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Cash or Condition?

Evidence from a Randomized Cash Transfer Program

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Abstract

Are the large enrollment effects of conditional cash transfer programs a result of the conditions or simply the cash? This paper presents the first experimental evidence on the effectiveness of conditionality in cash transfer programs for schooling. Using data from an intervention in Malawi that featured randomized conditional and unconditional treatment arms, the authors find that the program reduced the dropout rate by more than 40 percent and substantially increased regular school attendance among the target population of adolescent girls. However, they do not detect a higher impact in the conditional treatment group. This finding contrasts with previous non-experimental studies of conditional cash transfer programs, which found negligible "income" effects and strong "price" effects on schooling. The authors argue that their findings are consistent with the very low level of incomes and the high prevalence of teen marriage in the region. The results indicate that relatively small, unconditional cash transfers can be cost-effective in boosting school enrollment among adolescent girls in similar settings.

This paper—a product of the Poverty and Inequality Team, Development Research Group—is part of a larger effort in the department to improve the design and cost-effectiveness of cash transfer programs and to assess their impacts for a wider range of policy-relevant outcomes. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at bozler@worldbank.org.

CASH OR CONDITION?
EVIDENCE FROM A RANDOMIZED CASH TRANSFER PROGRAM[1]

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1. INTRODUCTION

A large and empirically well-identified body of evidence has demonstrated the ability of Conditional Cash Transfer programs (CCTs) to raise schooling rates in the developing world (Schultz 2004; de Janvry et al. 2006; Filmer and Schady 2008, among many others). Due in large part to the high-quality evaluation of Mexico's PROGRESA, CCT programs have become common in Latin America and are beginning to spread to other parts of the world. As of 2007, "…29 developing countries had some type of CCT program in place (in some cases, more than one) and many other countries were planning one." (Fiszbein and Schady 2009)[2] However, designing a new CCT program remains a complex task. Many difficult decisions need to be made regarding the selection of beneficiaries, the nature (and enforcement) of conditions, and the level and structure of payments. While numerous evaluations of CCTs have been conducted in Latin America, most studies evaluate a program with a single, fixed set of contract parameters. Therefore, the evidence base needed by a government to decide *how* to design a new CCT program is either limited or non-existent in several critical dimensions.

Among this set of design parameters, the issue of conditionality is seemingly the most controversial. Conditional transfer payments seek to constrain the actions undertaken by the beneficiaries, and hence cannot result in more welfare for the society as a whole than unconditional cash transfers in the absence of market failures (Ferreira 2009). Unconditional transfers generate a pure income effect, and can be expected to increase schooling either if households are credit constrained in their human capital investment decisions or simply through the concavity of utility in consumption. The conditionality imposed by the program increases the relative benefit of schooling, generating a further 'price effect' on the household decision to invest in schooling. The relative magnitude of these two effects has been hard to identify empirically, a distinction that is of much

[2] In fact, there is now a pilot CCT program in New York City that builds on the lessons learned from international CCT programs. For more on Opportunity NYC, see: http://www.nyc.gov/html/ceo/html/programs/opportunity_nyc.shtml

more than academic interest since it bears directly on the optimal design of (conditional) cash transfer programs.

The ideal experiment to answer this question – i.e. a randomized controlled trial with one treatment arm receiving *conditional* cash transfers, another receiving *unconditional* transfers, and a control group receiving *no* transfers – has not previously been conducted anywhere.[3] The evidence that can be gleaned so far is either from model-based simulation exercises (Bourguignon, Ferreira, and Leite 2003; Todd and Wolpin 2006) or from recent studies of interventions with implementation glitches (de Brauw and Hoddinott 2008; Schady and Araujo 2008).

