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Own and Sibling Effects of Conditional Cash Transfer Programs

Theory and Evidence from Cambodia

Francisco H. G. Ferreira
Deon Filmer
Norbert Schady

Abstract

Conditional cash transfers have been adopted by a large number of countries in the past decade. Although the impacts of these programs have been studied extensively, understanding of the economic mechanisms through which cash and conditions affect household decisions remains incomplete. This paper uses evidence from a program in Cambodia, where eligibility varied substantially among siblings in the same household, to illustrate these effects. A model of schooling decisions highlights three different effects of a child-specific conditional cash transfer: an income effect, a substitution effect, and a displacement effect. The model predicts that such a conditional cash transfer will increase enrollment

for eligible children—due to all three effects—but have an ambiguous effect on ineligible siblings. The ambiguity arises from the interaction of a positive income effect with a negative displacement effect. These predictions are shown to be consistent with evidence from Cambodia, where the child-specific program makes modest transfers, conditional on school enrollment for children of middle-school age. Scholarship recipients were more than 20 percentage points more likely to be enrolled in school and 10 percentage points less likely to work for pay. However, the school enrollment and work of ineligible siblings was largely unaffected by the program.

Own and Sibling Effects of Conditional Cash Transfer Programs: Theory and Evidence from Cambodia♣

Francisco H. G. Ferreira
Deon Filmer
Norbert Schady

Development Research Group
The World Bank

1.   Introduction

Many programs in the developing world make transfers to poor households conditional on the school enrollment of school-aged children. These programs have been shown to increase school attendance in a variety of settings. Frequently, impacts are concentrated on children in grades where, in the absence of the program, school dropout is large (see Schultz 2004 on the PROGRESA program in Mexico, and Schady and Araujo 2008 on the BDH program in Ecuador). This has led to calls for cash transfers to be directed towards households with children in these transition grades, on the grounds that this would be a more cost-effective way of increasing school attainment (de Janvry and Sadoulet 2006).

Such child-specific conditional transfers could potentially have important implications for the school enrollment of *ineligible* siblings, but these effects are hard to sign ex ante. Depending on the magnitude of the transfer, its income effect might lead to increased enrollment for *all* children in the household, whether or not the transfer is conditioned on any action on their part. Various models of schooling and child labor predict that greater family income during childhood increases school enrollment and reduces the time children spend working, quite independently of any conditionality (Ben-Porath 1967; Basu and Van 1998; Baland and Robinson 2000). On the other hand, cash transfer programs conditional on the school enrollment of one specific child might lead parents to reallocate child work away from the recipient and to other children in the household. Evidence of this effect has been found in some settings, including Colombia (Barrera-Osorio et al. 2008). More generally, the transfer may provide an incentive for parents to specialize in the education of the recipient, leading to a displacement of—less schooling for—his or her siblings.

In this paper, we assess the impact of a child-specific conditional cash transfer program on the school enrollment and work of recipients and their ineligible siblings. For this purpose, we first construct a simple model of child occupational choice. The main prediction of the model is that child-specific conditional cash transfer programs will unambiguously increase school enrollment among eligible children, but will have an ambiguous effect on ineligible siblings. The effect on eligible children reflects mutual reinforcement between a positive income effect, which affects the entire household, and a positive

but child-specific substitution effect, which is brought on by the reduction in the opportunity cost of schooling for the eligible child. The displacement effect is also positive for these children, as it refers to situations in which they displace their siblings from school. The ambiguity of the effect on ineligible siblings arises from the opposing (positive) income and (negative) displacement effects of the transfer on these children.

We then take the predictions of the model to data from Cambodia, where a program known as the CESSP Scholarship Program (CSP) makes very modest transfers, equivalent to between 2 and 3 percent of the total expenditures of the average recipient household, conditional on school enrollment for children of middle-school age. The results show that children who received scholarships were about 20 percentage points more likely to be enrolled in school, and 10 percentage points less likely to work for pay. However, the school enrollment and work of *ineligible* siblings was largely unaffected by the program. These results are robust to a variety of specification checks.

These findings have important implications for the way in which we think about household decisions regarding school enrollment and child labor, and for the design of cash transfer programs. We highlight three. First, the very large effect of the CSP program on the behavior of recipients confirms that scholarship and conditional cash transfer programs may be an effective way of increasing school enrollment in low income countries (see Filmer and Schady 2008, 2009a on Cambodia; Chaudhury and Parajuli 2008 on Pakistan). This is important because most of the evidence on these programs refers to Latin America, where income levels are generally higher and institutions stronger than in many African and Asian settings where such programs are now being implemented.[1]

Second, and as a cautionary counterpoint, our model suggests that very narrow age ranges for benefit eligibility could potentially lead to the unintended displacement of *some* poor children from school. Although this displacement did not take place in Cambodia (where sibling enrollment was unchanged), it is certainly a theoretical possibility, and one which appears to have been observed in

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[1] The literature from Latin America is extensive—see, among others, Schultz (2004) and Behrman et al. (2005) on Mexico; Schady and Araujo (2008) on Ecuador; Attanasio et al. (2005) on Colombia; Glewwe and Olinto (2004) on Honduras; Maluccio and Flores (2005) on Nicaragua. Fiszbein and Schady (2009) review these and other studies.

practice in Colombia, where child-specific CCTs increased own enrollment, but reduced sibling enrollment. Our model allows for both the empirical results observed in Cambodia and Colombia: The net effect on ineligible siblings depends on the relative magnitudes of the income effect of the transfer and of its displacement effect. This depends on size of the of the income transfer, as well as on the extent to which—in the absence of the program—eligible children would have been more, or less, likely to be enrolled than their ineligible siblings.

Third, the model cautions against generally interpreting a comparison of program effects for recipients and their siblings as definitive evidence about the relative size of the income and substitution effects of the transfer. The much larger net impact on eligible children is certainly *consistent with* an important role for the substitution effect, and this would confirm other findings in the literature (Bourguignon, Ferreira and Leite 2003; Todd and Wolpin 2006; Schady and Araujo 2008; de Brauww and Hoddinott 2009). But it can not be interpreted as *identifying* these effects, since there is a third effect—namely the displacement effect—which contributes negatively to the sibling effect, and positively to the own effect. In a context where ineligible children in transfer-receiving households are relatively numerous, more work is needed on quantifying the displacement effect.

The rest of the paper proceeds as follows. The next section describes our simple schooling decision model and introduces a child-specific CCT. Section 3 briefly discusses the CSP and the data we use for the evaluation. Section 4 discusses our empirical specification. The main results are presented in Section 5. Section 6 concludes.

2. The model

This section presents a basic model of schooling decisions, which adapts the early insights of Ben-Porath (1967), and of a large subsequent literature, to the specific context of a multiple-children household facing a child-specific CCT intervention. The model has two periods, and is partial equilibrium in nature. There is a continuum of households, each consisting of a parent and two children. Parents live only for the first period, but care about (the perfect foresight expectation of) their children's wellbeing in

period 2. They take all decisions in period 1 so as to maximize household welfare, which is a function of the family's consumption level in period 1, and of the expected utility of both children: $W(c_p, U_1, U_2)$.

Following Becker (1991) and Baland and Robinson (2000), we assume that this function is additively separable as follows:[2]

$$W(c_p, U_1, U_2) = U(c_p) + \beta[U(c_1) + U(c_2)] \qquad (1)$$

The subscripts p, 1 and 2 denote the parent(s) and each child respectively. $U(.)$ is an individual utility function that is common across all individuals in the household, and which satisfies the usual properties: $U' > 0$, $U'' < 0$, $U(0) = 0$, $\lim_{c \to 0} U'(c) = \infty$ and $\lim_{c \to \infty} U'(c) = 0$. Adults earn income A (from labor and capital), which is determined exogenously to the model. A is distributed according to a cumulative distribution function $F(A)$, with positive mass everywhere on a non-degenerate support $(0, \overline{A})$. This exogenous adult income is the only source of ex ante household heterogeneity in the model.

Parents use all of their endowment of time in period 1 to supply labor inelastically. They then choose between two occupations for their children in period 1: children can either work, in which case they are paid a wage w, or they can go to school.[3] The model assumes that there are no school fees, but this is merely an innocuous simplifying assumption. If each child had to pay a fixed fee f to attend school, then the total cost of schooling would be $w + f$ instead of just the opportunity cost w, and adult disposable income when both children are enrolled would be $B = A - 2f$. The remainder of the model would be unchanged.

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[2] Our parental welfare function is a simple transformation of Baland and Robinson's (2000), adjusted for the fact that parents do not consume in period 2. This eliminates the discussion of bequests and savings which, although essential for the efficiency argument in Baland and Robinson (2000), is not important for our purposes. Our discount parameter β plays the same role as their "altruism parameter" δ. For reasons which will become obvious, we focus on two (rather than n) children, but nothing of substance hinges on this, other than considerable presentational simplicity.

[3] As in Basu and Van (1998) and Baland and Robinson (2000), we abstract from the actual decision-making process within the household. As argued by Basu and Van (1998): "[the] model does not conflict with recent evidence and theories which ask for the rejection of the 'unitary model' of the household. This is because we assume that a child's labor supply decision is taken by a parent. [...] this decision could be different if the decision-making were shifted to another member of household." (p. 415).

