Cash Transfers and Child Schooling: Evidence from a Randomized Evaluation of the Role of Conditionality*

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February 21, 2013

Abstract

We conduct a randomized experiment in Burkina Faso to estimate the impact of alternative cash transfer delivery mechanisms on education. The two-year pilot program randomly distributed cash transfers that were either conditional (on enrollment and attendance) or unconditional. Results indicate that conditional and unconditional transfers have similar impacts increasing enrollment for children traditionally favored by parents for school participation, including boys, older children, and higher ability children. However, conditional transfers are significantly more effective than unconditional transfers in improving the enrollment of "marginal children", those less likely to go to school, such as girls, younger children, and lower ability children.

Keywords: Cash transfers; Conditionality; Education; Africa
JEL classification: I2; J1; O1

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* These data were collected for a project evaluating social protection strategies in Burkina Faso, which greatly benefited from the support of Marie-Claire Damiba, Seydou Kabré and Victorine Yameogo from the Secrétariat Permanent du Comité National de Lutte contre le SIDA et les Infections Sexuellement Transmissibles in Burkina Faso and Nono Ayivi-Guedehoussou, Hans Binswanger, Ousmane Haidara, Timothy Johnston, Mead Over, and Tshiya Subayi-Cuppen at the World Bank. Data collection was supervised by Robert Ouedraogo, Jean-Pierre Sawadogo, Bambio Yiriyibin and Pam Zahonogo from the University of Ouagadougou. The project is funded by the NBER Africa Project, the University of Illinois at Urbana-Champaign, and the following World Bank trust funds grants: Strategic Impact Evaluation Fund (SIEF), Bank-Netherlands Partnership Program (BNPP), Gender Action Plan (GAP), Knowledge for Change Program (KCP), WB-DFID Evaluation of the Community Response to HIV and AIDS, Trust Fund for Environmentally & Socially Sustainable Development (TFESSD), and Luxembourg Poverty Reduction Partnership (LPRP). The research protocol received IRB clearance from the Institutional Review Board at the University of Illinois at Urbana-Champaign (case #08334) and from the Burkina Faso National IRB ("Comité d'Ethique"). The authors thank Emilie Bagby, German Caruso, Christine Jachetta, Marleen Marra, and Nga Thi Viet Nguyen for excellent research assistance. The authors also thank Adam Wagstaff and seminar participants at Dalhousie, Oklahoma State, and the World Bank for helpful comments on earlier drafts. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank, its Executive Directors, or the countries they represent.

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1. Introduction

Conditional cash transfer (CCT) programs are one of the most popular social sector interventions in developing countries.[1] While program design details vary, they all transfer resources to poor households conditional on the household taking active measures to increase the human capital of their children (enrolling their children in school, maintaining their attendance, and taking them for regular health care visits). In making transfers conditional, interventions seek to encourage human capital accumulation and break a cycle where poverty is transmitted across generations. While both CCT and unconditional cash transfer (UCT) programs provide poor households with resources, UCT programs do not impose conditionality constraints. An important question is whether and how conditions imposed by CCTs influence the outcomes they seek to improve.

In this paper, we present evidence of the education impacts from a unique cash transfer pilot program in rural Burkina Faso, the Nahouri Cash Transfers Pilot Project (NCTPP). The NCTPP incorporated a random experimental design to evaluate the relative effectiveness of the following four cash transfer programs targeting poor households in the same setting in rural Burkina Faso: conditional cash transfers given to fathers, conditional cash transfers given to mothers, unconditional cash transfers given to fathers, and unconditional cash transfers given to mothers. This paper focuses on the differential impact of conditional and unconditional cash transfers on the educational outcomes of children between the ages of 7 to 15.

The main contribution of our paper is to develop and empirically test the hypothesis that CCTs are more effective than UCTs in improving the enrollment of "marginal children", those who are initially not enrolled in school or are less likely to go to school, such as girls, younger children, and lower ability children. We start from the observation that parents in this setting often decide strategically to invest more in the education of some of their children (Akresh, Bagby, de Walque, and Kazianga, 2012a, 2012b highlight strategic enrollment choices by parents based on child ability using our baseline survey). Because our sample population includes all children (boys and girls ages 7-15), we can explicitly measure the differential impacts of conditionality on "marginal" children compared to other children.

[1] As of 2011, eighteen countries in Latin America and the Caribbean had implemented CCT programs, with four others in the process of designing ones (Stampini and Tornarolli, 2012). These CCT programs have approximately 135 million beneficiaries, about a quarter of the population. In terms of program size, the budget costs range from 0.50 percent of GDP in Brazil and Mexico to 0.08 percent in Paraguay and Chile (Fiszbein and Schady, 2009). In addition to Latin America, a growing number of countries in Asia have implemented CCT programs, while in Africa, several CCT pilot programs have begun in Kenya, South Africa, Malawi, and Morocco.

There is credible evidence that both types of transfer schemes can substantially improve child education.[2] However, only one published study explicitly compares conditional and unconditional cash transfers in the same context (Baird, McIntosh, and Özler, 2011).[3] They examine in Malawi the impact of conditionality on the drop-out rate of adolescent girls enrolled at baseline and find that CCTs are more effective than UCTs for these girls. As we discuss in greater detail below, our results differ from theirs. We find that CCTs are more effective than UCTs for marginal children (a group that might include adolescent girls in Malawi), while UCTs are equally effective as CCTs for non-marginal children. Since our cash transfer intervention focused on a broader range of child age and gender and on both margins of school enrollment (bringing non-enrolled children into school and reducing drop-outs), we are able to explain how conditionality works and to specify for which types of children it works best.

We are aware of one other cash transfer evaluation project in Morocco, with a design similar to ours, which examines the impact of conditionality on educational outcomes. The Burkina Faso and Morocco projects were conducted independently but at exactly the same time. Preliminary results of the Morocco experiment indicate no differences between conditional and unconditional cash transfers (Benhassine, Devoto, Duflo, Dupas, Pouliquen, 2012). The authors offer several potential explanations for their results. In the Morocco experiment, child enrollment and attendance were high, so the conditionality constraints were inframarginal, while in Burkina Faso, many children were not enrolled at the baseline, so conditionality was binding. In addition, because program registration in Morocco for all treatments was done at the schools, including those receiving unconditional transfers, this could have increased parents' views about returns to education and the quality of schooling or confused them about the role of conditionality. In Burkina Faso, transfers were delivered in each treatment village in a central location away from the schools by a village committee set up by the government to administer the cash transfer pilot.

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[2] For the evidence of CCT impacts on education in Mexico see Schultz (2004), Behrman, Sengupta, and Todd (2005), de Janvry et al. (2006), and Attanasio, Meghir, and Santiago (2011); in Colombia see Attanasio et al. (2010) and Barrera-Osorio et al. (2012); in Nicaragua see Maluccio and Flores (2005) and Macours, Schady, and Vakis (2012); in Honduras see Glewwe and Olinto (2004); in Brazil see Bursztyn and Coffman (2012) and Glewwe and Kassouf (2012); in Cambodia see Filmer and Schady (2011). For the evidence of UCT education impacts in Ecuador see Paxson and Schady (2010) and Edmonds and Schady (2012); in South Africa see Case, Hosegood, and Lund (2005) and Edmonds (2006).

[3] Other studies use accidental glitches in program implementation to compare UCTs and CCTs. Some households in Mexico (de Brauw and Hoddinott, 2011) and Ecuador (Schady and Araujo, 2008) did not think the cash transfer program was conditional, and school enrollment was lower among those households who thought the transfers were unconditional. Evaluations using structural models conduct counterfactual analyses that find UCTs would have no impact or a much lower impact on enrollment (Bourguignon, Ferreira and Leite, 2003; Todd and Wolpin, 2006).

As a consequence, in the UCT villages, there was no explicit or implicit linking of the cash transfers to schooling, which enables us to capture the pure income effect of the UCTs.

Our results indicate that CCTs are more effective than UCTs in improving the enrollment of "marginal" children, those who are initially not enrolled in school or are less likely to go to school, including girls, younger children, and lower ability children. With annual transfer amounts of $17.6 for children ages 7-10 and $35.2 for children ages 11-15, we find that CCTs lead to statistically significant increases in enrollment of 20.3 percent for girls, 37.3 percent for younger children, and 36.2 percent for low ability children relative to mean enrollment in those sub-groups. For these same categories of marginal children, UCTs either had no statistically significant impact or showed an impact that was significantly smaller than the CCT effect.[4] However, we find that UCTs and CCTs have similar impacts in increasing the enrollment of children who are already enrolled at baseline or are traditionally prioritized by parents for school participation, including boys, older children, and higher ability children. We find enrollment increases due to CCTs and UCTs respectively of 21.8 and 22.2 percent for boys, 17.4 and 14 percent for older children, and 27.0 and 28.5 percent for higher ability children.

These results shed new light on the role of conditionality in cash transfer programs. In resource-poor settings, both UCTs and CCTs relax the budget constraint and allow households to enroll more of the children they would traditionally prioritize for human capital investments. But the conditions attached to CCTs play a critical role in improving the outcomes of children in whom parents are less likely to invest.

The remainder of the paper is organized as follows. In Section 2, we develop a conceptual framework formulating our hypothesis that CCTs are more effective in improving the schooling outcomes of marginal children. Section 3 describes the context of our experiment and the design of the cash transfer pilot program. Section 4 describes our empirical identification strategy, and Section 5 presents the main results and robustness checks. Section 6 concludes.

2. Conceptual Framework: "Marginal Child" Hypothesis

In this section, we motivate our underlying hypotheses for empirically testing the relative merits of CCTs and UCTs. The conditions attached to CCTs are meant to induce households to behave differently than they would have under UCTs that paid the same amount of cash. By distorting

[4] With regards to the Malawi evaluation (Baird, McIntosh, Özler, 2011), to the extent that adolescent girls in secondary school (the focus of their study) can be considered as "marginal" children from an education point of view, our marginal child hypothesis would have predicted that CCTs would have been more effective than UCTs.

household choices (if conditionality is binding) to achieve a more socially desirable outcome (in this case increased education for marginal children), CCTs can lead to lower household welfare compared to UCTs.[5] Another argument against making transfers conditional is that because the conditions need to be verified, CCTs are more expensive per child to implement and the administrative capacity to conduct them may not be sufficient in less-developed countries.

Conditionality is often justified by the observed low investment in human capital. This low investment may be due to parents not internalizing positive social externalities of education (de Janvry and Sadoulet, 2005), to parental agency problems whereby parents make education and child labor decisions but do not adequately consider the child's future welfare (Edmonds, 2008), to parental irrationality, impatience, or lack of self-control (Das, Do, and Özler, 2005), to borrowing constraints or the absence of negative bequests across generations (Martinelli and Parker, 2003), or to underestimates of returns to education (Jensen, 2010). A further justification for conditionality invokes political economy arguments claiming that non-poor individuals would only agree to transfer programs if conditions were in place (Gelbach and Pritchett, 2002).

The common approach in the literature when comparing CCTs and UCTs is to depict investment in human capital against another good (Das, Do and Özler, 2005).[6] This essentially assumes educational investments are homogenous across children, and households differ only in how much they invest in their children. We take a slightly different approach and introduce the idea of a marginal child to motivate our hypotheses. We define a marginal child as one who has a lower tendency to enroll in school absent an external intervention. In contrast, a non-marginal child is one the household would be more likely to enroll even without an external intervention. In the empirical section, we confirm that specific types of children, such as girls or low cognitive ability children, are less likely to be enrolled in the baseline prior to the transfer program.

We illustrate our conceptual framework in Figure 1. Households choose between education and other goods.[7] The minimum desired level of education (for example enrollment and 90 percent attendance) and the threshold for conditionality to be satisfied is represented by point *E*. In the absence of transfers, the household budget constraint is represented by line *AB*.

[5] This is similar to the textbook example of in-kind versus cash transfers (Cunha, 2010).

[6] In this section, we use education in our discussion because that is the focus of the empirical analysis. However, our framework can accommodate other types of human capital as well.

[7] Education of marginal and non-marginal children are two distinct goods and households allocate budgets between these two goods and other goods. This distinction between marginal and non-marginal child education could be due to higher effective expenses to educate marginal children (e.g. more grade repetition of marginal children).