This paper describes the schooling impacts of a randomized intervention in Malawi that provided cash transfers to adolescent girls to stay in school. In the experiment, 176 enumeration areas (EAs) were randomly assigned treatment or control status. Within the group of 88 treatment EAs, a sub-group of EAs was then randomly assigned to receive either *conditional* or *unconditional* transfers. This experimental design allows us to isolate the impact of the *conditionality* itself, above and beyond the simple income effect of a given transfer. In addition, the total transfers to the household were also independently randomized and took integer values between $5 and $15 per month.[4] This additional layer of experimentation with transfer size allows us to assess the income elasticity of schooling outcomes and to discern whether the marginal impact of the *conditionality* varies across the wide range of transfer amounts. We hope to contribute to the literature and inform

[3] To our knowledge, there are two other studies that plan to examine the impact of the conditionality in the near future. "Impact Evaluation of a Randomized Conditional Cash Transfer Program in Rural Education in Morocco" has three treatment arms: unconditional, conditional with minimal monitoring, and conditional with heavy monitoring (using finger printing machines at schools). A similar pilot in Burkina Faso has comparative treatment arms for conditional and unconditional transfers. Accumulation of reliable evidence on the effect of the conditionality on various outcomes of interest, such as those presented in this paper and to come from these other studies, promises to be of significant use to policy-makers designing cash transfer programs in the near future.

[4] World Bank (2009) argues that "…the key parameter in setting benefit levels is the size of the elasticity of the relevant outcomes to the benefit level". However, random variation in transfer size among program participants is rarely, if ever, observed in cash transfer programs around the world.

policymakers as to which combination of contract parameters might allow cash transfer programs to deliver the largest impacts per dollar spent.

We find that while the program has a strong impact on school enrollment, there is no difference in the size of that impact between the two randomized treatment arms. The findings become slightly more favorable towards the conditional treatment group when we analyze school attendance instead of enrollment, but because the basic income effect associated with the unconditional transfers is large, the marginal impact of the conditionality on schooling outcomes remains relatively small and is never statistically significant regardless of the outcome measure used in the analysis. Furthermore, increasing the total amount transferred to the household does not significantly improve schooling outcomes and the impact of the conditionality does not vary by this amount. Finally, we consider the relationship between income, schooling, and marriage for adolescent girls in Malawi, and show that while unconditional cash transfers nearly eliminate marriage in our study population, the conditional cash transfers have no effect on it.

In the next section, we provide a brief review of the literature on the relative importance of 'income' and 'price' effects in explaining the overall impacts of cash transfer programs. Because this literature is based solely on studies in Latin America, we also present some summary evidence contrasting Malawi with Latin America in terms of mean income and poverty levels. Section 3 describes the study design, and Section 4 our empirical methodology. Section 5 presents the core empirical results of the paper using Intention to Treat Effects and examines the heterogeneity of these impacts. We also discuss program impacts on marriage in this section. In Section 6, we conduct additional checks to test the robustness of our findings, and Section 7 concludes.

2. EVIDENCE FROM THE LITERATURE

2.1. Disentangling the 'Price Effect' from the 'Income Effect' in CCT Programs

In the absence of externalities, conditional cash transfers are worse than distributing an equivalent amount of unconditional cash. Das, Do, and Özler (2005) and Fiszbein and Schady (2009) provide reviews of the kinds of externalities that can justify the use of CCTs, which include physical and learning externalities as well as market failures arising from decisions made through an intra-household bargaining process.[5] Political economy considerations can also play a significant role in the design of conditional cash transfer programs (Gelbach and Pritchett 2002).

Regardless of the specific nature of the externality being addressed by the conditionality, it is important to know how much of the impact of CCT programs is a result of the income effect associated with the transfers and how much is due to the price change implicit in the condition. Conducting randomized pilots to answer this question can be difficult and expensive and, to date, experimental evidence has not been available to shed light on this issue. What we do know on the topic comes mainly from accidental glitches in program implementation or structural models of household behavior.

Evidence on the marginal effect of conditionality on school enrollment points us in favor of the conditions. Based on the fact that some households in Mexico and Ecuador did not think that the cash transfer program in their respective country was conditional on school attendance, de Brauw and Hoddinott (2008) and Schady and Araujo (2008) both find that school enrollment was significantly lower among those who thought that the cash transfers were *unconditional*. A reasonable objection to this type of identification strategy is that observed heterogeneity in the accuracy of implementation may be correlated with a set of unobserved factors that are driving the outcome.

[5] For example, Bursztyn and Coffman (2009) argues that parent-child conflict in schooling preferences can give rise to sub-optimal school attendance and provides evidence that the conditionality imposed by a government program helps solve the monitoring problem for the parents, who cannot control their children's school attendance.