The choice of occupation for child i is denoted by σ_i, which takes the value 1 if child i is sent to school, and 0 if she is sent to work.[4] There is a positive return to schooling, so that their period 2 income is higher if they attend school in period 1 (h), than if they do not (θ). There are no capital markets and parents cannot leave financial bequests to their children, so that the only way to invest in their future is through education.

The household's problem is then to maximize (1), by choice of σ_1, σ_2 subject to:

$$c_p = A + w(2 - \sigma_1 - \sigma_2) \tag{2}$$

$$c_i = h\sigma_i + \theta(1 - \sigma_i), \quad i = 1,2 \tag{3}$$

$$\sigma_i \in \{0,1\} \quad i = 1,2 \tag{4}$$

where $w > 0$ and $h > \theta > 0$.

The discrete nature of the control variable σ implies that the optimal decision for each household cannot be obtained from calculus. Instead, the utility levels arising from each possible decision must be compared against one another. Since instantaneous utility is concave in income, these levels will depend on the exogenous level of adult income, A. For example, households will choose to enroll both their children if:

$$U(A) + 2\beta U(h) \geq U(A + w) + \beta[U(h) + U(\theta)] \tag{5}$$

They will choose to enroll one child, but not both if

$$U(A + w) + \beta[U(h) + U(\theta)] > U(A) + 2\beta U(h) \text{ and} \tag{6}$$

$$U(A + w) + \beta[U(h) + U(\theta)] \geq U(A + 2w) + 2\beta U(\theta)$$

In fact, we can show that:

Proposition 1: There exist positive income levels A^* and A^{**}, $A^* < A^{**}$, such that:

(i) for $A < A^*$, $\sigma_1 = \sigma_2 = 0$;

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[4] Although the binary nature of this decision problem simplifies the presentation, the qualitative results extend to a version of the model in which σ is continuous in [0,1], so that children may divide their time between school and work. This extension, which also allows for child leisure, is not presented here to economize on space, but it is available from the authors on request.

(ii) for $A^* \leq A < A^{**}$, $\sigma_i = 0, \sigma_j = 1$; $i \neq j$

(iii) for $A^{**} \leq A$, $\sigma_1 = \sigma_2 = 1$.

Proof: See Appendix 1.

  Proposition 1 states that when households are identical in all dimensions other than adult income, and school enrollment has an opportunity cost (given by the forgone earnings of children in period 1), then enrollment decisions vary monotonically with family income. Above a certain adult income level A^{**}, all children are enrolled in school (and none work). Below a lower threshold A^*, no children are enrolled (and all work). Between the two thresholds, households can afford to (and do) enroll one child, but not the other. In that income range, and under our simplifying assumption that siblings are identical (in schooling ability, in labor productivity and in how much their parents value them), the decision of which child to enroll from each household is random, with child i being sent to school with probability π_i ($\pi_1 + \pi_2 = 1$).[5]

  Figure 1, which illustrates the proof of Proposition 1, shows that A^* marks the income level at which the discounted gain in expected child utility from enrollment ($\beta[U(h) - U(\theta)]$) equates the opportunity cost in first-period consumption from forgoing the earnings of a first child ($U(A + 2w) - U(A + w)$). Point A^{**} marks the corresponding income level for the second child, and it is clear that the existence of the intermediate range depends on utility being strictly concave in first period consumption.

  There are clear parallels to the previous literature. This strong negative relationship between household income and child labor is reminiscent of Basu and Van's (1998) result that child labor arises only from households in poverty, with no need for a strong "luxury axiom". As in Baland and Robinson

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[5] In a richer model, children might be allowed to differ in school ability, work productivity or parental preference. Such differences would alter the model in two ways. First, for households with $A^* \leq A < A^{**}$, child heterogeneity would alter the pre-transfer decision of which children to enroll. This change can be accommodated by our set up with $\pi_i \neq \pi_j$. More importantly, however, child heterogeneity would lead to a finite elasticity for the displacement of ineligible by eligible children once the transfer is introduced. This extension is left for future work, and we note only that the magnitude of the displacement effect we consider here is likely to be an upper-bound, since it assumes perfect substitutability across children.

(2000), the result is driven by missing capital markets: if families could borrow in period 1 against the child's income in period 2 then, for sufficiently large returns to education (i.e. for a sufficiently large value of $h - \theta$), child labor could be eradicated. Without that ability to borrow, poor households, for whom the marginal value of period 1 consumption is very high, use child-labor as an (inferior) alternative consumption-smoothing mechanism.

In this setting, a conditional cash transfer is a monetary payment τ in period 1, which is made if and only if a child is enrolled in school. Since we are interested in a situation where some children are eligible for the transfer but others are not, even within the same household, assume (without loss of generality) that only child 1 is eligible for the transfer. This policy leaves the household's problem unchanged, except for equation (2), which is replaced by:

$$c_p = A + w(2 - \sigma_1 - \sigma_2) + \tau\sigma_1 \tag{2'}$$

The effect of this policy on the household's decisions is twofold. First, it changes the adult income thresholds at which first one, and then both children are enrolled. Parents will now enroll both children if:

$$U(A + \tau) + 2\beta U(h) \geq U(A + w + \tau) + \beta[U(h) + U(\theta)] \tag{7}$$

The income level which satisfies (7) as an equality is A_τ^{**}. It is easy to see, once again from the concavity of the utility function, that $A_\tau^{**} < A^{**}$. Parents will enroll one child (but not both), if (7) does not hold and:

$$U(A + w + \tau) + \beta[U(h) + U(\theta)] \geq U(A + 2w) + 2\beta U(\theta) \tag{8}$$

A_τ^*, which solves (8) as an equality, is less than A^{**}.

The second change in household behavior is that, under the maintained assumption that siblings are identical, the choice of which child to enroll for those parents who enroll a single child is now no longer random. They all choose to enroll child 1, who is eligible for the transfer, which leads to the potential displacement effect.

Adult labor continues to be supplied inelastically; Appendix 2 shows that this is a reasonable approximation for Cambodia.

Figure 2 illustrates the changes brought about by the conditional cash transfer. The two-child enrollment threshold falls (from A** to A_τ**) because of a pure income effect: the value of the transfer is added to both sides of inequality (7), and the dashed curve $U(A + w + \tau) - U(A + \tau)$ lies below the original curve $U(A + w) - U(A)$ simply because that difference declines with income.

The reduction in the one-child enrollment threshold (from A* to A_τ*), on the other hand, arises from both an income and a substitution effect. The value of the transfer is added *only* to the left-hand-side of inequality (8), so that the dashed curve $U(A + 2w) - U(A + w + \tau)$ lies below the original curve $U(A + 2w) - U(A + w)$ because of a full "price effect", comprising both an income and substitution effect.

This allows us to state:

Proposition 2: The introduction of a child-specific conditional cash transfer that alters the household budget constraint from (2) to (2') will:

    (i) unambiguously increase enrollment of the eligible children (child 1);

    (ii) have an ambiguous effect on the enrollment of the ineligible children (child 2).

Proof of Proposition 2.

Denote by π_i the proportion of children of type i (i = 1,2) enrolled by families with a single child attending school prior to the introduction of the transfer. If children of types 1 and 2 were enrolled with equal probability by families with income in the interval (A*, A**), then $\pi_1 = \pi_2 = 0.5$. Then pre-transfer enrollment for child i was given by:

$$E_i = 1 - F(A**) + \pi_i [F(A**) - F(A*)] \quad i = 1, 2 \qquad (9)$$

Post-transfer enrollment is different for the two types of children, and given by:

$$E_1 = 1 - F\left(A_\tau^*\right) \tag{10}$$

$$E_2 = 1 - F\left(A_\tau^{**}\right) \tag{10'}$$

Changes in enrollment are obtained from subtracting (9) from (10):

$$\Delta E_1 = (1 - \pi_1)F(A**) + \pi_1 F(A*) - F\left(A_\tau^*\right) > 0 \tag{11}$$

where the inequality arises from the fact that $F(A**) > F\left(A_\tau^*\right)$ and $F(A*) > F\left(A_\tau^*\right)$. This proves (i).

$$\Delta E_2 = (1 - \pi_2)F(A**) + \pi_2 F(A*) - F\left(A_\tau^{**}\right) \tag{12}$$

Since A_τ^{**} may be greater than A* (and indeed will be greater for $\tau < w$), the sign of ΔE_2 may depend on the specific value of $\pi_2 \in (0,1)$. This proves (ii). ∎

While the model predicts an ambiguous impact of the child-specific CCT on siblings, it does allow us to go one step further and assess the likely relative size of the impacts on recipients and their siblings. Subtracting (10) from (9) is a comparison of the changes in enrollment for eligible and non-eligible children. $\Delta E_1 > \Delta E_2$ so long as

$$F\left(A_\tau^{**}\right) - F\left(A_\tau^*\right) > (\pi_1 - \pi_2)\left[F(A**) - F(A*)\right] \tag{13}$$

A corollary of Proposition 2, then, is that $\pi_1 \leq \pi_2$ is a sufficient (but not necessary) condition for $\Delta E_1 > \Delta E_2$. In other words, if eligible children were initially either equally or less likely to be enrolled than ineligible children, then their enrollment will increase by more as a result of the transfer. The necessary condition, which is given by (11) is evidently much weaker: if the measure of the population between A* and A** is not very different from the mass between A_τ^* and A_τ^{**}, then one would need a pre-transfer situation in which almost all eligible children were already enrolled ($\pi_1 \to 1$), while almost all ineligible children were not ($\pi_2 \to 0$). It is hard to conceive that a CCT would be targeted to type-1 children if this were the case. We thus say that (13) "generally" holds, and that the enrollment effect of CCTs on eligible children is in those circumstances larger than the enrollment effect on ineligible siblings.