Parents make different choices for marginal and non-marginal children[8]: under budget constraint *AB*, parents invest more in the education of their non-marginal children (point *a*) than their marginal children (point *a'*). The income elasticity of education is smaller for marginal children than for non-marginal ones so that as income increases the income-consumption curve is represented by the line *OH* going through points *a* and *b* for high ability (non-marginal) children and by the line *OL* going through points *a'* and *b'* for low ability (marginal) children.[9]

With UCTs, a household receives a quarterly cash transfer for each child in the relevant age range. This is equivalent to a shift of the budget constraint to the right, bringing the household to the UCT budget constraint *CD*. Under UCTs, parents increase education more for their non-marginal children (to point *b*) compared to the increase for their marginal children (to point *b'*). With CCTs, a quarterly cash transfer is paid to households for each child who is enrolled and attends school at least 90 percent of the time (i.e. consumes at least *E* of education). The budget constraint under CCTs is represented in bold (*AFc'D*) and is kinked at point *E*. To the right of *E*, the household receives the CCT, the budget constraint is represented by the line *c'D*, and it coincides with the UCT budget constraint. To the left of *E*, the condition is not satisfied, the household does not receive the CCT, and the budget constraint reverts to the line *AF*, along the initial budget constraint. For non-marginal children, the household's utility is maximized at point *b* under both the UCT and CCT programs, indicating that both interventions have the same effects on education. However, for marginal children, point *b'* is unattainable under a CCT. The household chooses point *c'*, satisfies the education condition *E*, and receives the transfer. Point *c'* is preferred to point *a'*, where the education condition is not satisfied, but the indifference curve at point *b'* under the UCT would have been preferred to the outcome under the CCT.

Figure 1's simple framework can motivate a clear empirically testable proposition: when considering only human capital investments, relative to UCTs, CCTs increase investment in human capital of marginal children and no non-marginal child is made worse off. The empirical implications are the following. First, CCTs increase education for marginal children more than UCTs. Second, UCTs and CCTs have similar educational impacts for non-marginal children.

[8] This feature allows us to accommodate situations where most households do not enroll all of their children, which is common in rural Burkina Faso (Akresh, Bagby, de Walque, and Kazianga, 2012a, 2012b). Baseline data indicate only 24 percent of households enroll all children, and non-marginal children such as boys, high ability children, and older children are more likely to be enrolled.

[9] While marginal and non-marginal children have different income elasticities, for simplicity, we assume the income elasticity is constant across the income range for each child type.

3. Context and Experimental Design

3.1 Context

Burkina Faso offers an important setting for exploring the effects of cash transfers on rural children's education. Even by African standards, education outcomes in Burkina Faso are poor. In 2010, the net attendance ratio for primary school in rural Burkina Faso was 44.4 (45.5 for boys and 43.1 for girls) and the gross attendance ratio was 64.9 (66.2 for boys and 63.5 for girls) (Institut National de la Statistique et de la Démographie and ICF International, 2012).[10]

The cash transfer program was run in Nahouri province in southern Burkina Faso, 100 miles from the capital, Ouagadougou. Households in the region consist mainly of subsistence farmers growing sorghum and groundnuts. Table 1a shows that for the entire sample (N = 2629 households), there are, on average, 6.6 members in each household, of whom 1.6 are children under 84 months and 1.9 are children of school going age (7 to 15). Mean annual household per capita expenditures (including own consumption) were 99,951 FCFA (approximately $220 USD using the January 2010 exchange rate of $1 USD = 455 FCFA). Of the children ages 7 to 15, 65.7 percent are reported by their parents to be enrolled in school, but when enrollment is measured using school administrative rosters, the enrollment rate is only 49.2 percent, suggesting survey respondents may overstate school participation as Baird and Özler (2012) document.[11] School attendance conditional on enrollment as measured from school rosters is high at 98.1 percent, suggesting that, once enrolled, children are very likely to attend classes. The attendance rates are consistent with other research in Africa using administrative school data (Miguel and Kremer, 2004 in Kenya; Benhassine et al., 2012 in Morocco; Kazianga, de Walque, and Alderman, 2012 in another region of Burkina Faso). Furthermore, at least for Burkina Faso

[10] The primary school net attendance ratio is the percentage of children attending primary school who are of the official school age. The primary school gross attendance ratio is the number of primary school students, irrespective of age, as a percentage of the official primary-school-age population. If there are a significant number of underage or overage students in primary school, the gross attendance ratio is higher than the net attendance ratio.

[11] To obtain the school roster information about enrollment and attendance, survey enumerators took the list of children reported to be enrolled by their parents and searched for them in the school's administrative rosters. After matching the child's name, they then recorded the child's enrollment status and the number of days the child was absent or present for each month during the academic year. Unfortunately, we were not able to collect school administrative data for every year for all children. There were 225 school rosters to be collected (3 rounds of data collection for 75 villages), but we could not collect 5 of them due to the school being closed and the teachers and principal having left for summer vacation. In addition, for some children, it was difficult to identify a match between the names on the school and household rosters because many children in a given class often have the same first and last name. For these difficult cases, we used child age, gender, and the mother and father's names to confirm a match. In Section 5.5, we discuss the robustness checks we perform to confirm there was no differential selection across treatment groups in which children we were not able to collect administrative data for.

where enrollment is low, they are suggestive of an environment where parents strategically choose which children they enroll and then make sure the children attend regularly. We also report attendance unconditional on enrollment, a broader measure of school participation that incorporates enrollment and attendance effects. On an average school day, 46.2 percent of children ages 7 to 15 are in class. Mean education expenses per child are $9.66 per year.

Table 1b focuses on baseline summary statistics for school enrollment and attendance. Columns 1 to 6 disaggregate those statistics by gender, age group, and ability level as measured by the Raven's raw score.[12] For enrollment using both self-reported and school-based measures and for attendance, we observe that, at baseline, girls are less likely to be enrolled and attend school than boys. A similar pattern of lower enrollment and attendance is observed for younger children[13] (ages 7 to 8) compared to older children (ages 9 to 13) and for children with lower cognitive ability (a Raven's score below the sample mean) compared to higher ability children (those with a Raven's score above the mean). All of those baseline differences are statistically significant. These observations at baseline support our description of girls, younger children, and lower ability children as categories of "marginal" children in our conceptual framework.

3.2 Experimental Design: Burkina Faso Nahouri Cash Transfers Pilot Project

The 75 villages in Nahouri province that each have a primary school were randomly allocated to the following five groups as illustrated in Panel A of Appendix Figure 1: (i) conditional cash transfers given to the father, (ii) conditional cash transfers given to the mother, (iii) unconditional cash transfers given to the father, (iv) unconditional cash transfers given to the mother, and (v) a control group.[14] There were 15 villages in each treatment arm and in the control group, and only poor households were eligible to receive a cash transfer.[15] After villages were randomly assigned

[12] We use the Raven's Colored Progressive Matrices (CPM) to measure a child's cognitive ability. The Raven's CPM is a measure of fluid intelligence or problem solving ability, and it does not require formal schooling to be able to answer the questions (Raven, Raven, and Court, 1998). The test does not depend heavily on verbal skills, making it relatively "culture free" (Borghans, Duckworth, Heckman, and ter Weel, 2008). In the Raven's test, the child respondent is asked to select the image that is missing in order to complete a picture.

[13] Age seven is the official school starting age in Burkina Faso, but many children start school at a later age.

[14] Due to the low primary school enrollment rates in Burkina Faso, the program intervention focused exclusively on primary schooling as opposed to also covering secondary schools.

[15] Immediately prior to the baseline survey, we conducted a household census in every village to collect information from each household about living structure (flooring, access to latrine), ownership of assets (plow, cart, draft animals, motorcycle, radio), whether the household head ever attended school, whether the household grows cotton, and whether there is a weekly village market. We combined this information with the Burkina Faso nationally representative household survey INSD Burkinabe Survey on Household Living Conditions 2003 to calculate a predicted poverty level for each household and compare that with the national poverty line to determine if a household should be considered poor and eligible to receive cash transfers.

to the five groups defined above, poor households in the treatment villages were randomly assigned to receive that particular type of cash transfer.[16]

In our three survey rounds (baseline, one-year follow-up, two-year follow-up) conducted in June 2008, June 2009, and June 2010, we interviewed all poor households in each of the treatment villages who were randomly selected to receive the transfer. In each of these four groups of 15 villages, we interviewed approximately 540 poor households randomly selected to receive transfers. The control group consisted of 615 randomly selected poor households that did not receive cash transfers in the 15 control villages where no households received transfers.[17]

In households randomly assigned to CCTs, the mother or father received a quarterly stipend for each child if that child satisfied the following conditions. For children under age seven, receiving the transfer required quarterly visits to the local health clinic for growth monitoring (Akresh, de Walque, Kazianga, 2013). For children ages 7 to 15, receiving the transfer required enrollment in school and attendance above 90 percent each quarter.[18] Each child in the CCT households was given a program booklet in which school attendance or health clinic visits were recorded by the school teachers or clinic staff, respectively. The booklets were used to confirm a child's satisfaction of the conditionality requirements needed to receive CCTs. In addition, 20 percent of these children were randomly selected and a village committee that had been specifically trained to do audits verified the information in the booklets against health clinic and school administrative registers. Based on our discussions with these committees, it appears that conditionality was enforced. Cash transfer take-up rates (the fraction of eligible households receiving transfers for at least some children) in the CCT villages declined as the school year progressed, which is also consistent with conditionality being enforced.[19]

In households randomly assigned to UCTs, the mother or father received a quarterly stipend for each child. There were no requirements or conditions linked to receiving the stipend.

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[16] To minimize child fostering in response to the program introduction and reduce any associated risk of statistical contamination (see Akresh, 2009, for evidence on the relationship between income shocks and child fostering), eligibility for transfers was based only on the children present in the household at the time of the baseline survey.

[17] Note that the difference between the number of households interviewed and the number used in this paper's analysis is due to some households being excluded from the analysis because they had no children ages 7 to 15.

[18] In the CCT villages, the first payment of the school year was conditional only on school enrollment and not attendance, since attendance cannot be measured in the holiday period preceding the start of the school year.

[19] The CCT take-up rates by quarter for school year 2008-2009 are 99.0, 91.0, 90.7, and 85.3 percent, respectively. In school year 2009-2010, the rates are 94.7, 91.6, 89.9, and 89.7 for each quarter, respectively. The take-up rates in the UCT villages are considerably higher. In school year 2008-2009, they are 99.4, 98.8, 98.6, and 94.5 percent for each quarter, respectively. In 2009-2010, they are 99.1, 98.8, 98.5, and 97.1 percent for each quarter, respectively.

CCT and UCT households were told they could use the funds at their convenience and no instructions were given as to how to spend the money. Cash distribution was done in each village to minimize any risks of cross-village information contamination of the randomization, since we did not want households in UCT villages to believe that health clinic or school attendance was going to be checked in their villages as well. In addition, each village had only one primary school, and no children attended a primary school that is not in their village. Furthermore, our program design explicitly assumed each treatment group would receive equal amounts of resources per capita over the two-year pilot, if households randomly allocated to the CCTs fully satisfied conditionality. In practice, because there was not full compliance with conditionality, households receiving UCTs, on average, received more money per capita.

In the CCT and UCT programs, for each child under age seven, the mother or father would receive 4,000 FCFA per year distributed in quarterly payments (approximately $8.8 USD or 4.0 percent of household per capita expenditures). For each child ages 7 to 10 (or in grades 1 to 4 in the CCT villages), the mother or father would receive 8,000 FCFA per year in quarterly payments (approximately $17.6 USD or 8.0 percent of household per capita expenditures), while for each child ages 11 to 15 (or in grades 5 or higher but younger than 15 in the CCT villages), the mother or father would receive 16,000 FCFA per year in quarterly payments (approximately $35.2 USD or 16.0 percent of household per capita expenditures). To compare the generosity of this pilot project to other cash transfer programs, we measure the annual transfer amount that each household was eligible for as a fraction of household per capita expenditures and find that at 10.4 percent, the Burkina Faso cash transfer pilot was small in size (see Fiszbein and Schady, 2009 who note CCT program generosity levels of 1, 6, 17, 22, and 29 percent of household expenditures in Bangladesh, Brazil, Colombia, Mexico, and Nicaragua, respectively).