An entirely different approach is the structural one, where a model of household behavior is calibrated using real data, and then the impact of various policy experiments is simulated. These ex-ante program evaluations provide further evidence that the impacts of CCT programs on schooling outcomes (and related outcomes, such as child labor) would have been significantly attenuated without the conditionality. In Brazil, Bourguignon, Ferreira, and Leite (2003) find that unconditional transfers would have no impact on school enrollment; while Todd and Wolpin (2006) report that the increase in schooling with unconditional transfers would be only 20% as large as the conditional transfers in Mexico and the cost per family would be an order of magnitude larger.[6]

Another key design parameter in CCT programs is the benefit level, which crucially depends on the size of the elasticity of the relevant outcomes to the transfer amounts. To our knowledge, there are no CCT programs under which the transfers are randomly varied across beneficiary households to estimate how schooling outcomes may improve as the transfer amount is increased. One study that addresses the issue of the impact of transfer size on enrollment is from Cambodia (Filmer and Schady 2009b).[7] The program offered two different transfer amounts to students based on their poverty status at baseline conditional on school enrollment and regular attendance. Using a regression discontinuity design, the authors find that while the difference between the impact of a

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[6] There is also some evidence that the condition that pre-school children receive regular check-ups at health clinics (enforced by a social marketing campaign, but not monitoring the condition) had a significant impact on child cognitive outcomes, physical health, and fine motor control. Two studies in Latin America – Paxson and Schady (2007) and Macours, Schady, and Vakis (2008) – show behavioral changes in the spending patterns of parents and households that they argue to be inconsistent with changes in *just* the household income. These studies, however, cannot isolate the impact of the social marketing campaign from that of the transfers being made to women.

[7] Structural models of household behavior have also been used to simulate the expected impacts of different transfer amounts on various outcomes. Bourguignon, Ferreira, and Leite (2003) find that doubling the transfer amount under Brazil's Bolsa Escola would have halved the percentage of children in poor households not attending school; while Todd and Wolpin (2006) estimate that incremental increases in transfer size in Mexico would have diminishing effects on school attainment.

$45 scholarship and no scholarship was large, the difference between the impact of a $60 scholarship and the $45 scholarship was quite small.[8]

In short, the evidence to date suggests that conditionality plays an important role in improving school enrollment and unconditional transfers are unlikely to be effective in reaching the same goal. Critically, however, all of this evidence comes from Latin American countries (Brazil, Ecuador, and Mexico). These countries are not only substantially wealthier than many countries in Sub-Saharan Africa, but also possess stronger institutional capacities to implement conditional cash transfer programs. We contrast the income levels in these countries with that in Malawi in some detail in the following section and discuss the obvious ways in which these differences may matter for the relative effectiveness of unconditional and conditional cash transfers.

2.2. Contrasting Latin America with Malawi

According to the World Development Indicators (2009), the GNI per capita (PPP, in current international US$) in 2008 was $10,070 in Brazil, $7,760 in Ecuador, and $14,270 in Mexico. In stark comparison, the same figure was $830 in Malawi. The same data source indicates that the percentage of the population living under $1.25 a day (PPP) was 3% in Mexico, 8% in Brazil, 10% in Ecuador, and 74% in Malawi for the period 2004-2005 (the latest period for which poverty figures are available for Malawi).

This large difference in per-capita incomes between the Latin America and Sub-Saharan Africa should not be underestimated as a potential cause of heterogeneity in the impact of UCT and CCT programs on schooling outcomes in these two regions. The idea that schooling rates would increase with income can emerge simply from the concavity of the utility function in consumption, as well as from any type of credit constraint in human capital investments. Hence, in poorer and/or

[8] It is worth noting that these estimates are not pure elasticities as they include the impact of the conditionality. Pure elasticity of the outcome with respect to transfer size can only be estimated by exploiting exogenous variation in unconditional transfer amounts.

credit constrained environments, we would not only expect the existing schooling rates to differ, but also the marginal effect of income on schooling to be stronger. Again, according to the World Development Indicators (2009), the net secondary school enrollment in 2007 is 77% in Brazil, 59% in Ecuador, and 72% in Mexico, while it is only 24% in Malawi.[9] The finding of a very small 'income effect' and a much larger 'price effect' in Latin America is consistent with the higher average incomes and schooling rates in the region: most of the children who drop out of school may be doing so not because their households are not able to afford to send them to school, but for other reasons. However, with its much lower initial schooling rates, we would expect to observe a stronger income effect in Malawi, making UCT programs appear relatively more attractive.