In sum, our simple model of schooling decisions for a multi-child household predicts that a child-specific conditional cash transfer will lead to increased enrollment for eligible children. This increase reflects the combination of effects. First, there is a displacement effect among those households that only enroll one child: they tend to replace their ineligible children with their eligible siblings in school. Second, some households that would not send any children to school in the absence of the program are now compelled to send an (eligible) child to school, due both to a substitution effect (the opportunity cost of that decision has fallen from w to $w - \tau$) and to an income effect (an increase in unearned period 1 income reduces the utility loss from forgoing that opportunity cost).

The effect on the enrollment of ineligible children is ambiguous. The displacement effect works against them, with families that send a single child to school shifting away from them towards their eligible children. Furthermore, they do not benefit from a substitution effect, since the opportunity cost of their going to school remains equal to w. However, some ineligible children can benefit from an income effect. Those are children in households whose income levels in the absence of the transfer were just insufficient to enroll both children but who, given the extra income from the transfer, are now willing to forgo the child earnings from their second child as well. (These are households with exogenous incomes between A_τ^{**} and A** in Figure 2.) This is a pure income effect, since they receive no additional transfer for this added enrollment.

3.   Program and data[6]

Cambodia has had programs that offer "scholarships" to poor children making the transition from primary to lower secondary school for a number of years. These programs have operated in some regions of the country and not others, and have been funded from a variety of sources, including government budgets, loans from multilateral and bilateral donor agencies, and NGOs. One of the programs that predated the CSP, known as the Japan Fund for Poverty Reduction (JFPR) scholarship program, was

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[6] Sections 3 and 4 draw from Filmer and Schady (2009b).

targeted at girls (and children from ethnic minorities) making the transition from primary school to lower secondary school. Filmer and Schady (2008) evaluate the program and conclude that, despite the small amount of the transfer, which (like the CSP) accounted for only 2-3 percent of the total consumption of the median recipient household, the JFPR increased enrollment rates by almost 30 percentage points. Program effects were particularly large among girls in the poorest households.

In the time period we study, the CSP operated in 100 of the approximately 800 middle schools in Cambodia. These schools were selected on the basis of administrative data which indicated that poverty rates in the areas served by these schools were high and, by implication, secondary school enrollment rates low. In addition, there was a requirement that none of the selected schools participate in other scholarship programs, including the JFPR.

The selection of CSP recipients within eligible schools was done in three stages. First, using administrative data from the 100 CSP schools, program officials identified all of the primary "feeder" schools for every CSP school. (A primary school was designated a feeder school if it had sent graduating students to a given CSP school in recent years.)

Second, within feeder schools *all* 6th graders were asked to complete a CSP "application" form— regardless of whether these students or their parents had previously expressed an interest in attending secondary school. The application form consisted of 26 questions about characteristics that were highly correlated with the probability of school dropout, as indicated by analysis of a recent nationwide household survey; the questions were also reasonably easy for students of this age to answer, and for peers and teachers to validate. In practice, the form elicited information on household size and composition, parental education, the characteristics of the home (the material of roof and floors), availability of a toilet, running water, and electricity, and ownership of a number of household durables. Forms were filled out in school, on a single day. Students and parents were not told beforehand of the content of the forms, nor were they ever told the scoring formula—both decisions designed to minimize the possibility of strategic responses; for example, by a student seeking to maximize her chances of receiving the award. Once completed, forms were collected by head-teachers, and sent to the capital,

Phnom Penh. There, a firm contracted for this purpose "scored" them, using the responses and the set of weights that reflected how well each characteristic predicted the likelihood of school dropout in the nationwide household survey. The formula used was the same for every school and, once calculated, the scores could not be revised.[7]

Finally, within every CSP school, all applicants were ranked by the score, regardless of which feeder school they came from. In "large" CSP schools, with total enrollment above 200, 50 students with the lowest value of the score were then offered a scholarship for 7th, 8th, and 9th grade; in "small" CSP schools, with total enrollment below 200 students, 30 students with the lowest value of the score were offered the scholarship.[8] In total, just over 3800 scholarships were offered in the year of the program we study.[9] The list of students offered scholarships was then posted in each CSP school, as well as in the corresponding feeder schools.

Once children had been selected to receive a CSP scholarship, their families received the cash award three times a year. Payments were made at widely attended school ceremonies, with the school principal publicly handing over the cash to parents. The majority of participants at these school ceremonies were CSP recipients. During the ceremonies, principals stressed the importance of secondary school education, and the responsibilities that parents had to ensure that their children were enrolled in school, attended regularly, and were successful students. Also, parents were told that they were meant to spend the CSP award on the schooling of the selected children. Although no attempt was made to monitor

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[7] Scholarship recipients and their scores were posted at feeder schools and at CSP schools. There was a complaint mechanism whereby community members could appeal the decisions made on the basis of the score—either because they believed that an applicant had mis-represented their characteristics on the form, or because they believed an applicant was poorer (or less poor) than indicated by the score. In practice, however, less than 1 percent of applicants appealed the decisions, and the recipient status of even fewer was revised as a result of a complaint.

[8] In practice, within every large school, the 25 students with the lowest dropout score were offered a scholarship of $60, and the 25 students with the next lowest scores were offered a scholarship of $45; in small schools, the comparable numbers were 15 students with scholarships of $60, and 15 with scholarships of $45. We do not focus on this distinction in this paper. Rather, we compare applicants who were offered a scholarship, regardless of the amount, with others that were not. Because the identification strategy is regression-discontinuity, we are implicitly comparing applicants who were offered a $45 scholarship, with those who were offered no scholarship at all. Students who were offered a $60 scholarship help estimate the control function that relates enrollment to the dropout score.

[9] Occasionally, there were tied scores at the cut-off. In these cases, all applicants with the tied score at the cut-off were offered the scholarships.

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household expenditures, CSP recipients may have responded powerfully to these messages, perhaps especially so given a tradition of deference to authority in Cambodia.

We analyze the impact of the program among the first cohort of eligible children. These children filled out the application forms in May 2005, and the list of scholarship recipients was posted in November 2005. We use data on children at two points in time. First, we have access to the composite dropout score, as well as the individual characteristics that make up the score for all 26,537 scholarship applicants. Second, we fielded a household survey of 3453 randomly selected applicants and their families in five provinces: Battambang, Kampong Thom, Kratie, Prey Veng, and Takeo.[10] The household survey was collected between October and December of 2006, approximately 18 months after children filled out the application forms. Since application forms were filled out at the end of 6th grade, estimates of program effects on the school enrollment of applicants based on the household survey refer to the beginning of 8th grade.[11]

Table 1 summarizes the characteristics of CSP recipients and non-recipients, as reported on their application forms—separately for all applicants (left-hand panel) and applicants within ten ranks of the cut-off of the score (right-hand panel). The first four columns of each panel show that, as expected, recipients are generally poorer than non-recipients. For example, in the full sample, CSP recipients are less likely to own a bicycle (54 percent of recipients own one versus 76 percent of non-recipients); less likely to own a radio (25 versus 39 percent); and less likely to live in a dwelling whose roof is made of solid materials such as tiles, cement, concrete or iron (44 versus 65 percent). The differences between recipients and non-recipients are smaller when we limit the sample to children whose value of the score is closer to the cut-off.

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[10] The sample was based on randomly selected schools in these five provinces. The survey was limited to applicants ranked no more than 35 places above the cutoff in these schools. This restriction was imposed to maximize the number of schools, while maintaining the density of observations "around" the cut-off—an important consideration when estimating program effects based on regression-discontinuity, as discussed below.

[11] We also have access to a third set of data. These come from four unannounced visits to the 100 CSP schools (in February, April, and June 2006, and in June 2007) in which the physical attendance of applicants was verified. These allow us to validate our schooling impacts on recipients (as we discuss below), but do not allow an analysis of labor impacts nor sibling effects.

The final two columns in each panel of Table 1 report the coefficient and p-value in a regression of each characteristic on the application form on a quartic in the composite score, school fixed effects, and dummies for the age of the child and her birth order. This corresponds to our basic estimation specification, discussed in more detail below, and is a standard check on the validity of the regression discontinuity (RD) specification (Imbens and Lemieux 2008). This specification check suggests that differences between recipients and non-recipients are unlikely to be an important source of bias to our estimates of program impact. In the full sample, the coefficients on only two characteristics are significant—the probability that a child is a boy, and the fraction of households with floors made of wood planks or bamboo. We estimate the impact of the CSP program separately for boys and girls throughout, which removes any possible bias associated with differences in the gender composition of recipients and non-recipients. The difference in the proportion of households with floors made up of wood planks or bamboo is not significant when the sample is limited to children whose score places them within ten points of the cut-off. As a robustness check on our estimates of CSP program effects, we therefore also present results for this smaller sample.