4. Empirical Identification Strategy

The key question we address is whether cash transfers improve educational outcomes, such as enrollment, attendance, and achievement test scores, of children ages 7 to 15 in recipient households. To obtain clearer comparisons between the different transfer modalities and to increase statistical power, in the empirical estimations, we pool treatment arms and consider households that were randomly selected to receive either conditional or unconditional cash transfers (Panel B of Appendix Figure 1). This approach combines into one group conditional cash transfers given to fathers or to mothers and into a second group unconditional cash transfers given to fathers or to mothers. With this approach, we highlight the role of conditionality, and we ignore the intra-household allocation aspects of the experimental design.[20]

The randomized experimental design provides a strong identification strategy that allows us to attribute differences in outcomes between the treatment and control groups to the impact of the program. We first present results based on a specification that does not include the baseline data and exclusively relies on the random allocation of interventions across villages and on the data from the final follow-up survey in 2010 (Round 3). We focus on the program's average treatment effect and estimate the following regression:

(1) $$y_{ihv} = \beta_0 + \alpha_1 CCT_{hv} + \alpha_2 UCT_{hv} + \beta_3 X_{ihv} + \varepsilon_{ihv}$$

where y_{ihv} is an educational outcome for child *i* in household *h* in village *v* in Round 3, CCT_{hv} is the treatment indicator that takes the value one if a child lives in a household that was randomly selected to receive conditional cash transfers and zero otherwise, UCT_{hv} is the treatment indicator that takes the value one if a child lives in a household that was randomly selected to receive unconditional cash transfers and zero otherwise, X_{ihv} is a vector of child characteristics (gender and age) included to reduce residual variation across arms after randomization, and ε_{ihv} is a random, idiosyncratic error term.

Since our data collection included baseline and follow-up surveys, we can control for differences across villages in the baseline values of the variables. To do so, we use the following difference-in-differences model:

(2) $$y_{ihvt} = \beta_1 + \beta_2 T_2 + \beta_3 T_3 + \sum_{j=1}^{2} \alpha_{2j} D_j T_2 + \sum_{j=1}^{2} \alpha_{3j} D_j T_3 + \beta_4 X_{ihvt} + L_v + \varepsilon_{ihvt}$$

where y_{ihvt} is an educational outcome for child *i* in household *h* in village *v* and year *t*, T_2 and T_3 are round indicators for the first and second follow-up surveys (Rounds 2 and 3, respectively), D_j is the treatment indicator that takes the value one if a child lives in a household that was randomly selected to receive treatment *j* (CCT or UCT) and zero otherwise, X_{ihvt} is a vector of child characteristics (gender and age), L_v is a village fixed effect, and ε_{ihvt} is a random, idiosyncratic error term.[21] The impact of transfer scheme *j* (*j*=1, 2) in period *t* (t=2, 3) is given by α_{tj}, the coefficient on the interaction between the treatment status and the round dummy. Since

[20] In on-going subsequent analysis, we explore the differential impacts of giving transfers to fathers or mothers.
[21] Correlation among the error terms of children living in a village and experiencing similar shocks in the baseline or follow-up rounds, combined with the design effect of our village-level before and after treatment, might bias the OLS standard errors downward, so in all regressions we cluster the standard errors at the village*follow-up level.

we randomized at the village level and we control for village fixed effects, the treatment dummies (D_j's) would be redundant in Equation 2 and therefore are not included.

Due to logistical reasons, the cash transfer program was unexpectedly launched late by Burkina Faso's government in the 2008-2009 school year. The first cash payment was only made at the end of November/early December 2008, while the school year started October 1, 2008. This meant most households were not able to enroll their children during the program's first year as they did not receive the transfer in time to pay school fees due at the start of the academic year. Subsequently, as we will see when we discuss the results, we do not observe any education impacts during the first year of the program. For this reason, we also present a difference-in-differences specification that only includes the baseline Round 1 data and the follow-up Round 3 data from the 2009-2010 school year. In Equation 3, the round indicator is for the second follow-up survey (Round 3) conducted in June 2010, and the other variables are as defined previously.

(3)
$$y_{ihvt} = \beta_1 + \beta_3 T_3 + \sum_{j=1}^{2} \alpha_{3j} D_j T_3 + \beta_4 X_{ihvt} + L_v + \varepsilon_{ihvt}$$

5. Empirical Results

5.1. Baseline Balance and Attrition

In Tables 2a and 2b, we use baseline data to confirm that household, school, and child characteristics are balanced across the treatment groups and between treatment and control. In columns 1-5, we present variable means measured at baseline for the control group and each of the treatment arms. In column 6, we estimate regressions of each characteristic on CCT and UCT treatment dummies, as that is the focus of this paper, and then calculate a Wald test of the equality of the UCT and CCT variables. In column 7, we estimate regressions of each characteristic on dummies for the five groups and then calculate an F-test of the joint test that the means of the five groups are equal. In Tables 2a and 2b, results show good balance overall across study arms for school, household, and child characteristics. In particular, school quality (graduation rates) and resources (provides meals, has latrines, water source, facilities for hand washing, and sufficient chalk and other teaching materials) appear to be consistent across groups. For only one variable (ethnic group is Nankana) is there a statistically significant difference between the CCT and UCT treatments. Across the five groups, we observe statistically significant differences for child age and the proportion of low ability children. Even though these three significant differences across the 72 tests are likely the product of chance and thus do not invalidate our identification, our main results are robust to including household level controls and child age and gender in the regressions.

Household attrition was extremely low between the baseline and one-year follow-up survey (1.26 percent), and increased slightly when comparing the baseline and two-year follow-up survey (3.56 percent). In Appendix Table 1a, we explore the relative differences between attritor and non-attritor households. Column 1 presents means of household-level characteristics from the baseline survey for households that were followed from the baseline to the two-year follow-up survey (non-attritors). Column 2 presents means for the sample of attritor households, and column 3 presents the average difference in characteristics between attritors and non-attritors, as well as whether the difference is statistically significant. Results suggest that attrition is not likely random, as attritor households are more likely to be smaller and Christian and less likely to be polygamous, animist, or of the Nankana ethnicity. However, what is more relevant for our analysis is whether the attritors' characteristics differ across treatment and control groups. In column 4, we show the coefficient for the interaction term from a difference-in-differences regression for each characteristic comparing the difference between attritors and non-attritors in the CCT treatment group with the same difference between attritors and non-attritors in the control group. Column 5 presents the corresponding interaction term from a difference-in-differences regression comparing the UCT and control groups. Across the 32 regressions, we find no statistically significant difference in 30, while we find differences between the CCT and control groups in terms of polygamy and whether the household's religion is animism.

Appendix Table 1b presents a similar attrition analysis for child level variables. For most characteristics (except math test scores), children from attritor and non-attritor households look similar. In comparing whether the characteristics of attritors differ across treatment and control groups, we find no statistically significant difference in 23 of the 26 cases. We find differences between the control group and the intervention groups in terms of child age (both CCT and UCT) and parental self-reports of enrollment (CCT). This last result suggests that differences in self-reports of enrollment between attritors and non-attritors are not similar across control and treatment groups and justifies our attrition-related robustness checks discussed in Section 5.5.

5.2. Impacts on Enrollment

To analyze the impact of cash transfers on school enrollment, we use two measures of enrollment as dependent variables. The first comes from parental self-reports in the household survey. The second comes from school administrative ledgers that we collected at each school. Using two measures, one collected at the household-level and potentially prone to self-reporting bias as highlighted by Baird and Özler (2012) and one collected at the school-level and potentially more objective, reinforces the robustness of our analysis. The correlation between the parental self-report and the school-based measure is 0.79.

In Table 3, we analyze the impact of cash transfers on enrollment for all children ages 7 to 15 using the three specifications in Equations 1-3. The Equation 1 specification uses only the round 3 cross-sectional data and relies on the random allocation of interventions but does not control for potential residual baseline variation. For the parental self-report measure (column 1), we find that only the CCT intervention has a positive and significant impact, and we reject equality of the CCT and UCT coefficients. However, using the school-based enrollment measure (column 4), we find positive and significant impacts for both the CCT and UCT interventions and no statistically significant difference between the coefficients.

In Table 3 (columns 2 and 5), we present results using the Equation 2 difference-in-differences strategy. As previously discussed, results show no impact of the conditional or unconditional transfers at round 2 for school year 2008-2009, the program's first year, because the transfers were delivered too late in that school year. However, the results show significant impacts of transfers at round 3 for school year 2009-2010, when the transfers were delivered on time. More precisely, columns 2 and 5 show significant positive impacts in the CCT villages for children ages 7 to 15, using both enrollment measures, while there is a positive but not significant coefficient for the UCT villages. At round 3, the CCT and the UCT coefficients are significantly different from each other using the self-reported but not the school roster measure. These results are confirmed in columns 3 and 6 when we use the Equation 3 difference-in-differences specification using only the baseline and last follow-up surveys (rounds 1 and 3).[22]

Overall, when looking at all school-age children, across the two measures of enrollment and the three alternative specifications, Table 3 allows us to conclude that the cash transfer intervention had no impact on school enrollment in the first year (2008-2009), but CCTs had a positive impact on enrollment in the second year. The impact of the UCT intervention on enrollment for all children is less clear and often not statistically significant. The remainder of

[22] The point estimates in columns 1 and 4 are larger than in the difference-in-differences specifications in the other columns suggesting that controlling for residual baseline variation is important.

our analysis extends our discussion of Figure 1 to investigate how the impacts of the conditional and unconditional cash transfers vary with the type of child.

In Table 4, we explicitly test our "marginal" child hypothesis that UCTs and CCTs would have similar positive effects increasing enrollment for children who are traditionally more likely to go to school, but CCTs are more effective at getting parents to invest in children they normally do not prioritize. We test this hypothesis by examining the impact of the two types of transfers on school enrollment for marginal and non-marginal children, as defined by their baseline enrollment status, gender, age, and cognitive ability. We focus our analysis on the more objective and reliable school-based measure of enrollment and on the Equation 3 specification, a difference-in-differences regression using the baseline and second follow-up surveys.[23] This specification acknowledges the absence of round 2 impacts and focuses on the round 3 impacts.

Columns 1 and 2 in Table 4 compare the impacts of the different types of transfers for children who were already enrolled at baseline (column 1) and those who were not enrolled at baseline (column 2). All else equal, children not initially enrolled can be considered more marginal. Both types of transfers lead to positive and significant increases in enrollment for both types of children. Yet while the UCT and CCT coefficients are similar and not significantly different from each other for children enrolled at baseline, the CCT coefficient is significantly larger than the UCT coefficient for children who were not initially enrolled. Thus, CCTs seem to outperform UCTs in bringing into school children who had not been enrolled.

In columns 3-4, we compare the impact of CCTs and UCTs for boys and girls ages 7 to 15. For boys, both transfer types have similar magnitude impacts, increasing enrollment by 11 percentage points. In contrast, for girls only CCTs have a statistically significant impact, raising enrollment by 9.2 percentage points, and we reject equality between CCT and UCT coefficients. Since girls are on average less likely to be enrolled at baseline (see Table 1b summary statistics), these results are consistent with our marginal child hypothesis: CCTs and UCTs are equally

[23] Baird and Özler (2012) suggest that self-reported enrollment is often overstated and recommend collecting school-level administrative enrollment data. In our survey, when comparing parental self-reports and school administrative data, we find that 11.4 percent of all children reported by their parents to be enrolled are not enrolled according to the school data. Appendix Table 2 provides a qualitative summary of the main enrollment results using the parental reports and school-level administrative data. Results are consistent with both data sources. As we discuss below, the overall picture, using both enrollment measures, confirms that CCTs have significantly larger impacts on enrollment than UCTs for marginal children such as girls, young children, less able children, and children not enrolled at baseline. Appendix Table 3 shows the actual regression results based on the parent self-reports. Appendix Table 4 uses the school-based data to estimate the other empirical specifications for all the sub-categories analyzed in Table 4 with consistent results.

effective increasing the enrollment of children who are more likely to go to school (boys), but CCTs are more effective increasing enrollment for more marginal children such as girls.

In columns 5-6, we focus on the differential program impacts by age group. As shown in Table 1b, children ages 9 to 13 form the core school-going population with a higher proportion of children enrolled. Enrollment is lower at ages 7 to 8 as starting school late is typical in rural areas.[24] Unconditional and conditional cash transfers have similar positive impacts for children ages 9 to 13. Only the CCT coefficient is significantly different from zero, but the CCT and UCT coefficients are of similar magnitude and the p-value indicates we cannot reject equality. In contrast, for younger children ages 7 to 8 who are traditionally less likely to be enrolled, CCTs have a significantly larger positive impact than UCTs (column 6).