One other important difference regarding the lives of adolescent girls in Latin America and Sub-Saharan Africa is age at first marriage, which has a direct bearing on schooling decisions. Among women aged 25-49, using Demographic Health Survey (DHS) data, we find that more than 51.2% of Malawian women are married by age 18, while the same figure is 33.3% in Mexico, 31.6% in Ecuador, and 22.7% in Brazil.[10] The fertility rate among women ages 15-19 in 2007 was 135 births per 1,000 women in Malawi, compared with 83 births in Ecuador, 76 in Brazil, and 65 in Mexico (WDI, 2009). Clearly, adolescent girls in Malawi, where the legal age of marriage currently stands at 16, start childbearing and get married much earlier than their counterparts in Brazil, Ecuador, and Mexico.[11] The relative impact of CCT and UCT on school enrollment may then, in part, depend on their impact on childbearing and the incidence of marriage. We discuss this issue in more detail in Section 5.3.

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[9] These figures are not helped by the fact that students have to pay non-negligible school fees at the secondary level in Malawi. The net enrollment rates for primary school, which is free in Malawi, are much closer at 93%, 97%, 98%, and 87%, respectively.

[10] The DHS data are most recent (2004) for Malawi, whereas the rest of the DHS data date back to 1987 for Mexico and Ecuador, and 1996 for Brazil.

[11] Legal age of marriage in Malawi has recently been raised from 15 to 16. As this paper was being written, the parliament was debating whether to raise the legal age of marriage to 18. However, in practice, it is not uncommon for girls to attend initiation rites at a very early age and be married at 15 or younger.

3. SURVEY AND RESEARCH DESIGN

3.1. Study Setting and Sample Selection

Malawi, the setting for this research project, is a small, poor country in southern Africa. Its population of almost 14 million in 2007 is overwhelmingly rural, with most people living from subsistence farming supplemented by small-scale income-generating opportunities that are typically more available to men than they are to women. The country is poor even by African standards: Malawi's 2008 GNI per capita figure (PPP, current international $) of $830 is barely 40% of the Sub-Saharan African average of $1,991 (World Development Indicators Database, 2009).

Zomba district in the Southern region was chosen as the site for this study. Zomba is a highly populated district with high rates of school dropout and low educational attainment, characteristic of Southern Malawi. According to the Second Integrated Household Survey (IHS-2, conducted by the National Statistical Office in 2005), the biggest reason for dropout from school is financial. Hence, a cash transfer program has a good chance of reducing dropout rates in Zomba. Zomba also has the advantage of having a true urban center as well as rural areas.

Clusters in Zomba were selected from the universe of EAs produced by the National Statistics Office of Malawi from the 1998 Census. Our stratified random sample of 176 EAs consists of 29 EAs in Zomba town, 8 trading centers in Zomba rural, 111 population areas within a 16-kilometer radius of Zomba town, and another 28 EAs that are located outside of this radius. Each household in the 176 sample EAs was listed using a short two-stage listing procedure, which identified the universe of never-married females, aged 13-22.[12] The target population was then

[12] The target population of 13-22 year-old, never-married females was selected for a variety of reasons. As the study was designed with an eye to examine the possible effect of schooling cash transfer programs on the risk of HIV infection, the study focused on females, as the HIV rate among boys and young men of schooling age is negligible. The age range was selected so that the study population was school-aged and had a reasonable chance of being or becoming sexually active during the study period. Finally, a decision was made to not make any offers to girls who were (or had previously been) married, because marriage and schooling seem to be mutually exclusive in Malawi – at least for females in our study district.

stratified into two main groups: those who were out of school at baseline (*baseline dropouts*) and those who were in school at baseline (*baseline schoolgirls*).