In order to place our results in context, Table 2 summarizes enrollment and work outcomes for children in the control group, separately for applicants and their siblings, and by gender. We consider six different outcome variables. The first three are the probability of enrollment, working for pay, and working without pay. These are binary indicator variables—for example, enrollment takes on the value of one if a child is enrolled in school, and zero otherwise. The remaining variables correspond to the number of hours an applicant or their sibling attended school, worked for pay, and worked without pay, conditional on the relevant binary variable taking on a value of one. (For example, hours in school refer only to children who are enrolled in school.) The measures of hours of school attendance and work refer to the last seven days.[12]

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[12] The definitions of these variables follow the questionnaire on which the data are based. Work for pay is defined as "work for pay on a farm, public or private sector, or in a business belonging to someone else." Work for no pay is defined as "work for no pay on a farm, private or public sector, own account or in a business belonging to yourself or someone else in your household."

The table shows that enrollment of boys is higher than that of girls: in this sample of applicants who were not offered a scholarship 63 percent of boys and 54 percent of girls are enrolled. Among their siblings, who are on average younger, overall school enrollment is higher—86 percent for boys and 80 percent for girls. Applicants who are enrolled in school attend, on average, for about 26 hours per week, and their siblings attend for approximately 21 hours.

About 31 percent of applicant boys in the control group worked for pay, compared to 37 percent of girls: Among those who work for pay, average hours are 24 for boys and 28 for girls. Siblings are much less likely to work for pay—9 percent of boys and 17 percent work of girls work. Work for pay among children in this age group is concentrated in the farm sector and construction for boys, and in the farm sector and garment industry for girls.[13]

Work without pay is much more widespread among applicants and their siblings: 64 percent of applicant boys and 51 percent of applicant girls work without pay. On average, these children work for about 19 hours. Among siblings, the incidence of work for pay is once again lower—52 percent among boys, and 47 percent among girls.

The last two columns of the table focus on patterns of work among applicants' parents. Many more adults work in the no-pay sector than in the for-pay sector, a pattern that is apparent for both men and women. Work hours are approximately 30 hours per week in the for-pay sector, and 32 to 36 hours in the no-pay sector.

4.   Identification strategy

The basic identification strategy we use in this paper is based on regression discontinuity (RD). The regressions we estimate take the following form:

(14) $Y_{ihs} = \alpha_s + f(S_h) + \mathbf{X}_i\beta + (R*Male)\delta_1 + (R*Female)\delta_2 + (R*S*Male)\delta_3 + (R*S*Female)\delta_4 + \varepsilon_{ihs}$

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[13] Our survey did not collect information on what work for pay children are engaged in, so we make use of a recent nationwide household survey, the 2004 Cambodia Socio-Economic Survey (CSES). We limit the sample in the CSES to rural areas, which most closely corresponds to the catchment areas of the CSP schools. In rural areas, 35 percent of boys age 10-18 who work for pay are farm workers, and another 26 percent work in construction; among girls, 35 percent of those who work for pay are farm workers, and 27 percent work in the garment industry.

where Y_{ihs} is an outcome variable, for example, the probability that child i in household h and CSP school s is enrolled in school; α_s is a set of CSP school fixed effects; $f(S_h)$ is the control function, a flexible parametrization of the dropout score. In our main results, we use a quartic in the score and allow the function to differ for males and females; we also test for the robustness of the results to this choice of functional form. $\mathbf{X}_i$ includes a set of single year age dummies and a set of birth order dummies. $\mathbf{X}_i$ also includes dummy for males, a dummy for siblings, and the interaction between siblings and males. The variables R*Male (R*Female) take on the value of one if the observation is a male (female) applicant who was offered a scholarship; the variables R*S*Male (R*S*Female) take on the value of one for male (female) siblings of applicants who were offered a scholarship; and ε_{ihs} is the regression error term. All regressions are limited to school-aged children, ages 7-18. Standard errors account for clustering at the level of the primary feeder school.

In this set-up, the parameters δ_1 and δ_2 are estimates of the program impact on male and female recipients, respectively, while the parameters δ_3 and δ_4 are estimates of the program impact on the male and female siblings of recipients, respectively. Note that because we include the main effects for boys and siblings in the vector $\mathbf{X}_i$, as well as the interaction terms between them, we are comparing treated applicants to control applicants (and not to their siblings), and treated siblings to control siblings (and not to applicants).

Three things are worth noting about this specification. First, because the score perfectly predicts whether or not an applicant is offered a scholarship, this is a case of sharp (as opposed to fuzzy) RD. Second, because we focus on the impact of being *offered* a scholarship, rather than that of actually *taking up* a scholarship, these are Intent-to-Treat (ITT) estimates of program impact. Third, as with every approach based on RD, the estimated effect is "local". Specifically, it is an estimate of the impact of the scholarship program around the cut-off. However, where the cut-off falls in terms of the dropout score varies from school to school. This is because the number of students offered the scholarship was the same in every large and small CSP school, respectively, but both the number of 6th graders and the distribution

of the underlying characteristics that make up the dropout score varied.[14] In practice, the value of the cut-off varies from a score of 21 to 40 in the schools attended by the study sample, with the median at 28. The estimates of δ are therefore weighted averages of the impacts for these different cut-off values.

For the three indicator variables (the probability of enrollment, of working for pay, and working without pay) the models are estimated by OLS. For the other variables, the hours spent in each of these activities in the past 7 days, we present results both from OLS regressions and the marginal effects from Tobit specifications; the latter take account of the fact that the variables are censored, with a substantial fraction of the sample reporting zero.[15]

5.   Results

5.1   Main results

Before turning to the estimates of equation (14) we motivate our results by showing outcomes as a function of the ranking based on the dropout score, relative to the cut-off. We do this by plotting average outcomes at each value of the relative ranking, and overlaying a quartic in the score.[16,17] Figure 3 has six panels, corresponding to enrollment, work for pay, work without pay for applicants and their siblings. In each case, distinct "jumps" at the cut-off would suggest that the program affected behavior.

For applicants, panel A suggests that the program had large effects on enrollment, approximately 20 percentage points; panel B suggests that the probability of work for pay dropped; and panel C suggests

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[14] All else being equal, in CSP schools that received more applications, and in those in which children have characteristics that make it more likely they will drop out, a child with a high dropout score is more likely to be turned down for a scholarship than a similar child applying to a school that receives fewer applications or serves a population with a lower average dropout score.

[15] See Black, Galdo and Smith (2007) for an application of the Tobit model in an RD framework.

[16] Because the cut-off falls at different values of the underlying score in different schools, depending on the number of applications, the mean characteristics of applicants, and whether a school was defined as "large" or "small", it is not informative to graph outcomes as a function of the score. Rather, for these figures we redefine an applicant's score in terms of the distance to the school-specific cut-off, so that (for example), a value of -1 represents the "next-to-last" applicant to be offered a scholarship within a school, 0 the "last", and a value of +1 represents the "first" applicant within a school who was turned down. The figures then graph outcomes as a function of this relative rank.

[17] These parametric regressions include a quartic in the relative rank, but not the vector of school fixed effects or child characteristics. Note that these differ slightly from the models estimated below which control for the composite score, CSP school fixed effects, and age and birth order dummies. We note that using locally-weighted least squares regressions (as in Fan and Gijbels 1996) instead of a quartic produces almost identical results.

that the program led to a small increase in the likelihood that children engaged in unpaid work. For

siblings, panels A and B suggest little relationship between scholarships and enrollment or work for pay;

panel C suggests a small increase in work without pay. Figure 1 is thus consistent with the CSP program

having had a large effect on the schooling of children who were offered scholarships, but little or no

effect on their siblings.

The results of parametric estimates of program impact, described in equation (1) above, are

reported in Table 3. For each outcome, we show estimates of program impact on males and female

recipients, as well as on male and female siblings. We also test for differences in the recipient effects by

gender ($\delta_1 = \delta_2$), in the sibling effects by gender ($\delta_3 = \delta_4$) and whether the gender-specific recipient and

sibling effects are the same [($\delta_1 = \delta_3$), and ($\delta_2 = \delta_4$)].

The first two rows of the table confirm that the program had large effects on recipients. School

enrollment increased dramatically—by 22 percentage points for boys and 20 percentage points for girls.

This increase came hand in hand with a sharp reduction in the probability that CSP recipients work for

pay—of 12 percentage points in the case of boys, and 9 percentage points in the case of girls. Finally,

applicants also more likely to work without pay, a result that is significant for girls. Paid work may be

more difficult to combine with schooling than unpaid work, because paid work generally involves less

flexible hours and a greater intensity of work (as suggested by Edmonds 2007; Edmonds and Schady

2008). The results in Table 3 are strongly consistent with this pattern.

Before discussing the effects on siblings, we consider how the CSP program affected the hours

spent on each of these activities. These results are presented in Table 4. The left-hand panel of the table

presents the marginal effects from Tobit regressions, and the right-hand panel presents corresponding

results estimated by OLS. The coefficients on hours of schooling suggest that recipients spent 6-8 more

hours in school than non-recipients. The reduction in hours worked for pay is smaller—between 1 and 3

hours. Note that the estimated effects on hours worked without pay are negative—ranging from a

reduction in 30 minutes to a reduction of almost an hour and three quarters. So while the program effect on the incidence of work without pay is positive (Table 3), recipients worked fewer hours (Table 4).[18]

We next turn to a discussion of CSP program effects on the siblings of applicants, focusing on both changes in participation (in Table 3) and hours (Table 4). Table 3 suggests that siblings of CSP recipients increased the likelihood of work without pay by between 4 and 7 percentage points. However, Table 4 shows that the implied change in hours is very small, and is not significant. Both tables also make clear that the school enrollment choices of siblings were unaffected by the program.