Columns 7-8 compare the impacts of CCTs and UCTs for higher and lower ability children as measured by the child's Raven's raw score. We define higher ability children as those who have a baseline Raven's score above the sample mean of 6.1 (column 7) and lower ability children as those who have a baseline Raven's score of 6 or below (column 8). The Table 1b summary statistics and earlier work analyzing the baseline data (Akresh, Bagby, de Walque, and Kazianga, 2012a, 2012b) show that lower ability children are less likely to be enrolled. Results show that both CCTs and UCTs have a positive impact on enrollment for more able children, and we cannot reject the equality of the coefficients. For lower ability children, both UCTs and CCTs have a positive impact in improving their enrollment, but the effect of CCTs is larger than UCTs (17.4 versus 9.2 percentage points, equality of coefficients rejected). The results again confirm our marginal child hypothesis suggesting conditionality plays a critical role in ensuring that children who are not normally prioritized for school are now being enrolled.

In Table 5, we further investigate the differential effects of CCTs and UCTs for sub-categories of marginal children.[25] In columns 1 and 2, we divide the sample of young children by gender. For both young boys and girls, CCTs have a statistically significant positive impact on enrollment, and we can reject equality between the CCT and UCT coefficients in both cases. In

[24] Consistent with other CCT programs, the Burkina Faso government decided to provide larger transfer amounts to older children ages 11-15 and smaller amounts to younger children ages 7-10. Our marginal child analysis deviates from those specific age cut-offs because we believe the youngest aged children (7-8) are more marginal due to delayed school enrollment and children ages 14-15 show sharp declines in enrollment because most rural villages do not have access to secondary schools. Nevertheless, our marginal child results are still consistent (results not shown) if we use the government age cut-offs to define marginal and non-marginal children. Further, results (not shown) are also consistent using slightly different age groupings for either the older or younger groups.

[25] Results (not shown) using the two alternative empirical specifications lead to similar conclusions.

columns 3 and 4, we divide the sample of less able children by gender. CCTs have a larger impact than UCTs for both lower ability boys and girls, although we only can reject the equality of coefficients for girls (p-value for the test in the boys sample is 0.195). In columns 5 and 6, we divide the sample of less able children by age. For both age groups of low ability children, we find that CCTs have a statistically significant positive impact, but we only can reject the equality of coefficients for the younger group (p-value for the test in the older sample is 0.144).

In Appendix Table 5, we present robustness checks where we vary the Raven's score cut-offs used to categorize children as low or high ability.[26] Raven's scores range from 0 to 18. In Table 4, we use the sample mean of 6.1 as the threshold, with lower ability children defined as those with scores from 0 to 6 and higher ability children as those with scores from 7 to 18. We reproduce those results in column 3. In the other columns, we decrease and increase the ability cut-off to verify that the results do not depend on the chosen threshold. We find that for low ability children, CCTs consistently outperform UCTs irrespective of the ability threshold used.

5.3 Impacts on Attendance

In Table 6, we analyze the impact of cash transfers on school attendance during the academic year for all children ages 7-15, unconditional on their enrollment. For each child, we compute the percentage of school days attended for the entire academic year. Children who are not enrolled receive an attendance rate of zero. This is a broad measure of school participation with direct policy relevance that accounts for enrollment and attendance effects and is not confounded by changes in the share of the sample enrolled. We rely on attendance recorded in school ledgers collected from each school.[27] The attendance results are consistent with the marginal child hypothesis described for enrollment.[28] CCTs increase school attendance at round 3 for all children and in all subgroups. UCTs increase attendance for non-marginal children (those enrolled at baseline, boys, older children, and higher ability children). For marginal children (those not enrolled at baseline, girls, young children, and low ability children), CCTs significantly outperform UCTs and we can reject the equality of the coefficients, while we cannot reject that equality for the non-marginal children.

[26] Results (not shown) using the two alternative empirical specifications lead to similar conclusions.

[27] The school ledgers have daily attendance data that we aggregate for the entire academic year. We also collected parental self-reports on attendance for the two weeks prior to the survey, but the data are not directly comparable with the school-based data. Moreover, since some villages were surveyed after the end of the school year, this self-reported measure is potentially less-reliable. Nevertheless, the analysis using parental self-reports of attendance yield results similar and consistent with the Table 6 results. .

[28] Similar results using the other empirical specifications are presented in Appendix Table 6.

5.4 Impacts on Learning Outcomes

Table 7 measures impacts of the two different types of cash transfers on learning. We examine scores on a standardized mathematics and French test (French is the official language in Burkina Faso and the language used in all primary schools), which were designed by the survey team in collaboration with education specialists at the Burkina Faso Ministry of Education. The tests were given to all children ages 7 to 15 in the surveyed households. We examine the impacts separately for enrolled children (as indicated by the school data) and for all children. We also examine impacts on final end-of-year school grades (column 1). Unlike the tests we designed for French and mathematics, final grades are only available for enrolled children. They are also not standardized and can thus vary across schools or even within schools. In columns 2 and 5, the dependent variable is the age-standardized z-score for the number correct on the mathematics test. In columns 3, 4, 6, and 7, the dependent variables are age-standardized z-scores for the number correct on the overall French test and the French reading sub-section, respectively.

We find no significant impact of transfers on grades or achievement tests, except for a positive impact of CCTs on the French reading test when examining all children.[29] However, it is important to stress that even though there is no differential learning across treatment and control groups, this does not mean there is no learning going on for these children. For children in the treatment groups who enroll between baseline and round 3, their mean test scores improve at the same rate as children in the control group who enroll across rounds. Our findings imply that transfers increase enrollment, and these children (who would not have been enrolled absent the intervention) are learning as much as their peers in the control group. This can further be seen by comparing the results for all children with only enrolled children. While not all statistically significant, the coefficients tend to be positive and larger (especially for the CCTs) for the full sample compared to the only enrolled sample of children. This suggests that, in the overall population of children, learning increases as more children are enrolled. In the sub-group analyses by gender, age, and ability level (results not shown), most coefficients are not significant. Overall, it is fair to conclude that the impacts on learning are limited, which is consistent with results for most other cash transfer programs (Filmer and Schady, 2009 in Cambodia and Benhassine et al., 2012 in Morocco also find limited learning impacts, but Baird, McIntosh, and Özler, 2011 document positive learning in Malawi).

[29] Results (not shown) using the two alternative empirical specifications lead to similar conclusions.

5.5 Robustness Checks: Attrition and Selection

Table 8 and Appendix Table 7 include robustness checks related to attrition and selection for the analyzed samples. In Table 8, we investigate with child-level regressions whether the child's household was resurveyed in round 3 (column 1), whether the child's enrollment or attendance information was missing from the school roster (columns 2 and 3), and whether the child did not take the mathematics and French achievement tests (column 4). We do not find any evidence that the treatment groups are correlated with household attrition, missing child information in the school rosters, or missing achievement tests.

While those results are reassuring and while attrition in our sample is low, to further confirm that attrition does not significantly impact our findings, in Appendix Table 7, we re-estimate regressions adjusted for attrition using an inverse probability weighting (IPW) approach suggested by Wooldridge (2002, 2010). IPW is based on the key assumption that sample attrition is ignorable with respect to the dependent variable, conditional on the observables in the attrition equation (Wooldridge, 2002). The IPW procedure consists of two stages. First, data from the baseline round are used to estimate the probability a household remains in the survey in round 3. The inverse of the predicted probabilities are then used to weight the data, essentially giving more weight to households who are more likely to leave, conditional on observables. The results of the IPW regressions in Appendix Table 7 are consistent with the results on all children (columns 3 and 6 of Table 3) and on marginal children (columns 2, 4, 6, and 8 of Table 4).

5.6 Cost-effectiveness Analysis

In Table 9, we compare the program's impact to its cost. The cost estimates in columns 1 and 2 include only the cash transfers given to households. Columns 3 and 4 include the cash transfers as well as the administrative costs. On average, each child received $13 per year under UCTs and $9 per year under CCTs (columns 1 and 2).[30] Including administrative costs, UCTs cost about $22 per child per year whereas CCTs cost about $20 per child per year.[31] Administrative costs are large relative to intervention costs, but this is common for pilot projects for which there are

[30] We report total transfers distributed divided by the number of eligible children in the treatment households. Hence for CCTs, the amount actually received would be higher for children who satisfied conditionality.

[31] In this small scale pilot, the administrative costs of verifying conditionality in the CCT villages were low because the government relied on existing committees of village volunteers. Therefore, administrative costs raised the total program costs only marginally more for CCTs than UCTs. Such an arrangement might not be feasible in all settings.

no economies of scale.[32] Given the size of the cash transfers and the estimated program impacts, we estimate how much it would cost to enroll one additional child. We also disaggregate our cost-effectiveness estimates by gender, age, and child ability.

It costs less to enroll an additional child under CCTs than under UCTs. If we consider transfer costs only (columns 1-2), enrolling an additional child ages 7 to 15 for one year requires $89 under CCTs and $194 under UCTs. The gender difference is more pronounced under UCTs than CCTs. Under UCTs, enrolling an additional girl costs $458 or 4 times more than enrolling an additional boy ($116), whereas under CCTs enrolling a girl costs 1.2 times more than enrolling a boy.[33] Under CCTs, enrolling one additional child ages 7 to 8 costs $37 per year, which is less than the $94 it costs to enroll one additional child ages 9 to 13. Similarly, CCTs cost less to enroll an additional low ability child ($51) than a high ability child ($61). In contrast, enrolling an additional low ability child under UCTs costs $139, 1.7 times the amount needed to enroll an additional high ability child. Overall, the estimates indicate that CCTs are more cost-effective at improving enrollment, particularly for marginal children that parents would not have enrolled. Accounting for administrative costs (columns 3-4) does not alter the overall pattern. Given the higher administrative costs for CCTs, it is noteworthy that CCTs remains more cost-effective even after we account for these administrative costs.

One way to compare transfer programs would be to consider the hypothetical scenario where all resources are shifted from UCTs to CCTs. This corresponds to dividing column 1 by 2 or column 3 by 4. For all children, the cost per additional enrollment under UCTs would be 2.2 additional enrollments under CCTs or 1.7 additional enrollments when administrative costs are incorporated. The gains from reallocating resources from UCTs to CCTs are even larger when considering marginal children. For girls, the gain is about 4.8 and 3.7 additional enrollments, with and without administrative costs, respectively. For young children, the corresponding amounts are 3.7 and 2.9, while for low ability children, the change is 2.8 and 2.1.

We also compare our program's impact with other programs with similar objectives. Such a comparison is made difficult not only by the fact those programs took place in different contexts and countries, but also because programs often have multiple objectives and should not

[32] Caldés, Coady, and Maluccio (2006) document cost-to transfer ratios (CTRs) for three programs in Latin America. Over the length of the programs, they find CTRs of 10.6 percent in Mexico, 49.9 percent in Honduras and 62.9 percent in Nicaragua. For the first year of the programs, the CTRs are 134.2, 114.5 and 254.3 respectively.

[33] Note that the estimated impact of UCTs is small in magnitude and not statistically different from zero for girls. Therefore, it is possible that $458 would not get one additional girl in school.

be judged solely on school enrollment impacts. With these caveats in mind, our CCTs have comparable enrollment impacts to the mid-range of cost-benefit estimates from other studies, including school meals in Kenya at $43.34 (Vermeersch and Kremer, 2005) and teacher incentives in India at $67.64 (Duflo, Hanna and Ryan, 2012). However, the costs to enroll an additional child are higher than cheaper interventions such as deworming in Kenya at $4.36 (Miguel and Kremer, 2004). On the other hand, the cost per additional child enrolled is substantially lower compared to other CCT programs. de Janvry and Sadoulet (2006) estimate the Mexican Progresa program cost $9600 for each additional primary school enrollment. They demonstrate that efficiency gains through better targeting mechanisms could reduce this cost to $802-$1151, which would still be significantly larger than our cost estimates.