Of these two strata, the *baseline schoolgirls*, who form 87% of the target population within our study EAs, are the subject of this paper.[13] In each EA, we sampled 75%-100% of all eligible baseline schoolgirls, where the percentage depended on age. This sampling procedure led to a total sample size of 2,915 schoolgirls in 176 EAs, or an average of 16.6 schoolgirls per EA. After subtracting a randomly selected group of 629 girls, who were assigned control status within treatment EAs to measure spillover effects, the sample in this paper consists of 2,286 schoolgirls in 161 EAs.

3.2. Research Design and Intervention

Treatment status was assigned at the EA level, so the sample of 176 EAs was randomly divided into two equally sized groups: treatment and control. The sample of 88 treatment EAs was further divided into three groups based on the treatment status of the *baseline schoolgirls*: in 46 EAs (a randomly determined share of) schoolgirls received *conditional* transfers; in 27 EAs schoolgirls received *unconditional* transfers; and in the remaining 15 EAs they received *no* transfers.[14] As the group *baseline schoolgirls* who received no treatment in 15 EAs are part of the 629 girls sampled to study spillover effects, and therefore are not the subject of this paper, we are left with a sample of 2,286 schoolgirls in 161 EAs (1,497 in 88 control EAs, 506 in 46 CCT EAs, and the remaining 283 in 27 UCT EAs).

[13] While outcomes for *baseline dropouts* were also evaluated under the broader study, the 'conditionality' experiment was not conducted among this group. As the sample size for this group is quite small (804 girls in 176 EAs – less than 5 girls per EA – in our two-year panel of individuals), dividing the treatment group into a CCT and a UCT group would yield an experiment with low statistical power. Hence, the treatment group in this stratum received CCT offers only. See Baird et al. (2009) for program impacts on this group.
[14] In these 15 EAs, only *baseline dropouts* received treatment.

From December 2007 through January 2008, offers to participate in the program were made.[15] Of the 789 schoolgirls in the baseline survey who were originally assigned to the treatment group, 6 were subsequently deemed ineligible, 15 could not be located, and one refused to participate in the program. Because we continue to code all 22 of these 'non-compliers' as treated, we effectively estimate the Intention to Treat Effect of the original treatment assignment. While the assignment of 'conditionality' status is at the EA level, meaning that treatment status with respect to conditionality does not vary within EAs, the treatment unit is the adolescent girl and not the household. This means that the study sample contains households with more than one eligible girl in the program, of whom all, some, or none may have treatment status within treatment EAs.

As part of the offer, a detailed informational sheet was given to each household that detailed the transfer amount, as well as the conditions of the contract.[16] The total transfer to the household (per treated girl) was randomly varied between \$5 and \$15 per month to estimate the elasticity of schooling outcomes with respect to transfer size.[17] In addition, the *conditional* offer sheet for secondary school CCT recipients stated that their school fees would be paid in full directly to the school.[18] In the *unconditional* treatment arm, for households with girls eligible to attend secondary schools, the monthly transfer amounts paid were adjusted upwards by an amount equal to the

[15] Due to uncertainties regarding funding, the initial offers were only made for the 2008 school year (conditional on adequate school attendance for the girls receiving the conditional transfers). However, upon receipt of more funds for the intervention in April 2008, all the girls in the program were informed that the program would be extended to also cover the 2009 school year.

[16] For examples of a conditional and an unconditional offer letter, please see Appendix A.

[17] The total transfer amount offered to the household consisted of a transfer to the parents and a separate transfer given directly to the girl. The household amount was randomly varied across EAs from \$4/month to \$10/month, with all recipients in a given EA receiving the same amount. To determine their individual transfer amounts, girls participated in a lottery, where they picked bottle caps out of an envelope to win an amount between \$1 and \$5 per month. We do not exploit the random variation in the share that was directly transferred to the girl in this paper as it falls outside the scope of this study. Our impact estimates hence represent impacts averaged over these random shares transferred to the girl.

[18] Primary schools are free in Malawi, but student have to pay non-negligible school fees at the secondary level. The program paid these school fees for students in the conditional treatment arm upon confirmation of enrollment for each term. Private secondary school fees were also paid up to a maximum equal to the average school fee for public secondary schools in the study sample.

average secondary school fees paid in the conditional treatment arm.[19] This ensured that the average transfers offered in both the conditional and unconditional EAs were identical and the only difference between the two groups was the "conditionality" of the transfers on satisfactory school attendance.

