring large sums of money to drop the conditionality/monitoring device. Each parent is currently observing π_{CCT} and making inferences of what monitoring would look like counterfactually, π. A parent's beliefs of π might be different were everyone to lose their CCT program rather than just this one parent. If a small number of parents get the conditionality removed in our experiment, they might think that they will not be able to induce the government to introduce an alternative system just for them.[55]

Finally, one could be concerned that parents prefer to keep the conditionality in the baseline treatment as a means to signal to their child that they care about their child's education in order to stimulate school attendance. If this story is true, the text message treatment results suggest that parents think that providing better monitoring (the only difference from the baseline treatment) is a substitute for parental signaling for stimulating school attendance; hence, even under this story, monitoring is an important factor in the schooling decision. Moreover, parents sending a costly signal to their child to promote school attendance suggests that (i) the child is the agent of the schooling decision, (ii) the child might not want to attend school absent the signal, and (iii) parents are willing to forgo money to make sure the child attends school. Such elements are consistent with the assumptions from our theoretical framework. Although we cannot show that the reason parents demand monitoring is because of the specified channel in the theoretical framework, the signaling interpretation of the preference for conditional payments suggests that parents believe monitoring is sufficient to induce their child to attend school.

VI. Concluding Remarks

Using a real-stakes experiment, we identify a moral hazard problem in school attendance in poor households with adolescent children in urban Brazil. Parents are willing to pay substantial sums for mechanisms that can increase their monitoring over their children's school attendance.

Our findings provide a new understanding not only of schooling among poor families but also about the efficacy of CCT programs. Traditionally, CCTs are believed to operate through one of two channels—the cash transferred slackens credit constraints in the household and/or the incentive to send the children to school is great enough to change the parents' choices regarding schooling. Our results additionally suggest that the monitoring provided by CCTs is first order for solving the school attendance problem.

This evidence can help frame the CT versus CCT debate. 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 op-

[55] Also, 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 to their common monitoring problem.

timally in education. In the context of schooling that we study, however, a CT alone may be insufficient to induce schooling; monitoring is also necessary. The parental decisions in our study are prima facie evidence that CCTs are preferable to CTs without sufficient monitoring technology. We should note, however, that we cannot speak to whether monitoring alone would be preferable to a CT or would be enough to spark school attendance.

These findings can 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. This approach assumes that parents make the schooling decision for their children and that intrahousehold information asymmetries are absent. Viewing high dropout rates through that paradigm leaves one to conclude that either parents underestimate the actual returns to schooling or that they cannot afford to have a child not working, thus preferring to have the child drop out.[56] In our sample, parents have accurate beliefs about the actual returns to schooling and demonstrate a strong preference to keep their children in school. 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.[57]

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 instead of focusing on parents, as many do in practice. If parents in our study are correct in their beliefs, an important component of the solution for promoting school attendance is to provide them with more information (and therefore increased control) regarding their children's actions.

If a government that is already transferring cash to low-income households also wants to provide good monitoring over school attendance behavior, there are different options, such as making the cash transfers conditional on school attendance or sending parents text message notifications.

[56] Edmonds (2006) and Edmonds and Schady (2009) provide evidence, in South Africa and Ecuador, respectively, that increasing household income through transfers reduces the allocation of children to labor and increases school attendance.

[57] Our regression results suggest that poorer parents may be willing to pay less for information/conditionality. This is consistent with the results in Bursztyn (2012), which show that poorer voters in Brazil are less likely to favor public educational spending relative to increases in cash transfers.

The relative cost effectiveness of these two policy designs analyzed in the paper depends on the comparison of the costs associated with enforcing the conditionality and those associated with sending text messages. In 1995, when the *Bolsa Escola* program was introduced in the Distrito Federal state, cell phones were not widespread among low-income households and making transfers conditional could have been the optimal solution for meeting parental demand for monitoring. It is possible that CCTs are no longer the most efficient policy—the monitoring provided by text messages might be cheaper—but remain in use quite simply because they are the status quo and they work.[58]

We end with suggestions for directions of future research. It would be interesting to address the extent to which our findings concerning the key role of intergenerational conflict and agency problems in schooling decisions are central to schooling choices in other contexts. The analysis could be particularly relevant in poor, rural areas in developing countries, where the nature of the intrahousehold problem might be different from the one addressed in this paper, or in areas of urban poverty in developed countries. Understanding how the findings export will rely first on understanding in what settings monitoring would be valuable. The information provided by text messages in our experiment was highly valued by parents, but it was always accompanied by a large cash transfer. Would information prove valuable to families who may not have resources to implement school attendance? It remains unclear if sending text messages alone to very poor areas in developing countries would be sufficient to induce schooling. Understanding that might help illuminate how and why governments may implement truancy laws. As CCTs might be a governmental response to a need for monitoring, truancy laws might be implemented when the public also lacks the resources necessary to provide incentive for their children's education. Although it is beyond the scope of this paper, perhaps our findings can open a door toward understanding the optimal choice by the state of what technology to use to enforce schooling, be it monitoring, CCTs, or truancy laws. Finally, another line of important future research would

[58] De Janvry et al. (2010) not only show that dropout rates are very low under the national CCT program in Brazil, they also find that good program performance significantly increases the probability of reelection of mayors in Brazil. As a result, beyond the cost of switching to a new monitoring system, such as text messages, politicians might also find it risky to move away from a program that yields political returns. Still, some governments in Brazil seem to realize the relevance of providing monitoring even beyond the level provided by CCT technology and the cost effectiveness of text message notifications. As an example, since 2009, the state government of Rio de Janeiro has been implementing an automated system of text message notifications to households with children enrolled in state public schools (see André Zahar [2009] "Escolas do Rio vão usar celular para alertar pai sobre ausencia de aluno" [Rio schools are going to use text messages to alert parents of students' absences], http://www1.folha.uol.com.br/folha/informatica/ult124u499847.shtml, June 2).

be to see how our findings would apply to younger children: 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); however, parents are likely to have much better monitoring over younger children's behavior.

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