6. Conclusion

Social safety nets are actively promoted in developing nations both as responses to financial crises and as mechanisms to alleviate poverty. Conditional cash transfers, which are now common in Latin America but remain relatively rare in other regions, are also seen as a way to reduce future poverty by investing in the next generation's human capital (Fiszbein and Schady, 2009). However, the role of conditionality in achieving this objective is unclear. In this paper, we explicitly compare the impact of conditional and unconditional cash transfers on schooling outcomes in the same environment using a randomized experiment in rural Burkina Faso. Our results indicate that unconditional and conditional cash transfers have similar impacts increasing the enrollment of children who are traditionally prioritized by households for school participation such as boys, higher ability children, and those of core school-going age. However, conditional cash transfers are more effective than unconditional cash transfers in improving the enrollment of "marginal" children, those who are initially less likely to go to school, such as girls, lower ability children, and younger children. Results are consistent with the literature on compensating versus reinforcing investments that finds parents often decide strategically to invest more in the education of some of their children (Behrman, Rosenzweig and Taubman, 1994; Almond and Currie, 2011; Adhvaryu and Nyshadham, 2012; Bharadwaj, Loken and Neilson, forthcoming).

Our results shed new light on the role of conditionality in cash transfer programs, by suggesting how and for which categories of children CCTs outperform UCTs. In resource-poor settings, both UCTs and CCTs relax the budget constraint and allow households to enroll more of the children they would traditionally prioritize for human capital investments. However, the conditions attached to CCTs play a critical role in improving the outcomes of children for whom parents are less likely to invest.

The policy implications of these results are clear: the choice between CCTs and UCTs should be influenced by the objectives of the education policy. If the objective is to increase overall enrollment, UCTs might have comparable effects to CCTs. Since CCT programs are generally significantly more costly to administer per recipient than UCT programs due to the expenses associated with monitoring that the conditions are met, UCTs are generally assumed to be more cost-effective under that objective. However, this is not what we found in this study, as administrative costs for CCTs in our pilot were relatively limited. Furthermore, if the policy objective also includes an emphasis on improving the enrollment and educational outcomes of children who are less likely to be part of the education system, then CCTs are likely to have larger impacts and be more cost-effective. That conclusion is especially relevant in the context of Millennium Development Goal 3 which focuses on reducing the gender gap in education.

From a policy-making perspective, our study also addresses the feasibility of conditional cash transfer schemes in Sub-Saharan Africa. Since CCT programs rely on a certain level of administrative capacity (the ability to target households, plan meetings to notify households of their obligations and rights, monitor household compliance and conditionality, and transfer funds to families), there is a debate on whether these programs, which have been successful in Latin America, can be successfully implemented by African central or local governments (Samson, 2006; Schubert and Slater, 2006; Szekely, 2006; Freelander, 2007). The cash transfer program we study relied on existing government structures and was implemented in an environment where there is no systematic population registration and where formal banking is almost non-existent. Even though our study was a two-year pilot limited to one province and its scalability remains to be investigated, it nevertheless indicates that CCTs can be implemented and be effective in an environment with limited administrative capacity.

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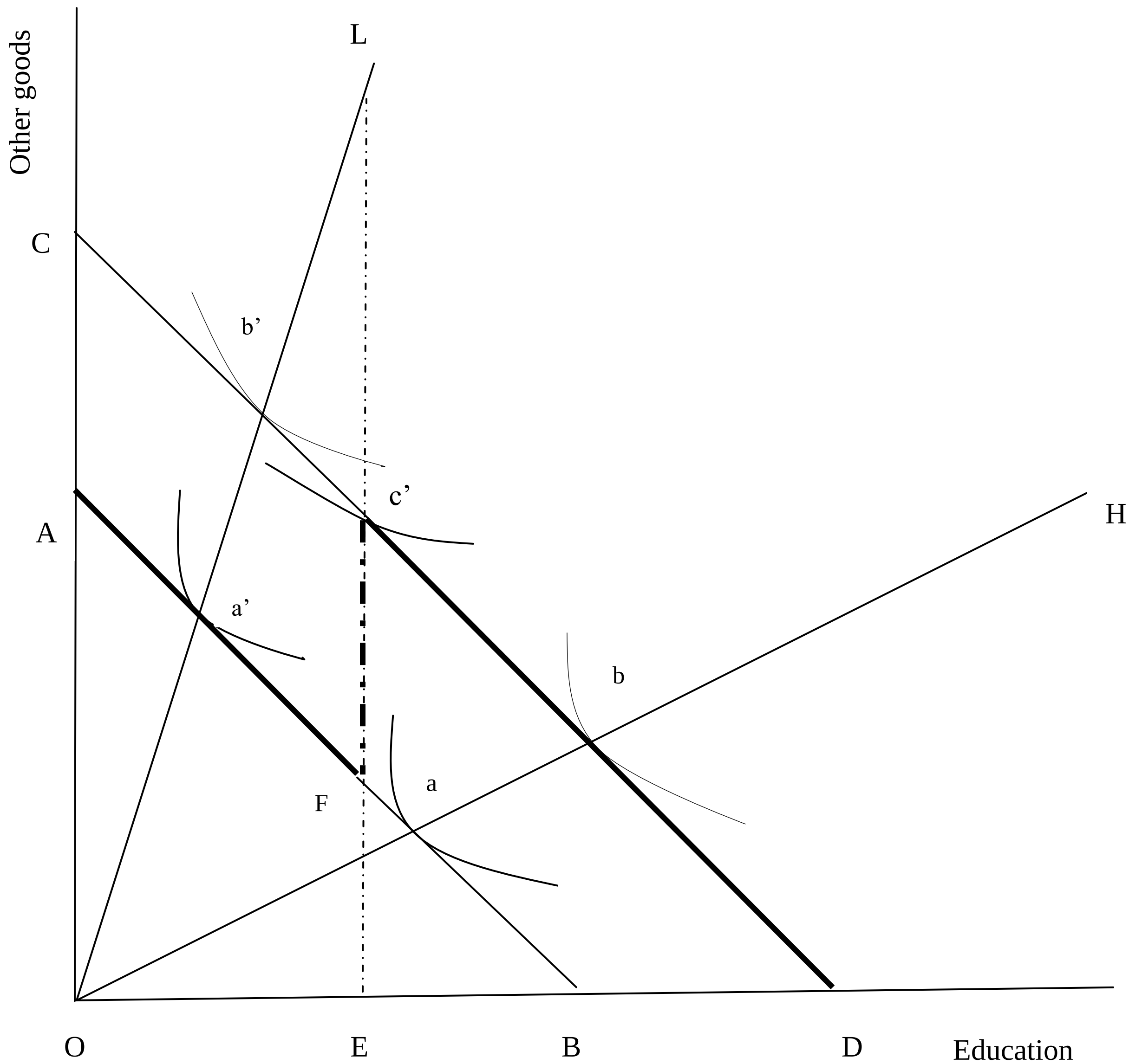
Figure 1: Conditional and Unconditional Cash Transfers and Child Types



Notes: The budget line at baseline is *AB*. The government threshold for the minimum desired level of education is given at *E*. The income-consumption curves are *OH* for non-marginal children and *OL* for marginal children. At follow-up with the cash transfer intervention, there are two budget lines: *CD* under the unconditional cash transfer and *AFc’D* under the conditional cash transfer. The budget constraint under the conditional cash transfer is kinked at *E* because the household does not receive any transfers unless a child receives at least *E* education. Under both the unconditional and conditional cash transfer programs, education for high ability non-marginal children moves from point *a* to point *b*. However, for low ability marginal children, under the unconditional cash transfer, education moves from point *a’* to point *b’*, while under the conditional cash transfer, it moves to point *c’* due to the conditionality requirement.

Table 1a: Summary Statistics of Burkina Faso Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation Data

Variable	Mean	Standard Deviation
Household Characteristics		
Household Size	6.58	3.10
Number of Children Ages 0 to 6 Years	1.56	1.24
Number of Children Ages 7 to 15 Years	1.88	1.41
Proportion Either Parent Ever Enrolled in School	0.15	0.36
Household Expenditures Per Capita (in FCFA)	99,951	67,183
Child Characteristics (children ages 7-15)		
Child Gender (1 = female)	0.49	0.50
Child Age (in years)	10.63	2.52
Proportion Enrolled (parent report)	0.657	0.475
Proportion Enrolled (school roster report)	0.492	0.500
Proportion Attending School, Conditional on Enrollment (school roster report)	0.981	0.077
Proportion Attending School, Unconditional on Enrollment (school roster report)	0.462	0.493
Mean French Test Z-score	0.007	0.996
Mean French Reading Test Z-score	0.003	0.991
Mean Math Test Z-score	-0.008	0.994
Mean Final Grade in School	5.21	1.99
Probability of Taking Math and French Tests	0.880	0.325
Raw Raven Score	6.05	3.38
Probability of Taking the Raven Test	0.798	0.401
Proportion of Lower Ability Children (Raw Raven Score 0-6)	0.727	0.445
Mean Per Child Education Expenses (in FCFA)	4,396	8,464

Notes: Household characteristics are based on the 2,629 households that were eligible to receive cash transfers (treatment and control groups) and that have children ages 7 to 15. Child characteristics are based on the children ages 7 to 15 present in these households during at least one of the three survey rounds. Household expenditures are measured in FCFA (455 FCFA=$1 USD), and they also include the value of household consumption of own-produced staple crops. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Table 1b: Baseline Summary Statistics for Education, By Gender, Age, and Ability

Variable	Boys, Ages 7-15 (1)	Girls, Ages 7-15 (2)	All, Ages 9-13 (3)	All, Ages 7-8 (4)	High ability, Ages 7-15 (5)	Low ability, Ages 7-15 (6)	P-value Testing Equality of Boys and Girls (col. 1 = col. 2) (7)	P-value Testing Equality of Ages 9-13 and 7-8 (col. 3 = col. 4) (8)	P-value Testing Equality of High and Low Ability (col. 5 = col. 6) (9)
Proportion Enrolled	0.639	0.604	0.679	0.610	0.699	0.622	0.011	0.000	0.000
(parent report)	(0.480)	(0.489)	(0.467)	(0.488)	(0.459)	(0.485)			
Proportion Enrolled	0.501	0.453	0.541	0.461	0.534	0.481	0.004	0.000	0.005
(school roster report)	(0.500)	(0.498)	(0.498)	(0.499)	(0.499)	(0.500)			
Proportion Attending,	0.482	0.433	0.519	0.447	0.509	0.464	0.003	0.000	0.016
Unconditional on Enrollment (school roster report)	(0.493)	(0.489)	(0.492)	(0.490)	(0.491)	(0.492)			
Number of Children	2587	2366	2780	1375	1360	3018			

Notes: Robust standard deviations clustered at the village level in parentheses. Child enrollment and attendance are based on the 4953 children ages 7 to 15 in the baseline survey. Ability is measured using the Raven's Colored Progressive Matrices. Low ability children are those with a baseline Raven's raw score below the sample mean of 6.1; higher ability children have a baseline Raven's raw score above the sample mean. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008.

Table 2a: Baseline Means and Randomization Balance: Household Characteristics

	Mean for CCT-Father (1)	Mean for CCT-Mother (2)	Mean for UCT-Father (3)	Mean for UCT-Mother (4)	Mean for Control (5)	P-value Testing UCT = CCT (6)	P-value Testing 5 Groups Equal (7)
Household Head is Female	0.16	0.16	0.14	0.12	0.18	0.231	0.494
Household Head Ever Enrolled in School	0.16	0.13	0.11	0.16	0.11	0.713	0.354
Household Head Age	47.43	47.46	47.43	46.52	47.85	0.596	0.867
Household Size	6.98	6.91	7.33	7.09	6.59	0.236	0.293
Marital Status = Monogamous	0.56	0.55	0.55	0.59	0.55	0.709	0.685
Marital Status = Polygamous	0.23	0.23	0.26	0.25	0.21	0.498	0.669
Marital Status = Single	0.21	0.22	0.20	0.16	0.24	0.311	0.454
Ethnic Group = Kassena	0.57	0.36	0.56	0.71	0.52	0.134	0.190
Ethnic Group = Nankana/Farfarse	0.38	0.49	0.26	0.15	0.40	0.038**	0.149
Ethnic Group = Mossi	0.03	0.07	0.12	0.08	0.06	0.222	0.418
Religion = Muslim	0.20	0.27	0.21	0.26	0.22	0.955	0.866
Religion = Christian	0.26	0.28	0.22	0.24	0.28	0.318	0.637
Religion = Animist	0.53	0.43	0.57	0.49	0.49	0.541	0.698
Number of Wives of Household Head's Father	2.18	2.20	2.56	2.22	2.24	0.168	0.551
Number of Children of Household Head's Father	9.10	9.09	10.00	8.93	9.04	0.486	0.798
Household Head's Father is Educated	0.020	0.038	0.042	0.023	0.025	0.710	0.454

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT-Father (conditional cash transfers to fathers), CCT-Mother (conditional cash transfers to mothers), UCT-Father (unconditional cash transfers to fathers), and UCT-Mother (unconditional cash transfers to mothers). Marital status refers to the marital status of the household head. In column 6, we estimate regressions of each characteristic on CCT and UCT treatment dummies and then calculate a Wald test of the equality of the UCT and CCT variables. In column 7, we estimate regressions of each characteristic on dummies for the 5 groups and then calculate an F-test of the joint test that the means of the 5 groups are equal. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008.