5.2   Robustness checks

We conducted a large number of robustness checks to our main results. These include specifications that limit the sample to children with a score that places them within 10 ranks of the school-specific cut-off; specifications that allow for school-specific control functions (in addition to the school-specific intercepts); specifications in which the control function is defined in terms of an applicant's ranking relative to the school-specific cut-off (as in Figure 3), rather than in terms of the score; and specifications that separately consider program effects on older and younger siblings. None of these changes has a qualitatively important effect on our basic results.

A.   *Sample restricted to households within 10 ranks of cut-off*: A standard check on the RD specification involves testing whether the estimated coefficients are robust to limiting the sample to observations that are "close" to the cut-off. We do this by restricting the sample to children in households with a score that places them no further than 10 ranks from the school-specific cut-off. This comes at a cost—our sample is reduced by almost two-thirds (from 8182 observations to 2920).

The results from this robustness check are reported in the left-hand panel of Table 5. In terms of work, the coefficients in this smaller sample tend to be somewhat larger for boys. For example, among applicants, we estimate a reduction in work for pay of 18 (rather than 12) percentage points; among male siblings, we estimate an increase in work without pay of 7 (rather than 5) percentage points. Among girls,

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[18] We also carried out this analysis for an additional activity; household chores. We found small and statistically insignificant program effects on time spent in household chores. These results are available from the authors on request.

the only notable change is that the coefficient on work for pay for applicants is reduced substantially, from 9 to 5 percentage points, and is no longer significant. In terms of schooling, the results for the smaller sample are extremely close to those estimated for the full sample of children. Because the results for the smaller sample are very similar to those that use the full sample of children, we conclude that our main set of results is not driven by possible biases introduced by using observations that are "far" from the cut-off.

B.   *School-specific control function*: Although our basic specification allows for school-specific intercepts, it imposes a common control function across schools. This assumes that a given change in household socioeconomic status (as measured by the composite score) is associated with an increase in the probability of enrollment or work of the same magnitude across all schools. Conceivably, such an assumption of equal control functions may not do justice to the data. For example, there may be differences in school quality which affect not only whether school enrollment is higher in some schools than in others at all levels of socioeconomic status (a difference in *intercepts* across schools), but also the gradients between socioeconomic status and enrollment (a difference in *slopes* across schools).

The right-hand side of Table 5 reports results from specifications that allow for school-specific quartic trends and intercepts. This places large demands on the data—for each school, there are two intercepts (for boys and girls), and eight polynomials in the score (quartics for boys and girls), for a total of 570 terms. The right-hand panel of Table 5 shows, however, that the results from this more flexible formulation are very close to those that impose a common control function. For example, in this specification the CSP program effect on the probability that applicant boys are enrolled in school implies an increase of 19 percentage points (compared to 22 points in the specification that imposes a common control function), while that for girls implies an increase of 21 percentage points (compared to 20 points); in terms of work for pay, the coefficients in 5 imply a reduction of 12 percentage points for boys, and 7 points for girls, compared to 12 percentage for boys and 9 points for girls in Table 3. Sibling effects remain small and insignificant. It does not appear that the assumption of a common control function across schools introduces substantial biases into our estimates of the impact of the CSP program.

C.	*Defining control function in terms of ranking, rather than the score*: In our main set of results, the control function is defined in terms of the score on the application form, rather than in terms of an applicant's ranking relative to the school-specific cut-off. In principle, this could introduce biases as recipients in some schools are compared to non-recipients with the same score in a different school. (Although one would expect the potential biases to be small, especially in specifications that include school-specific intercepts and slopes.) An alternative is to define the control function in terms of an applicant's ranking relative to the school-specific cut-off. Table 6 reports the results from specifications that are based on an applicant's rank, with a common control function (left-had panel) and school-specific control functions (right-hand panel). In these specifications, the impact of the program on school enrollment among applicants is a little smaller—about 18 (rather than 21) percentage points. In terms of work, the impacts among boys appear to be somewhat larger than those in our main set of results—for example, in the specification that allows for school-specific control functions the impact on work for pay is 14 (rather than 12) percentage points; for girls, the estimated effects are somewhat smaller in terms of the reduction in work for pay (6, rather than 9 percentage points), but larger in terms of the increase in work without pay (14, rather than 7 percentage points). Among siblings, the impacts are again estimated to be insignificant. One coefficient emerges as significant in the single control function specification: male siblings who are recipients increase their work without pay. However, given that this result does not feature in any other specifications, we do not think that it undermines the overall finding of lack of impact on siblings. In general, therefore, the patterns of results in specifications that are based on rank are similar to those that are based on an applicant's score.

D.	*Sibling-effects differentiated by relative age*: First-born, or earlier-born, siblings have typically been found to be less likely to attend school.[19] We investigate the extent to which our results could mask heterogeneity by the relative age of siblings. In order to isolate the issue of relative age, we re-estimate our basic model but now allow the impacts to differ by whether a sibling is younger or older than the

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[19] This has been documented in settings as diverse as Brazil (Emerson and Souza 2008), Nepal (Edmonds 2006), and Taiwan (Parish and Willis 1993). Edmonds (2007) provides a thoughtful review.

applicant. (We do not differentiate by gender to keep sample sizes reasonable; however, results that disaggregate by the gender of both the applicant and her sibling are similar to those we report, but substantially less precise.) Table 7 shows that our results are not an artifact of aggregation: sibling effects are not significantly (or substantively) different depending on the relative age of the sibling.[20]

E.   *School visits*: A final possible concern is the possibility of systematic reporting biases in our measure of school enrollment based on household survey data. Conceivably, parents of scholarship recipients could be more likely to lie to enumerators about school enrollment than those of non-recipients (although it is less clear why they would lie about the school enrollment and work status of ineligible siblings). As we report elsewhere, however, results from an analysis of data on directly observed school attendance from four unannounced school visits are very similar to those that use the household survey (Filmer and Schady 2009c).[21]

5.3.   Magnitude of program effects

The CSP program effects on the enrollment of eligible children are large. One way of placing the magnitude of the effects in context is by calculating the elasticity of enrollment with respect to cost. To do this, we calculated the total (direct and indirect) cost of schooling for children affected by the CSP program, using data we collected in the survey, and limiting the sample to children who applied for a scholarship but did not receive one.

The direct cost is given by the sum of various school fees, including annual fees (which include exam fees, various "allowances" and fees for various school events and ceremonies); fees incurred at the beginning of the school year (including registration fees, uniforms, books, and school material); and daily

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[20] We also explored whether restricting the analysis to siblings who are close in age to the applicant alters our findings. For this purpose, we estimated our basic model, but restricted the sample to children (both applicants and siblings) ages 14 to 18. Results from these estimates are very similar to those we report in the paper—that is, strong own effects on schooling and work for pay but small and insignificant impacts on siblings. Finally, we restricted the sample to male applicants only and analyzed the effect on brothers, and to female applicants only and analyzed the effects on sisters—on the grounds that same-sex siblings might be closer substitutes for each other. As with the age restriction, we do not find that restricting the sample in these ways changes our findings. These results are not included in the paper but are available from the authors upon request.

[21] The impact of being offered a scholarship on physically verified attendance is equal to 25 percentage points when pooling across all four visits (February/March 2006; April/May 2006; June 2006; June 2007), and equal to 20 percentage points when restricting the analysis to June 2007 when the applicants would have been in 8th grade, if they did not repeat school grades.

expenditures (including snacks, extra classes, bicycle parking, lesson copies and other daily expenses). The sum of these expenses is US $44 per year per child, on average; of these, the bulk is made up of fees incurred at the beginning of the school year (34 percent of the total), and daily expenditures (63 percent of the total), while other annual fees are a very small amount (2 percent of the total). The indirect cost of the CSP is given by the foregone earnings. In Table 4 above, and focusing on the Tobit marginal effects, we show that the average recipient reduced work for pay by approximately 1.2 hours per week; separate calculations using the survey data show that the mean hourly wage in the for-pay sector for applicant children who did not receive scholarships is US $0.38. Assuming that the school year has 8 months, this amounts to $14 of foregone earnings per year. Total costs (including direct and opportunity costs) are therefore $58, while the transfer is $45. The CSP program represented a 78 percent (45/58) reduction in costs. This, in turn, resulted in an increase in school enrollment of approximately 21 percentage points, from a baseline value of approximately 55 percent, for an increase in enrollment of 37 percent. The elasticity of enrollment with respect to cost is therefore 0.48 (37/78).[22] (If we include the reduction in hours worked without pay, which is not significant, and value it at the wage paid to children working for pay, the elasticity is similar, 0.51.) Although we are not aware of estimates from other settings we could use for comparison, this appears to be a reasonable value for this elasticity.

6.   Discussion and conclusion

Cash transfer programs, both conditional and unconditional, have become very popular in the developing world. In many countries, they have become the largest social assistance program, covering millions of households (as is the case in Brazil, Mexico, Ecuador, and South Africa). Many of these programs also seek to increase the educational attainment of children. However, because enrollment rates are already very high at some school grades, some analysts have suggested that cash transfers (in particular, those which are conditioned on school enrollment) could be made more efficient if they were

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[22] These calculations are weighted averages of the means and impacts for boys and girls, respectively. Similar calculations using the OLS regression coefficients rather than the Tobit marginal effects yield an elasticity of 0.69, which is larger because of the larger estimated effect on hours worked for pay in the OLS specifications.

narrowly targeted at child ages and grades where, in the absence of the program, dropout rates are high (de Janvry and Sadoulet 2006).