At the time of the offer, the photographs of the participant (if not taken at the time of survey) and her parent or designated guardian to receive the household payment were taken. Payments were only made to those people and one designated proxy. Adolescent girls and their parents/guardians were asked to bring such proxies to the first cash payment point for them to be identified and photographed. For the rest of the program, no one other than the girl, her parent/guardian, and the designated proxy was allowed to pick up any payments.

Recipients were informed of the location and the timing of the first monthly transfer payment during the offer stage, and about the next transfer date when they picked up each transfer. The cash payment points were chosen to take place at centrally located and well-known places, such as churches, schools, etc. For each EA, they were selected so that no recipient had to travel for more than 5 kilometers to the cash payment point. Each recipient was given a sealed envelope with her name on it.[20] After counting the amount and making sure it was correct, each recipient signed a piece of paper to acknowledge the receipt of the money.

The cash transfers took place monthly and at each meeting some basic information was collected for each sample respondent, such as who was picking up the money (girl, guardian, or proxy), how far they had to travel, etc. As part of the transfer program, monthly school attendance

[19] Because the average school fees paid in the conditional treatment arm could not be calculated until the first term fees were paid, the adjustment in the unconditional treatment arm was made starting with the second of 10 monthly payments for the 2008 school year. The average school fees paid for secondary school girls in the conditional treatment group for Term 1 was multiplied by three (for each of the three school terms) and divided by nine (the number of remaining payments in 2008) and added to the transfer received by household with girls eligible to attend secondary school in the unconditional treatment arm. The NGO implementing the program was instructed to make no mention of school fees but only explain these households that they were randomly selected to receive this 'bonus'.

[20] The young woman and the guardian are given separate envelopes, each with their own randomly assigned amount.

of all the conditional cash transfer recipients was checked and payment for the following month was withheld for any student whose attendance was below 80% of the number of days school was in session for the previous month. However, no one was ever kicked out of the program, i.e. cash transfer payments were independent of each other across months.

3.3. Household Surveys and School Surveys

The annual household survey consists of a multi-topic questionnaire administered to the households in which the sampled respondents reside. The survey consists of two parts: one that is administered to the head of the household and another that is administered to the core respondent, i.e. the sampled girl from our target population. The former collects information on the household roster, dwelling characteristics, household assets and durables, shocks and consumption. The core respondent survey provides information about her family background, her education and labor market participation, her health, her dating patterns, sexual behavior, marital expectations, knowledge of HIV/AIDS, her social networks, as well as her own consumption of girl-specific goods (such as soaps, mobile phone airtime, clothing, braids, sodas and alcoholic drinks, etc.). The baseline survey was conducted between October 2007 and February 2008, and follow-up data collection took place exactly one year later.

Following Round 2 data collection, we also conducted a school survey that visited every school in Zomba attended by any of the core respondents in our study sample, and collected data on, *inter alia*, each student's attendance and their grade progression (term by term) for the 2008 school year. In this paper, we utilize school attendance data from this survey, as well as self-reported data on school enrollment and marital status, as our main outcome variables of interest to assess the impact of the program on schooling and marriage after one year.[21]

[21] For more detail on enrollment and attendance data from the school survey, see Baird and Özler (2010).

4. ESTIMATION STRATEGY

4.1. Attrition and Balance

We began with a sample of 2,286 respondents who were in school at baseline and formed the experimental sample for our study of conditionality. Of this sample, 2,153 were tracked successfully for the follow-up household survey. When we then conducted the School Survey (described above in Section 3.3), we were able to gather information on school attendance for 2,087 of these girls, which gave us the final sample for this paper. Table I examines attrition across the treatment and control groups; in panel A, we look at attrition from the household survey, and in panel B we examine the joint attrition from household and school surveys. The results indicate that there has been no significant differential attrition moving from the baseline study sample to the sub-sample used in the final analysis here.