Table 2b: Baseline Means and Randomization Balance: School and Child Characteristics

	Mean for CCT-Father (1)	Mean for CCT-Mother (2)	Mean for UCT-Father (3)	Mean for UCT-Mother (4)	Mean for Control (5)	P-value Testing UCT = CCT (6)	P-value Testing 5 Groups Equal (7)
School Characteristics							
School Provides Meals	0.462	0.629	0.529	0.500	0.583	0.815	0.915
Water is Available at the School	0.538	0.484	0.202	0.600	0.333	0.416	0.119
School Has Well-Maintained Latrines	0.333	0.590	0.429	0.400	0.417	0.685	0.734
School Has Facilities for Students to Wash Hands	0.077	0.071	0.135	0.067	0.083	0.722	0.979
School Lacked Chalk During Previous Year	0.154	0.236	0.135	0.133	0.083	0.519	0.858
School Lacked Other Teaching Materials	0.583	0.649	0.606	0.533	0.583	0.705	0.980
Number Students Graduated Primary School Last Year	7.00	9.56	12.60	9.14	8.80	0.365	0.739
Child Characteristics							
Child is Female	0.49	0.46	0.47	0.48	0.49	0.847	0.530
Child Age in Years	10.53	10.58	10.57	10.31	10.65	0.156	0.009**
Proportion Enrolled (parent report)	0.637	0.661	0.580	0.631	0.608	0.247	0.649
Proportion Enrolled (school roster report)	0.491	0.534	0.486	0.481	0.395	0.494	0.226
Proportion Attending, Unconditional on Enrollment	0.455	0.507	0.472	0.473	0.384	0.853	0.401
French Test Z-score	-0.042	0.086	0.040	0.045	-0.134	0.799	0.377
French Reading Test Z-score	-0.093	0.083	0.038	0.004	-0.051	0.695	0.662
Math Test Z-score	-0.047	0.001	0.036	0.032	-0.097	0.470	0.542
Final Grade in School	5.338	5.188	5.336	5.414	5.336	0.552	0.862
Probability of Taking Math and French Tests	0.941	0.941	0.947	0.933	0.952	0.928	0.740
Probability of Taking Raven Test	0.891	0.854	0.886	0.893	0.894	0.413	0.766
Proportion Lower Ability Children (Raven Score 0-6)	0.647	0.691	0.660	0.681	0.766	0.965	0.091*
Mean Per Child Education Expenses (in FCFA)	4011	4131	4593	3385	3905	0.888	0.306

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The treatment arms are abbreviated as CCT-Father (conditional cash transfers to fathers), CCT-Mother (conditional cash transfers to mothers), UCT-Father (unconditional cash transfers to fathers), and UCT-Mother (unconditional cash transfers to mothers). In column 6, we estimate regressions of each characteristic on CCT and UCT treatment dummies and then calculate a Wald test of the equality of the UCT and CCT variables. In column 7, we estimate regressions of each characteristic on dummies for the 5 groups and then calculate an F-test of the joint test that the means of the 5 groups are equal. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008.

Table 3: Impact of Cash Transfers on School Enrollment, All Children Ages 7-15

	Round 3 Only	All 3 Rounds, Diff-in-Diff	Rounds 1&3, Diff-in-Diff	Round 3 Only	All 3 Rounds, Diff-in-Diff	Rounds 1&3, Diff-in-Diff
Dependent variable:	Parental Self-Report Enrollment			School Roster Report Enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.095**			0.179***		
	[0.040]			[0.049]		
UCT	0.012			0.136***		
	[0.044]			[0.048]		
CCT * Round 3		0.055**	0.057***		0.105*	0.099**
		[0.022]	[0.019]		[0.054]	[0.047]
UCT * Round 3		0.012	0.014		0.073	0.066
		[0.021]	[0.018]		[0.050]	[0.042]
CCT * Round 2		0.009			-0.004	
		[0.024]			[0.055]	
UCT * Round 2		0.036			-0.003	
		[0.023]			[0.055]	
Village Fixed Effects?	No	Yes	Yes	No	Yes	Yes
Child Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Round Dummies?	No	Yes	Yes	No	Yes	Yes
Number of observations	5,686	16,073	10,639	4,425	12,241	8,110
P-value testing equality between CCT and UCT:						
CCT*Rd3 = UCT*Rd3		0.018	0.010		0.362	0.276
CCT*Rd2 = UCT*Rd2		0.104			0.986	
At Round 3, CCT = UCT	0.021			0.307		

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include child age fixed effects and child gender. Columns 1 and 4 use the specification in Equation 1, columns 2 and 5 estimate Equation 2 and columns 3 and 6 estimate Equation 3. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). The last three rows report p-values testing the equality of the CCT and UCT coefficients at rounds 2 and 3 respectively. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Table 4: Impact of Cash Transfers on School Enrollment, By Baseline Enrollment Status, Gender, Age, and Ability

Dependent variable: Enrollment (School Roster Report)	Enrolled at Baseline	Not Enrolled at Baseline	Boys, Ages 7-15	Girls, Ages 7-15	Older Children, Ages 9-13	Younger Children, Ages 7-8	Higher Ability Children	Lower Ability Children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CCT * Round 3	0.117**	0.159***	0.109**	0.092*	0.094*	0.172***	0.144***	0.174***
	[0.056]	[0.037]	[0.046]	[0.053]	[0.055]	[0.060]	[0.054]	[0.059]
UCT * Round 3	0.125**	0.090**	0.111***	0.028	0.076	0.060	0.152***	0.092*
	[0.053]	[0.036]	[0.041]	[0.047]	[0.048]	[0.054]	[0.054]	[0.053]
Number of observations	3,023	3,827	4,187	3,923	4,587	2,271	1,681	4,477
P-value testing equality between CCT and UCT:								
CCT*Rd3 = UCT*Rd3	0.763	0.047	0.964	0.061	0.591	0.028	0.839	0.032

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions measure enrollment from the school roster report and use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven's Colored Progressive Matrices. Lower cognitive ability children have a baseline raw Raven's score below the mean of 6.1; higher cognitive ability children have a baseline raw Raven's score above the sample mean. Column 1 is restricted to children who were enrolled at the baseline Round 1 before the cash transfer intervention began. Column 2 is restricted to children who were not enrolled at the baseline Round 1. The last row reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Table 5: Impact of Cash Transfers on School Enrollment, By Gender, Age, and Ability Interactions

Dependent variable: Enrollment (School Roster Report)	Young Boys, Ages 7-8	Young Girls, Ages 7-8	Lower Ability Boys	Lower Ability Girls	Old and Lower Ability, Ages 9-13	Young and Lower Ability, Ages 7-8
	(1)	(2)	(3)	(4)	(5)	(6)
CCT * Round 3	0.163**	0.177**	0.194***	0.155**	0.165**	0.173**
	[0.082]	[0.073]	[0.056]	[0.072]	[0.070]	[0.074]
UCT * Round 3	0.065	0.049	0.139***	0.057	0.101	0.026
	[0.079]	[0.061]	[0.051]	[0.067]	[0.064]	[0.066]
Number of observations	1,154	1,117	2,319	2,158	2,523	1,434
P-value testing equality between CCT and UCT:						
CCT*Rd3 = UCT*Rd3	0.083	0.060	0.195	0.027	0.144	0.014

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions measure enrollment from the school roster report and use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven’s Colored Progressive Matrices. Lower cognitive ability children have a baseline raw Raven’s score below the mean of 6.1; higher cognitive ability children have a baseline raw Raven’s score above the sample mean. The last row reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Table 6: Impact of Cash Transfers on School Attendance, By Gender, Age, and Ability

Dependent variable: Attendance (School Roster Report)	All Children, Ages 7-15 (1)	Enrolled at Baseline (2)	Not Enrolled at Baseline (3)	Boys, Ages 7-15 (4)	Girls, Ages 7-15 (5)	Older Children, Ages 9-13 (6)	Younger Children, Ages 7-8 (7)	Higher Ability Children (8)	Lower Ability Children (9)
CCT * Round 3	0.134***	0.147**	0.163***	0.135***	0.137***	0.146**	0.191***	0.241***	0.218***
	[0.049]	(0.064)	(0.039)	[0.048]	[0.053]	[0.057]	[0.057]	[0.076]	[0.058]
UCT * Round 3	0.067	0.137**	0.099***	0.108**	0.032	0.090*	0.043	0.237***	0.091*
	[0.043]	(0.064)	(0.036)	[0.042]	[0.049]	[0.050]	[0.053]	[0.074]	[0.053]
Number of observations	7,818	2,811	3,765	4,038	3,780	4,377	2,222	1,598	4,300
P-value testing equality between CCT and UCT:									
CCT*Rd 3 = UCT*Rd 3	0.044	0.752	0.090	0.464	0.002	0.135	0.004	0.933	0.003

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. Attendance is school attendance unconditional on enrollment. The measure is taken from the school roster and measures the proportion of school days the child attended during the entire academic year. The regressions use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven's Colored Progressive Matrices. Lower cognitive ability children have a baseline raw Raven's score below the mean of 6.1; higher cognitive ability children have a baseline raw Raven's score above the sample mean. Column 2 is restricted to children who were enrolled at the baseline Round 1 before the cash transfer intervention began. Column 3 is restricted to children who were not enrolled at the baseline Round 1. The last row reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Table 7: Impact of Cash Transfers on Learning

Dependent Variable:	Final Grade in School	Math Test Z-score	French Test Z-score	French Reading Test Z-score	Math Test Z-score	French Test Z-score	French Reading Test Z-score
Sample Restricted To:	Only Enrolled	Only Enrolled	Only Enrolled	Only Enrolled	All children	All children	All children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CCT * Round 3	-0.191	-0.043	-0.152	0.119	0.051	0.069	0.196**
	[0.235]	[0.103]	[0.173]	[0.149]	[0.065]	[0.095]	[0.090]
UCT * Round 3	-0.044	-0.104	-0.221	-0.062	-0.083	-0.130	0.003
	[0.226]	[0.104]	[0.161]	[0.132]	[0.069]	[0.097]	[0.084]
Number of observations	3,741	3,687	3,526	3,526	8,594	7,733	7,733
P-value testing equality between CCT and UCT:							
CCT*Round 3 = UCT*Round 3	0.253	0.565	0.488	0.097	0.059	0.031	0.008

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. Achievement test scores on French and Math tests were administered by the survey enumerators at the child's home. All children whether enrolled in school or not were given the tests. Regressions in columns 1-4 are restricted to only children who were enrolled in school during that survey round; columns 5-7 include all children. We compute Z-scores for each child, where the Z-score is defined as the difference between the child's raw test score and the mean test score of the same-aged children, divided by the standard deviation of those same-aged children. Final grades for each child enrolled in school were recorded from school administrative rosters. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). The last row reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Table 8: Attrition and Selection

Dependent Variable:	Child's Household Is Present In Round 3	Child Missing School Roster Enrollment	Child Missing Attendance	Child Missing Achievement Test
	(1)	(2)	(3)	(4)
CCT * Round 3	0.012	0.023	-0.041	0.085
	[0.016]	[0.058]	[0.033]	[0.053]
UCT * Round 3	0.011	0.053	-0.016	0.074
	[0.016]	[0.045]	[0.026]	[0.050]
Number of observations	13,872	10,639	8,110	4,037
P-value testing equality between CCT and UCT:				
CCT*Round 3 = UCT*Round 3	0.890	0.492	0.380	0.703