In this paper, we construct a simple model of schooling decisions and how these respond to a child-specific CCT. We show that such a program will unambiguously increase school enrollment among eligible children, but will have an ambiguous effect on the school enrollment of their ineligible siblings. We then take the predictions of the model to data from a child-specific CCT in Cambodia. This analysis shows that the program significantly increased the school enrollment of eligible children, but left schooling outcomes for their siblings unaffected.

However, this need not have been so. Barrera-Osorio et al. (2008) analyze a program in Bogotá, Colombia. This program makes reasonably large transfers, equivalent to about 8 percent of expenditures for the median recipient household, conditional on the school enrollment of specific children selected for the program.[23] Barrera-Osorio et al. compare families in which two, one, or no children were selected into the program. They conclude that the program positively affected the school enrollment of recipients, but that this came, in part, at the expense of their siblings, who were more likely to drop out of school and enter the labor market. Similarly, Manacorda (2006) uses historical data from the United States to show that minimum working age laws that enabled a child of a particular age to join the labor market legally led to a reduction in their siblings' labor participation and an increase in their siblings' school participation.

Our simple model of two-child households can account for the results for Cambodia, Colombia, and the early-twentieth century United States. The model suggests that households will fall into one of three types. First, "poor" households who would send neither child to school in the absence of the program. If these households take up the program, they will enroll the eligible child, and perhaps the ineligible child if the transfer is large enough. Second, "middle-income" households who would send only one child to school in the absence of the program. If these households take up the program, they will

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[23] The amount of the transfer and its value as a share of expenditures for the average recipient household are not reported in the paper. We are grateful to Felipe Barrera for providing us with this detail.

ensure that the eligible child attends school, but this *could* come hand in hand with a displacement effect on their ineligible sibling. Third, "wealthier" households who would send both children to school, even without the program. We would expect no impact on enrollment in this case.

In any given setting there will presumably be some households of all three types. The overall impact of the program therefore represents the average across types—and will be weighted by the population share of each type. The case of Colombia (Barrera-Osorio et al. 2008) suggests a situation dominated by the second type of household—the direct impact on recipients was accompanied by a reduction in schooling among their siblings; the results reported by Manacorda (2006) for the early-twentieth century United States are also consistent with this. In Cambodia, however, we find no evidence of such spillovers to siblings—suggesting a situation in which most households are of the first type.

More generally, our results suggest that it is premature to conclude that cash transfer programs that are directed at individual children will always affect siblings (positively or negatively). Rather, these spillover effects are likely to depend on the details of the program, the age- grade-, and gender-specific patterns of school enrollment, and the opportunities available to children outside school. Understanding these differences across settings and programs should be a priority for future research.

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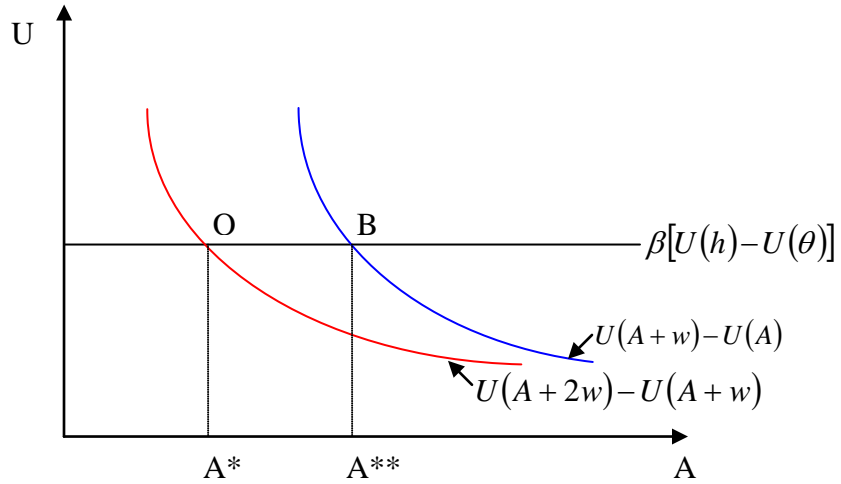**Figure 1: Enrollment decisions and Adult Income in the Basic Model**
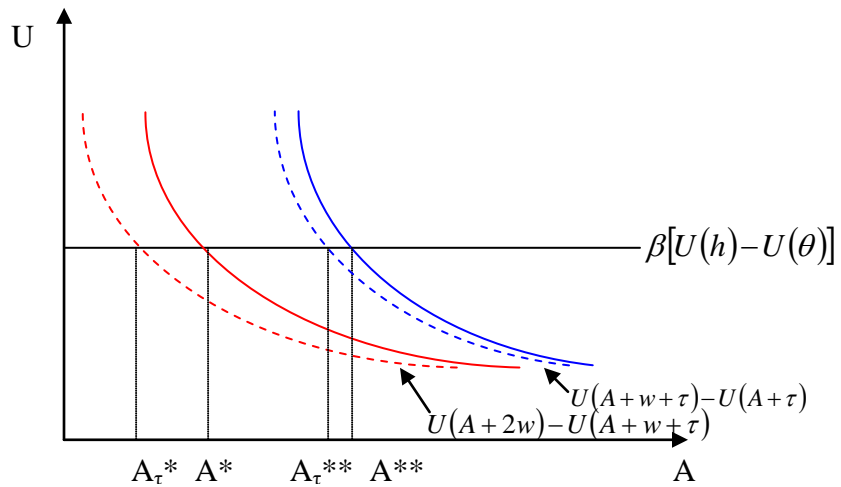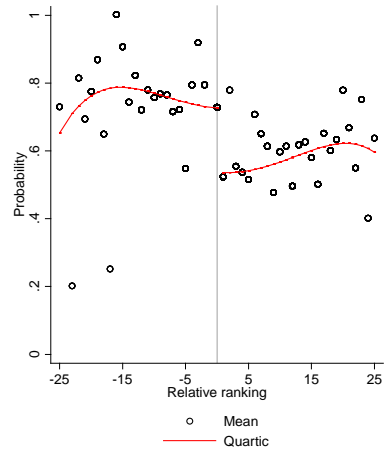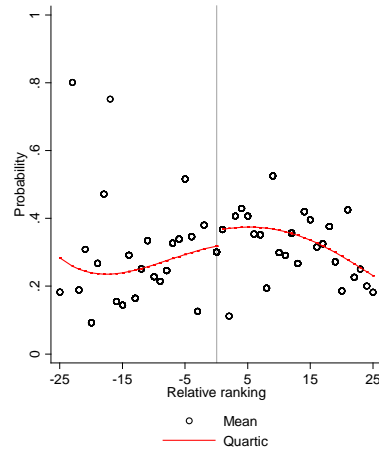

Figure 2: Introducing a Conditional Cash Transfer
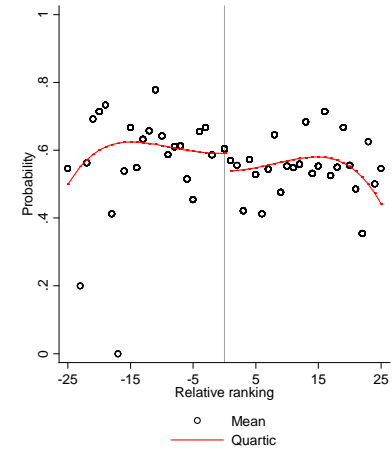
Figure 3: Program effects on applicants and their siblings

A. Enrollment   B. Work for pay   C. Work for no pay
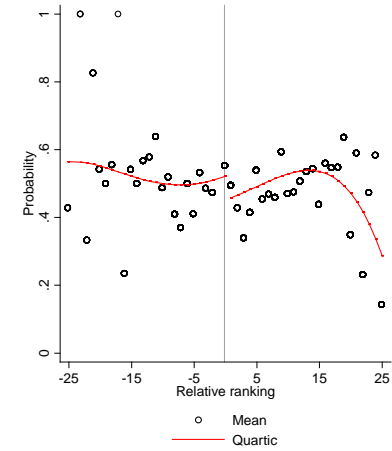
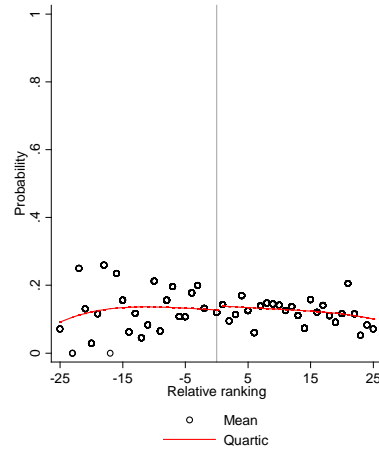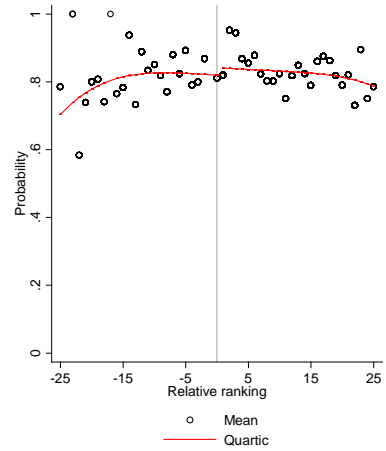**Applicants**

Siblings

30

Table 1: Characteristics of CSP recipients and non-recipient applicants

	Overall						Within 10 ranks of cutoff					
	Non recip-ients	Recip-ients	Diff.	P-value	Dummy RD Coef.	Dummy RD P-value	Non recip-ients	Recip-ients	Diff.	P-value	Dummy RD Coef.	Dummy RD P-value
Male	0.392	0.234	-0.157	0.000**	-0.057	0.025*	0.389	0.305	-0.084	0.003**	-0.062	0.156
Live with mother	0.861	0.774	-0.086	0.000**	0.023	0.248	0.851	0.844	-0.007	0.752	0.013	0.648
Mother attended school	0.501	0.363	-0.138	0.000**	0.050	0.073	0.481	0.477	-0.004	0.898	0.057	0.201
Live with father	0.683	0.526	-0.157	0.000**	0.002	0.944	0.627	0.624	-0.003	0.935	0.039	0.398
Father attended school	0.578	0.413	-0.165	0.000**	0.015	0.554	0.569	0.494	-0.075	0.014*	0.049	0.258
Parent is civil servant	0.051	0.023	-0.029	0.000**	-0.007	0.487	0.046	0.026	-0.020	0.181	-0.015	0.411
Number of other children in hh	1.302	1.334	0.033	0.445	0.097	0.146	1.378	1.398	0.020	0.790	0.040	0.749
Number of adults in household	2.963	2.726	-0.236	0.000**	0.116	0.125	2.878	2.902	0.024	0.811	0.107	0.373
Disabled household member	0.164	0.197	0.033	0.030*	-0.025	0.291	0.193	0.157	-0.036	0.140	-0.052	0.112
Own bicycle	0.754	0.545	-0.209	0.000**	0.002	0.922	0.720	0.672	-0.049	0.071	0.003	0.938
Own ox/horses cart	0.371	0.255	-0.116	0.000**	-0.018	0.474	0.354	0.307	-0.047	0.107	0.063	0.073
Own motorbike	0.114	0.034	-0.079	0.000**	0.005	0.677	0.080	0.063	-0.018	0.240	0.007	0.757
Own car or truck	0.018	0.001	-0.016	0.001**	0.003	0.500	0.016	0.003	-0.013	0.059	0.000	0.964
Own radio	0.392	0.255	-0.137	0.000**	-0.048	0.081	0.373	0.305	-0.068	0.018*	-0.024	0.621
Own TV	0.386	0.144	-0.242	0.000**	-0.007	0.789	0.346	0.247	-0.099	0.001	-0.068	0.110
Roof made of solid materials	0.656	0.445	-0.211	0.000**	-0.034	0.205	0.629	0.575	-0.054	0.089	-0.048	0.282
Floors: Polished wood/Tiles	0.014	0.010	-0.003	0.370	0.007	0.200	0.005	0.016	0.011	0.040*	0.015	0.118
Floors: wood planks or bamboo	0.943	0.900	-0.043	0.000**	-0.046	0.001**	0.952	0.924	-0.028	0.060	-0.013	0.603
Drinking water: piped into house	0.003	0.001	-0.002	0.136	-0.002	0.220	0.000	0.000	0.000		0.000	
Drinking water: well/pump	0.793	0.766	-0.028	0.085	-0.033	0.074	0.780	0.753	-0.027	0.309	-0.019	0.554
Drinking water: vendor purchased	0.015	0.014	-0.001	0.800	0.007	0.250	0.014	0.023	0.010	0.189	0.012	0.221
Toilet: Flush	0.031	0.005	-0.027	0.010**	0.008	0.098	0.027	0.009	-0.019	0.202	0.006	0.618
Toilet: Pit latrine	0.077	0.051	-0.026	0.004**	0.014	0.203	0.055	0.071	0.016	0.248	0.033	0.108
Lighting: Electricity from a generator	0.012	0.002	-0.010	0.197	0.002	0.547	0.005	0.001	-0.003	0.515	-0.002	0.712
Lighting: Electricity from a battery	0.518	0.317	-0.201	0.000**	-0.005	0.850	0.483	0.419	-0.064	0.041	-0.032	0.461
Cooking fuel electricity,gas,kerosene	0.003	0.000	-0.003	0.102	0.000	0.810	0.000	0.001	0.001	0.314	0.004	0.318

Note: Information based on application forms for sample of applicants covered by household survey. ** significant at the 1 percent level, * at the 5 percent level.

Table 2: Summary statistics on enrollment and work of non recipient applicants and their siblings

	Males 7-18		Female 7-18		Parents	
	Non recipient applicants	Siblings of non recipient applicants	Non recipient applicants	Fathers of non-recipient applicants	Fathers of non-recipient applicants	Mothers of non-recipient applicants
Enrolled	0.628	0.860	0.544	0.804	-	-
	(0.484)	(0.347)	(0.498)	(0.397)	-	-
Enrolled, hours	25.96	21.34	26.12	21.50	-	-
	(9.36)	(7.19)	(9.27)	(6.70)	-	-
Work for pay	0.309	0.093	0.366	0.167	0.285	0.218
	(0.463)	(0.290)	(0.482)	(0.373)	(0.452)	(0.413)
Work for pay, hours	24.03	21.96	27.76	24.61	30.19	24.03
	(23.04)	(23.86)	(26.52)	(24.90)	(23.07)	(23.04)
Work for no pay	0.637	0.517	0.513	0.466	0.778	0.742
	(0.481)	(0.500)	(0.500)	(0.499)	(0.416)	(0.437)
Work for no pay, hours	19.13	18.18	19.56	16.83	35.62	32.03
	(14.53)	(13.19)	(14.76)	(12.77)	(18.90)	(18.47)

Note: Standard deviations in parentheses

Table 3: Program effects on recipients and siblings, by gender

	School enrollment	Work for pay	Work without pay
Own effect*male	0.215**	-0.120**	0.043
	(0.031)	(0.032)	(0.037)
Own effect*female	0.200**	-0.088**	0.074*
	(0.026)	(0.025)	(0.029)
Sibling effect*male	0.011	-0.007	0.046
	(0.019)	(0.020)	(0.030)
Sibling effect*female	-0.000	-0.034	0.028
	(0.019)	(0.021)	(0.028)
R-squared	0.31	0.21	0.11
P-value: Own(M)=Own(F)	0.70	0.42	0.48
P-value: Sib(M)=Sib(F)	0.60	0.28	0.63
P-value: Own(M)=Sib(M)	0.00	0.00	0.95
P-value: Own(F)=Sib(F)	0.00	0.02	0.10

Note: Sample size is 8182 in all regressions. Sample includes all children ages 7 to 18. All specifications include a set of school dummies, a set of single year age dummies, a set of birth order dummies, a dummy for the gender of the child, dummy variables for sibling*gender, and a quartic in the score. Standard errors adjust for clustering at the applicant primary-school level. ** significant at the 1 percent level, * at the 5 percent level. P-values are from an F-test of equality of parameter estimates.

Table 4: Program effects on recipients and siblings, by gender

	Tobit (Marginal Effects)			OLS		
	Hours of schooling	Hours worked for pay	Hours worked without pay	Hours of schooling	Hours worked for pay	Hours worked without pay
Own effect*male	7.667**	-1.213*	-0.480	6.130**	-3.225**	-1.552
	(1.193)	(0.370)	(0.845)	(0.945)	(1.091)	(1.052)
Own effect*female	8.275**	-1.162*	-0.339	6.626**	-3.297**	-1.690*
	(0.993)	(0.325)	(0.693)	(0.783)	(1.035)	(0.787)
Sibling effect*male	1.104	0.010	0.674	1.022*	-0.848	0.276
	(0.686)	(0.603)	(0.790)	(0.595)	(0.671)	(0.825)
Sibling effect*female	0.158	-0.546	0.174	0.209	-0.825	-0.125
	(0.683)	(0.424)	(0.702)	(0.596)	(0.712)	(0.731)
R-squared				0.21	0.12	0.10
P-value: Own(M)=Own(F)	0.61	0.79	0.89	0.66	0.96	0.91
P-value: Sib(M)=Sib(F)	0.24	0.39	0.61	0.24	0.98	0.71
P-value: Own(M)=Sib(M)	0.00	0.01	0.16	0.00	0.02	0.06
P-value: Own(F)=Sib(F)	0.00	0.08	0.45	0.00	0.01	0.04

Note: Sample size is 8182 in all regressions. Sample includes all children ages 7 to 18. All specifications include a set of school dummies, a set of single year age dummies, a set of birth order dummies, a dummy for the gender of the child, dummy variables for sibling*gender, and a quartic in the score. Standard errors adjust for clustering at the applicant primary-school level. ** significant at the 1 percent level, * at the 5 percent level. P-values are from an F-test of equality of parameter estimates.