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. The regressions use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes but use the following dependent variables: column 1: a binary variable indicating whether a household that was surveyed at baseline is resurveyed in round 3; column 2: a binary variable indicating whether a child is missing from the school administrative roster; column 3: a binary variable indicating whether a child is missing from the school attendance records; column 4: a binary variable indicating whether a child did not take the Math and French achievement tests. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). The last row reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Table 9: Cost-Effectiveness Analysis

	Excluding administrative costs		Including administrative costs	
	UCT	CCT	UCT	CCT
	(1)	(2)	(3)	(4)
Annual Transfer per child ($USD)	12.83	8.81	22.22	19.76
Cost to enroll one additional child (in $USD):				
All Children Ages 7-15	194.36	89.03	336.68	199.60
By gender				
Boys	115.57	80.86	200.19	180.26
Girls	458.14	95.80	793.60	216.13
By age				
Children Ages 9-13	169.90	93.86	293.49	210.31
Children Ages 7-8	137.17	36.67	293.71	100.31
By ability				
Higher Ability	84.39	61.21	146.19	137.22
Lower Ability	139.43	50.65	241.53	113.56

Notes: Annual transfer per child is the total transfers paid out in each treatment arm divided by the number of age-eligible children. Therefore, children who satisfied the conditionality requirements in CCT villages would have received larger transfers than this average amount. The coefficient estimates used in the calculations are: All children 7-15: column 6, Table 3; Boys: column 3, Table 4; Girls: column 4, Table 4; Older children 9-13: column 5, Table 4; Younger children 7-8: column 6, Table 4; Higher ability: column 7, Table 4; Lower ability: column 8, Table 4. All costs were converted from the local currency to US Dollars using the average exchange rate at the time of the surveys (455 FCFA = $1 USD). Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

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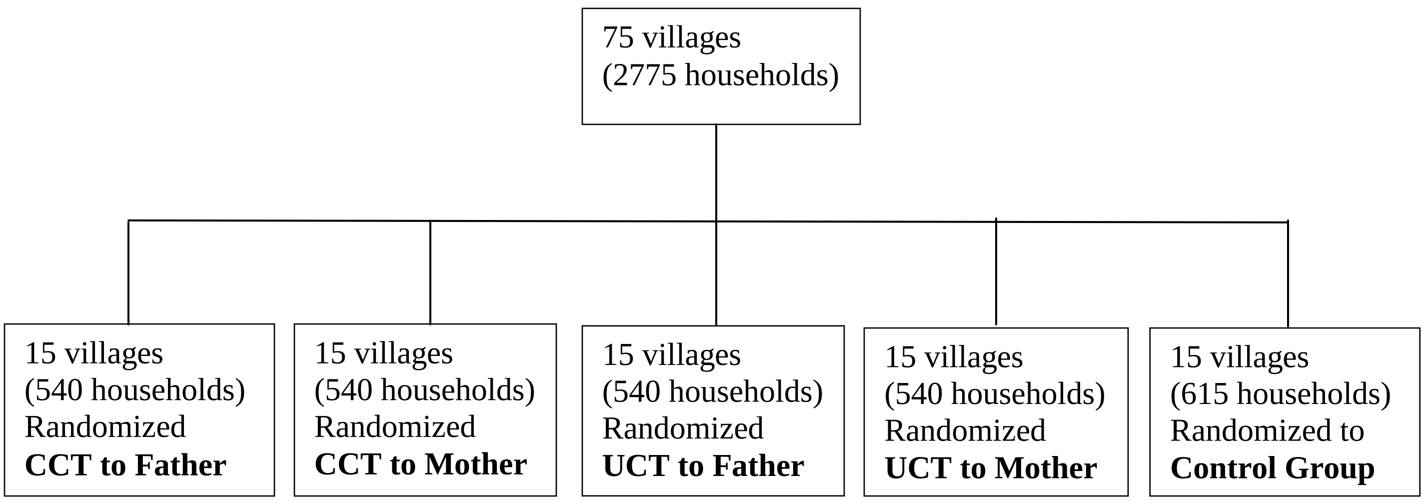
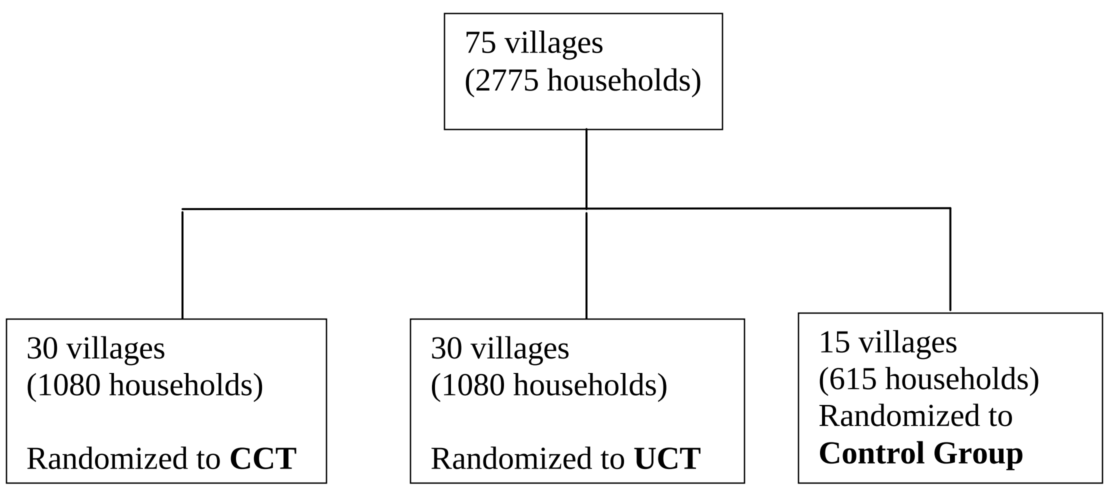
Appendix Figure 1: Summary of Treatment and Control Group Randomization Plan

Panel A: Experimental Design for Pilot Program



Panel B: Conditional Transfers versus Unconditional Transfers Comparison



Notes: The treatment arms are abbreviated as CCT-Father (conditional cash transfers to fathers), CCT-Mother (conditional cash transfers to mothers), UCT-Father (unconditional cash transfers to fathers), and UCT-Mother (unconditional cash transfers to mothers).

Appendix Table 1a: Relative Differences Between Attriting and Non-Attriting Households

	Non-Attritors (n=2139)	Attritors (n=79)	Mean Difference	CCT Diff in Diff	UCT Diff in Diff
	(1)	(2)	(3)	(4)	(5)
Household Head is Female	0.151	0.190	0.038	-0.062	-0.125
	(0.008)	(0.048)	(0.042)	(0.135)	(0.118)
Household Head Ever Enrolled	0.134	0.101	-0.032	-0.012	-0.074
	(0.007)	(0.034)	(0.039)	(0.094)	(0.084)
Household Head's Age	47.36	47.05	-0.31	2.04	1.48
	(0.303)	(1.524)	(1.603)	(3.159)	(3.611)
Household Size	7.006	6.139	-0.867**	-0.554	-0.301
	(0.068)	(0.276)	(0.359)	(0.522)	(0.566)
Marital Status = Monogamous	0.558	0.608	0.049	0.160	0.184
	(0.011)	(0.055)	(0.057)	(0.115)	(0.128)
Marital Status = Polygamous	0.238	0.139	-0.099**	-0.111*	-0.007
	(0.009)	(0.039)	(0.049)	(0.0633)	(0.0803)
Marital Status = Single	0.204	0.253	0.049	-0.050	-0.177
	(0.009)	(0.049)	(0.046)	(0.127)	(0.117)
Ethnic Group = Kassena	0.543	0.595	0.052	0.030	-0.007
	(0.011)	(0.056)	(0.057)	(0.175)	(0.185)
Ethnic Group = Nankana/Farfarse	0.341	0.241	-0.101*	-0.114	-0.100
	(0.010)	(0.048)	(0.054)	(0.180)	(0.165)
Ethnic Group = Mossi	0.072	0.089	0.017	0.045	0.040
	(0.006)	(0.032)	(0.030)	(0.0573)	(0.0866)
Religion = Muslim	0.231	0.241	0.009	-0.167	-0.166
	(0.009)	(0.048)	(0.048)	(0.118)	(0.131)
Religion = Christian	0.253	0.354	0.102**	-0.180	-0.0575
	(0.009)	(0.054)	(0.050)	(0.114)	(0.148)
Religion = Animist	0.507	0.392	-0.114**	0.316**	0.229
	(0.011)	(0.055)	(0.057)	-0.124	(0.158)
Number Wives of Household Head's Father	2.285	2.228	-0.057	0.571	0.682
	(0.044)	(0.229)	(0.235)	(0.435)	(0.598)
Number Children Household Head's Father	9.272	8.228	-1.044	-0.029	-0.778
	(0.159)	(0.592)	(0.834)	(1.594)	(1.637)
Household Head's Father is Educated	0.030	0.025	-0.005	-0.003	-0.042
	(0.004)	(0.018)	(0.019)	(0.0401)	(0.0291)

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Column 1 presents means and standard deviations of household-level characteristics from the baseline survey for the sample of households that were followed from the baseline to the two-year follow-up survey (non-attritors). Column 2 presents means and standard deviations for the sample of attritor households. Column 3 presents the average difference in characteristics between attritors and non-attritors. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Columns 4-5 test for differential impacts of attrition between treatment and control groups. For each characteristic, we estimate difference-in-differences regressions comparing attritors and non-attritors for the treatment (CCT or UCT) and control groups. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008.

Appendix Table 1b: Relative Differences Between Children from Attriting and Non-Attriting Households

	Non-Attritors (n=4803)	Attritors (n=150)	Mean Difference	CCT Diff in Diff	UCT Diff in Diff
	(1)	(2)	(3)	(4)	(5)
Child is Female	0.478	0.480	0.002	-0.041	0.034
	(0.007)	(0.041)	(0.041)	(0.0662)	(0.0666)
Child Age in Years	10.528	10.633	0.106	-0.915**	-0.679*
	(0.036)	(0.202)	(0.209)	(0.391)	(0.372)
Proportion Enrolled (parent report)	0.623	0.587	-0.037	-0.165**	-0.126
	(0.007)	(0.040)	(0.040)	(0.0712)	(0.0813)
Proportion Enrolled (school roster)	0.480	0.398	-0.082	-0.120	-0.109
	(0.008)	(0.050)	(0.051)	(0.119)	(0.105)
Proportion Attending, Unconditional	0.461	0.385	-0.076	-0.144	-0.102
	(0.008)	(0.049)	(0.051)	(0.117)	(0.105)
French Test Z-score	-0.003	-0.005	0.002	-0.154	-0.054
	(0.017)	(0.092)	(0.095)	(0.279)	(0.241)
French Reading Test Z-score	-0.010	0.091	0.101	-0.110	-0.252
	(0.017)	(0.094)	(0.094)	(0.277)	(0.176)
Math Test Z-score	-0.022	0.169	0.191**	0.145	-0.212
	(0.015)	(0.082)	(0.088)	(0.178)	(0.222)
Final Grade	5.313	5.555	0.243	-0.212	-0.493
	(0.051)	(0.458)	(0.370)	(0.714)	(0.832)
Probability Takes Math and French	0.944	0.920	-0.023	-0.0243	-0.0491
	(0.004)	(0.029)	(0.025)	(0.0765)	(0.0540)
Probability of Taking Raven test	0.884	0.873	-0.011	-0.005	0.009
	(0.005)	(0.027)	(0.027)	(0.0726)	(0.0537)
Proportion Low Ability Children	0.691	0.641	-0.050	-0.029	-0.014
	(0.007)	(0.042)	(0.041)	(0.116)	(0.113)
Mean Per Child Education Expenses	3981	4873	892	103.60	86.42
	(171.9)	(959.5)	(1,054.3)	(1,217)	(1,572)

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Column 1 presents means and standard deviations of child-level characteristics at baseline from the sample of households that were followed from the baseline to the two-year follow-up survey (non-attritors). Column 2 presents means and standard deviations for children in the sample of attritor households. Column 3 presents the average difference in characteristics between children in attritor and non-attritor households. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Columns 4-5 test for differential impacts of attrition between treatment and control groups. For each characteristic, we estimate difference-in-differences regressions comparing attritors and non-attritors for the treatment (CCT or UCT) and control groups. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008.