Table 5: Program effects on recipients and siblings, by gender – alternative estimation approaches

	Restricted to within 10 ranks of cutoff			Control function is school-specific function		
	Enrolled	Worked for pay	Worked without pay	Enrolled	Worked for pay	Worked without pay
Own effect*male	0.215**	-0.175**	0.098	0.188**	-0.120**	0.036
	(0.056)	(0.052)	(0.055)	(0.040)	(0.040)	(0.047)
Own effect*female	0.192**	-0.046	0.074	0.209**	-0.070*	0.123**
	(0.039)	(0.039)	(0.047)	(0.034)	(0.035)	(0.040)
Sibling effect*male	0.001	-0.014	0.078	-0.017	-0.006	0.045
	(0.029)	(0.029)	(0.044)	(0.028)	(0.029)	(0.041)
Sibling effect*female	-0.018	-0.049	0.052	0.000	-0.010	0.070
	(0.028)	(0.031)	(0.046)	(0.028)	(0.031)	(0.040)
R-squared	0.31	0.23	0.14	0.35	0.27	0.19
P-value: Own(M)=Own(F)	0.71	0.02	0.71	0.68	0.33	0.15
P-value: Sib(M)=Sib(F)	0.54	0.30	0.60	0.64	0.93	0.66
P-value: Own(M)=Sib(M)	0.00	0.00	0.73	0.00	0.00	0.79
P-value: Own(F)=Sib(F)	0.00	0.92	0.65	0.00	0.01	0.07

Note: Sample size is 2920 for the sample of children in households within 10 ranks of cutoff, and 8182 for the full sample. All specifications include a set of school dummies, a set of single year age dummies, a set of birth order dummies, a dummy for the gender of the child, dummy variables for sibling*gender, and a quartic in the control function. Standard errors adjust for clustering at the applicant primary-school level. ** significant at the 1 percent level, * at the 5 percent level. P-values are from an F-test of equality of parameter estimates.

Table 6: Program effects on recipients and siblings, by gender–defining control function as within-school ranking

	Control function is function of within-school ranking			Control function is school-specific function of within school ranking		
	Enrolled	Worked for pay	Worked without pay	Enrolled	Worked for pay	Worked without pay
Own effect*male	0.187**	-0.142**	0.114*	0.181**	-0.120*	0.075
	(0.040)	(0.045)	(0.053)	(0.045)	(0.047)	(0.054)
Own effect*female	0.175**	-0.047	0.112*	0.188**	-0.058	0.135**
	(0.037)	(0.036)	(0.047)	(0.039)	(0.039)	(0.048)
Sibling effect*male	-0.018	-0.030	0.120*	-0.021	-0.007	0.093
	(0.032)	(0.036)	(0.049)	(0.035)	(0.037)	(0.050)
Sibling effect*female	-0.027	0.008	0.064	-0.020	0.003	0.083
	(0.032)	(0.032)	(0.046)	(0.033)	(0.035)	(0.048)
R-squared	0.31	0.21	0.12	0.35	0.27	0.19
P-value: Own(M)=Own(F)	0.83	0.08	0.98	0.90	0.29	0.40
P-value: Sib(M)=Sib(F)	0.84	0.37	0.38	0.98	0.83	0.89
P-value: Own(M)=Sib(M)	0.00	0.00	0.85	0.00	0.00	0.63
P-value: Own(F)=Sib(F)	0.00	0.02	0.09	0.00	0.01	0.08

Note: Sample size is 8182 in all regressions. Sample includes all children ages 7 to 18. All specifications include a set of school dummies, a set of single year age dummies, a set of birth order dummies, a dummy for the gender of the child, dummy variables for sibling*gender, and a quartic in the control function. Standard errors adjust for clustering at the applicant primary-school level. ** significant at the 1 percent level, * at the 5 percent level. P-values are from an F-test of equality of parameter estimates.

Table 7: Program effects on recipients and siblings, by age relative to applicant's age

	School enrollment	Work for pay	Work without pay
Own effect	0.198**	-0.092**	0.059*
	(0.020)	(0.020)	(0.024)
Sibling effect*younger	0.016	-0.028	0.029
	(0.016)	(0.016)	(0.025)
Sibling effect*older	-0.025	0.018	0.069
	(0.032)	(0.038)	(0.037)
R-squared	0.32	0.21	0.11
P-value: Sib(Y)=Sib(O)	0.21	0.21	0.30

Note: sample size is 8182 in all regressions. All specifications include a set of school dummies, a set of single year age dummies, a set of birth order dummies, a dummy for the gender of the child, dummy variables for sibling*gender, and a quartic in the score. Standard errors adjust for clustering at the applicant primary-school level. ** significant at the 1 percent level, * at the 5 percent level. P-values are from an F-test of equality of parameter estimates.

Appendix 1: Proof of Proposition 1.

Consider first the conditions under which both children would be enrolled. $\sigma_1 = \sigma_2 = 1$ if and only if:

$$U(A) + 2\beta U(h) \geq U(A+w) + \beta[U(h) + U(\theta)] \tag{A1}$$

and
$$U(A) + 2\beta U(h) \geq U(A+2w) + 2\beta U(\theta) \tag{A2}$$

(A1) implies $\quad U(A+w) - U(A) \leq \beta[U(h) - U(\theta)]$ $\hfill$ (A3)

(A2) implies $\quad U(A+2w) - U(A) \leq 2\beta[U(h) - U(\theta)]$ $\hfill$ (A4)

Concavity of U(.) implies that $2[U(A+w) - U(A)] > U(A+2w) - U(A)$, so (A4) is always implied by (A3). (A3) is the necessary and sufficient condition for $\sigma_1 = \sigma_2 = 1$. Given the Inada conditions on U(.), $\exists A^{**}$ such that $U(A^{**}+w) - U(A^{**}) = \beta[U(h) - U(\theta)]$. Concavity again implies that $U(A+w) - U(A) > \beta[U(h) - U(\theta)]$, $\forall A < A^{**}$; and $U(A+w) - U(A) < \beta[U(h) - U(\theta)]$, $\forall A > A^{**}$. (At equality (A3) corresponds to point B in Figure 1.)

Now consider the conditions (if any) under which a single child would be enrolled. $\sigma_i = 1, \sigma_j = 0, i \neq j$, if and only if:

$$U(A+w) + \beta[U(h) + U(\theta)] \geq U(A) + 2\beta U(h) \tag{A5}$$

and
$$U(A+w) + \beta[U(h) + U(\theta)] \geq U(A+2w) + 2\beta U(\theta) \tag{A6}$$

(A5) is just the converse of (A1), and implies the converse of (A3). (A6) implies:
$$U(A+2w) - U(A+w) \leq \beta[U(h) - U(\theta)] \tag{A7}$$

The Inada conditions imply that $\exists A^*$ such that $U(A^*+2w) - U(A^*+w) = \beta[U(h) - U(\theta)]$. Concavity of U(.) implies that $U(A+2w) - U(A+w) < \beta[U(h) - U(\theta)]$, $\forall A > A^*$, and $U(A+2w) - U(A+w) > \beta[U(h) - U(\theta)]$, $\forall A < A^*$. (At equality, (A7) corresponds to point O in Figure 1.)

It follows that
(i) $\sigma_1 = \sigma_2 = 0$, $\forall A < A^*$
(ii) $\sigma_i = 1, \sigma_j = 0, i \neq j$, , $\forall A \in [A^*, A^{**})$
(iii) $\sigma_1 = \sigma_2 = 1$, $\forall A \geq A^{**}$ ∎

Appendix 2: Program effects on parents

The model proposed in the paper assumes that parents continue to supply labor inelastically in response to a transfer that is conditioned on child schooling. This is consistent with the literature on CCTs (see Fiszbein and Schady 2009, especially Chapter 4, for a review of the evidence from a number of countries.) In addition, we find no evidence of significant changes in parental labor supply in Cambodia. Table 5 summarizes the results of estimating a simplified version of equation (1), focusing both on the incidence of different kinds of work (left-hand panel) and hours (right-side panel), with the latter estimated by Tobit and OLS, as before. We estimate this model for fathers and mothers, and for male and female applicants, separately. The only significant finding in Table 5 is that mothers are less likely to work without pay when their daughters receive a scholarship (a difference of 7 percentage points) although the change in hours (about 2 hours, on average) is quite small. There is little evidence of large reallocations of parent labor in these data.

Appendix Table: Program effects on parents

| | Did activity | | Hours of activity | | | |
| | | | Tobit (marginal effect) | | OLS | |
	Worked for pay	Worked without pay	Hours worked for pay	Hours worked without pay	Hours worked for pay	Hours worked without pay
Male applicant						
Father	-0.047	-0.040	2.083	-1.700	5.628	-1.711
	(0.078)	(0.078)	(2.158)	(3.769)	(3.428)	(3.868)
R-squared	0.20	0.20			0.18	0.22
Mother	0.030	0.021	0.687	-0.189	1.409	-0.334
	(0.048)	(0.056)	(0.956)	(2.544)	(2.023)	(2.600)
R-squared	0.20	0.17			0.19	0.17
Female applicant						
Father	0.032	-0.023	-0.032	-2.596	-1.664	-2.763
	(0.038)	(0.036)	(1.201)	(1.949)	(1.466)	(1.906)
R-squared	0.13	0.08			0.10	0.11
Mother	-0.011	0.073	-0.017	2.411	-0.486	1.803
	(0.031)	(0.032)*	(0.816)	(1.428)	(1.076)	(1.461)
R-squared	0.17	0.07			0.12	0.11

Note: Sample sizes are 489 for the sample of male applicants/fathers; 758 for the sample of male applicants/mothers;1425 for the sample of female applicants/fathers; and 1889 for the sample of female applicants/mothers. All specifications include a set of school dummies, a set of single year age dummies, and a quartic in the score. Standard errors adjust for clustering at the applicant primary-school level. ** significant at the 1 percent level, * at the 5 percent level.