Appendix Table 2: Summary Table of the Impact of Cash Transfers on School Enrollment, Parental Self-Reports and School Roster Reports, For All Children and By Baseline Enrollment Status, Gender, Age, and Ability

Dependent variable: Enrollment	All 7-15 (1)	Enrolled at Baseline (2)	Not Enrolled at Baseline (3)	Boys 7-15 (4)	Girls 7-15 (5)	Older Children, Ages 9-13 (6)	Younger Children, Ages 7-8 (7)	Higher Ability Children (8)	Lower Ability Children (9)
School Roster Report									
CCT * Round 3	√	√	√	√	√	√	√	√	√
UCT * Round 3		√	√	√				√	√
Parental Self-Report									
CCT * Round 3	√	√	√	√	√	√	√		√
UCT * Round 3						√			
P-value testing equality between CCT = UCT									
School Roster Report	0.276	0.763	**0.047**	0.964	**0.061**	0.591	**0.028**	0.839	**0.032**
Parental Self-Report	**0.010**	**0.002**	**0.009**	0.176	**0.003**	0.593	**0.002**	**0.025**	**0.016**

Notes: √ denotes a positive and statistically significant coefficient [at least at the 10% level]. The last two rows report p-values testing the equality of the CCT and UCT coefficients at round 3, the p-values in bold indicate that the CCT coefficient is larger than the UCT coefficient and that we can reject their equality at least at the 10% level. This table is based on regressions using the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. All regressions are otherwise specified as in Tables 3 and 4. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Appendix Table 3: Impact of Cash Transfers on School Enrollment (Parental Self-Report), By Baseline Enrollment Status, Gender, Age, and Ability

Dependent variable: Enrollment (Parent Self-Report)	Enrolled at Baseline	Not Enrolled at Baseline	Boys 7-15	Girls 7-15	Older Children, Ages 9-13	Younger Children, Ages 7-8	Higher Ability Children	Lower Ability Children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rounds 1 & 3, Difference-in-Difference							
CCT * Round 3	0.045***	0.118***	0.050***	0.062**	0.055**	0.122***	0.053	0.080***
	[0.010]	[0.033]	[0.019]	[0.025]	[0.023]	[0.040]	[0.035]	[0.023]
UCT * Round 3	0.014	0.030	0.024	0.007	0.045**	0.003	-0.013	0.032
	[0.012]	[0.037]	[0.019]	[0.024]	[0.020]	[0.039]	[0.032]	[0.024]
Number of observations	5,803	4,161	5,453	5,186	6,018	2,785	2,172	5,663
P-value testing equality between CCT and UCT								
CCT*Rd3 = UCT*Rd3	0.002	0.009	0.176	0.003	0.593	0.002	0.025	0.016

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions measure enrollment from parental self-reports. The regressions use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. Treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven's Colored Progressive Matrices. Low ability children are those with a baseline Raven's raw score below the sample mean of 6.1; higher ability children have a baseline Raven's raw score above the sample mean. Column 1 is restricted to children who were enrolled at the baseline Round 1 before the cash transfer intervention began. Column 2 is restricted to children who were not enrolled at the baseline Round 1.The last row reports p-values testing equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Appendix Table 4: Impact of Cash Transfers on School Enrollment (School Roster Report), By Baseline Enrollment Status, Gender, Age, and Ability, Alternative Empirical Specifications

Dependent variable: Enrollment (School Roster Report)	Enrolled at Baseline	Not Enrolled at Baseline	Boys 7-15	Girls 7-15	Older Children, Ages 9-13	Younger Children, Ages 7-8	Higher Ability Children	Lower Ability Children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All 3 Rounds, Difference-in-Difference								
CCT * Round 3	0.121*	0.152***	0.110**	0.103*	0.110*	0.162**	0.173***	0.190***
	[0.072]	[0.042]	[0.053]	[0.060]	[0.063]	[0.073]	[0.066]	[0.067]
UCT * Round 3	0.126*	0.086**	0.114**	0.036	0.094	0.045	0.183***	0.109*
	[0.072]	[0.041]	[0.050]	[0.055]	[0.059]	[0.067]	[0.065]	[0.063]
CCT * Round 2	0.175**	0.012	0.018	-0.021	0.016	0.002	-0.044	0.018
	[0.072]	[0.039]	[0.054]	[0.061]	[0.067]	[0.074]	[0.073]	[0.062]
UCT * Round 2	0.174**	0.031	0.044	-0.045	0.007	0.022	-0.021	0.016
	[0.076]	[0.036]	[0.055]	[0.059]	[0.067]	[0.073]	[0.071]	[0.063]
Number of observations	4,495	5,580	6,319	5,922	6,957	3,406	2,463	6,767
CCT*Rd3 = UCT*Rd3	0.890	0.082	0.912	0.086	0.659	0.039	0.817	0.067
Panel B: Round 3 Only, Cross-sectional Analysis								
CCT	0.125	0.151***	0.165***	0.192***	0.181***	0.244***	0.284***	0.217***
	[0.085]	[0.049]	[0.051]	[0.051]	[0.053]	[0.070]	[0.085]	[0.057]
UCT	0.134	0.092*	0.137***	0.133**	0.164***	0.097	0.277***	0.145***
	[0.082]	[0.047]	[0.048]	[0.052]	[0.055]	[0.065]	[0.088]	[0.054]
Number of observations	1,261	1,904	2,249	2,176	2,546	1,181	663	2,147
At round 3 CCT = UCT	0.819	0.213	0.545	0.176	0.698	0.013	0.919	0.099

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions measure enrollment from the school roster report. Panel A regressions use the difference-in-differences specification in Equation 2 with all 3 rounds of data. Panel B regressions use only Round 3 data to estimate the Equation 1 specification. All regressions include child age fixed effects and gender. Panel A regressions include village fixed effects and survey round dummies. Treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven's Colored Progressive Matrices. Low ability children have a baseline Raven's raw score below the sample mean; high ability children have a baseline Raven's raw score above the sample mean. Each panel's last row reports p-values testing equality of the round 3 CCT and UCT coefficients. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) 2008-2010 evaluation data.

Appendix Table 5: Impact of Cash Transfers on School Enrollment, Robustness Checks by Alternative Ability Threshold Cut-offs

Dependent variable: Enrollment (School Roster Report)	Lower Ability Children, Raven 0-4 (1)	Lower Ability Children, Raven 0-5 (2)	Lower Ability Children, Raven 0-6 (3)	Lower Ability Children, Raven 0-7 (4)	Lower Ability Children, Raven 0-8 (5)
CCT * Round 3	0.198***	0.172***	0.174***	0.184***	0.169***
	[0.064]	[0.061]	[0.059]	[0.058]	[0.057]
UCT * Round 3	0.131**	0.113**	0.092*	0.101*	0.096*
	[0.057]	[0.055]	[0.053]	[0.052]	[0.051]
Number of observations	2,949	3,775	4,477	4,990	5,385
P-value testing equality between CCT and UCT:					
CCT*Round3 = UCT*Round3	0.101	0.112	0.032	0.023	0.043

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions measure enrollment from the school roster report and use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven's Colored Progressive Matrices. Scores range from 0 to 18. Column 3 replicates the regression shown in Table 4 column 8 using the high/low cognitive ability threshold at the sample mean of 6.1. Columns 1 and 2 focus on children with lower cognitive abilities, those having a raw Raven's score of 0 to 4 or 0 to 5. Columns 4 and 5 define lower ability children as those with Raven's scores of 0 to 7 or 0 to 8. The last row reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Appendix Table 6: Impact of Cash Transfers on School Attendance, By Gender, Age, and Ability, Alternative Empirical Specifications

Dependent variable: Attendance (School Roster)	All Children 7-15 (1)	Boys 7-15 (2)	Girls 7-15 (3)	Older Children, Age 9-13 (4)	Younger Children, Age 7-8 (5)	Higher Ability Children (6)	Lower Ability Children (7)
	Panel A: All 3 Rounds, Difference-in-Difference						
CCT * Round 3	0.141**	0.136**	0.147**	0.161**	0.190***	0.267***	0.231***
	[0.058]	[0.056]	[0.063]	[0.067]	[0.071]	[0.084]	[0.069]
UCT * Round 3	0.076	0.114**	0.040	0.108*	0.039	0.257***	0.113*
	[0.053]	[0.052]	[0.058]	[0.062]	[0.066]	[0.082]	[0.065]
CCT * Round 2	0.017	0.025	0.015	0.043	0.027	0.016	0.028
	[0.063]	[0.063]	[0.067]	[0.073]	[0.080]	[0.078]	[0.071]
UCT * Round 2	0.000	0.041	-0.035	0.002	0.045	0.026	0.003
	[0.060]	[0.061]	[0.063]	[0.071]	[0.077]	[0.075]	[0.068]
Number observations	11,747	6,054	5,693	6,635	3,289	2,345	6,452
CCT*Rd3 = UCT*Rd3	0.109	0.605	0.012	0.241	0.010	0.838	0.022
	Panel B: Round 3 Only, Cross-sectional Analysis						
CCT	0.195***	0.178***	0.210***	0.204***	0.253***	0.372***	0.236***
	[0.052]	[0.053]	[0.055]	[0.057]	[0.073]	[0.078]	[0.059]
UCT	0.139***	0.139***	0.136**	0.175***	0.092	0.366***	0.150***
	[0.050]	[0.050]	[0.054]	[0.057]	[0.066]	[0.079]	[0.056]
Number observations	4,207	2,136	2,071	2,394	1,141	612	2,012
At round 3,CCT = UCT	0.241	0.442	0.140	0.555	0.011	0.934	0.087

Notes: Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. Attendance is school attendance unconditional on enrollment. The measure is taken from the school roster and measures the proportion of school days the child attended during the entire academic year. Regressions in Panel A use the difference-in-differences specification in Equation 2 using all 3 rounds of data. Regressions in Panel B use only the Round 3 data to estimate the Equation 1 specification. All regressions include child age fixed effects and child gender. Panel A regressions also include village fixed effects and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven's Colored Progressive Matrices. Lower cognitive ability children have a baseline raw Raven's score below the mean of 6.1; higher cognitive ability children have a baseline raw Raven's score above the sample mean The bottom row in each panel reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.

Appendix Table 7: Inverse Probability Weighted Estimates of the Impact of Cash Transfers on School Enrollment, By Baseline Enrollment Status, Gender, Age, and Ability

Dependent variable:	Parent Self-Report Enrollment	School Roster Report Enrollment				
	All Children 7-15	All Children 7-15	Not Enrolled at Baseline	Girls, Ages 7-15	Younger Children, Ages 7-8	Lower Ability Children
	(1)	(2)	(3)	(4)	(5)	(6)
CCT * Round 3	0.058***	0.098**	0.158***	0.092*	0.169***	0.172***
	[0.019]	[0.047]	[0.037]	[0.053]	[0.060]	[0.059]
UCT * Round 3	0.014	0.064	0.090**	0.026	0.058	0.091*
	[0.018]	[0.042]	[0.036]	[0.047]	[0.054]	[0.053]
Number observations	10,639	8,110	3,827	3,923	2,271	4,477
P-value testing equality between CCT and UCT:						
CCT*Rd3 = UCT*Rd3	0.009	0.562	0.046	0.060	0.030	0.032

Notes: Inverse probability weighted (IPW) estimates. Robust standard errors in brackets, clustered at the village*follow-up level. * significant at 10%; ** significant at 5%; *** significant at 1%. Column 1 measures enrollment from the parent self-report. Regressions in columns 2-6 measure enrollment from the school roster report. All regressions use the difference-in-differences specification in Equation 3 comparing Round 1 and 3 outcomes. All regressions include village fixed effects, child age fixed effects, child gender, and survey round dummies. The treatment arms are abbreviated as CCT (conditional cash transfer) and UCT (unconditional cash transfer). Ability is measured using the Raven's Colored Progressive Matrices. Lower cognitive ability children have a baseline raw Raven's score below the mean of 6.1; higher cognitive ability children have a baseline raw Raven's score above the sample mean. The last row reports p-values testing the equality of the CCT and UCT coefficients at round 3. Data source: Nahouri Cash Transfers Pilot Project (NCTPP) Evaluation data from 2008-2010.