In Table II we test the balance of the experiment, using baseline data only for the analysis sample on which the school survey data are available.[22] The top panel of the table shows balance on household attributes, and the bottom panel on individual attributes. Overall the experiment appears well balanced over a broad range of economic, social, and behavioral outcomes; at the household level the treatment group appears to be very slightly wealthier but this difference is never significant at the 95% level. The sole strong difference in the treatment/control comparison is a lower rate of female-headed households in the treatment, and it appears to be well balanced according to all individual-level variables. There is some imbalance over age in the conditional/unconditional comparison – the conditional group is, on average, half a year younger than the unconditional group – and this difference is then reflected in the highest grade attended at baseline (older girls are naturally more advanced in their studies).

[22] The balance of the experiment has been checked for the entire baseline sample, as well as the sample for which only household survey data are available, and the results are similar.

4.2. Estimation

Conventional practice in development economics has been to analyze experimental data while controlling for important covariates. Several distinct reasons exist for this practice. First, the inclusion of covariates that explain residual variation in the outcome will increase the power of impact regressions (Miguel and Kremer 2004; Duflo, Glennerster, and Kremer 2006). Second, the use of model-based inference (rather than randomization inference) requires that we account for the randomization procedure in the analysis through the inclusion of dummies for the strata used in assigning treatment probabilities (Bruhn and McKenzie 2008). If, in addition, there is observable imbalance in the randomization, then a simple comparison of means may not provide an unbiased measure of impacts. We therefore conducted regression analysis (not shown here) predicting schooling changes in the control group, and selected a group of baseline control variables based on this regression. These controls are a set of age dummies, a household asset index, the highest grade attended at baseline, and a dummy for having started sexual activity at baseline. Our covariates consist of these variables along with indicators for the strata used to perform block randomization (Zomba Town, within 16km of the town, and beyond 16km).

Freedman (2008) provides an counter-argument to such 'regression adjustment', showing that misspecification in the use of OLS to control for covariates can lead to the introduction of bias, and even to an increase in residual variance. Although our sample size is larger than the threshold of 1,000 at which Freedman's simulations suggest the bias becomes negligible, in the spirit of this argument we present our initial impact estimates with and without baseline controls.[23]

We analyze intention-to-treat effects of the interventions using cross-sectional regressions. The regressions compare each of the two treatment arms to the control group (as well as to each other). For outcomes that we observe in a panel such as self-reported attendance, English literacy,

[23] Once we establish that the impact estimates with or without baseline controls are very similar to each other, we omit the specifications without controls from our regression tables for brevity.

and marriage, we use changes on the left-hand side and hence use a structure identical to individual fixed effects in a two-period panel. The regression-adjusted intention-to-treat impact of conditionality is thus estimated via:

$$(1) \qquad \Delta Y_i = \beta_0 + X_i\beta + T_i\gamma + C_i\delta + \varepsilon_i ,$$

where T_i is an indicator for having been offered either the CCT or the UCT and C_i is an indicator for having been offered the CCT. $\hat{\gamma}$ thus gives the ITE of unconditional treatment and $\hat{\delta}$ gives the additional impact of conditionality. The errors are clustered at the EA level to account for the 'design effect' as treatment was assigned at that level, and age- and stratum-specific sampling weights are used to make the results representative of the target population in the study area. The unadjusted regression models exclude X_i from (1). When we use cross-sectional outcomes from the school survey that are available only in follow-up, we place Y_i instead of ΔY_i on the left-hand side of (1).

5. RESULTS

5.1. Average Treatment Impacts on Schooling Outcomes

Table III presents the overall impact of the program on school enrollment and attendance, the impact under each treatment arm, and the marginal effect of 'conditionality'. In Panel A, we begin by presenting the impact of the program using self-reported school enrollment – the most commonly used outcome indicator in the literature.[24] Column (1) shows that 10.8% of students in the control group had dropped out of school after one year. The impact of the program is 4.5 percentage points, indicating that the program led to a 42% reduction in the probability of dropping

[24] The outcome indicator used here is a dummy variable that is equal to one if the core respondent answered the following question with a "yes": "Are you currently attending school, or (if school is no longer in session) were you attending school when the session was ending?" On the comparison of self-reported enrollment data with administrative data for CCT recipients from this program, see Baird and Özler (2010). Barrera-Osorio et al. (2008) compare administrative enrollment data with actual attendance data under a CCT program in Colombia.

out of school. Columns (3) and (5) show this impact separately for the conditional (3.9 percentage points) and the unconditional treatment arm (5.5 percentage points). School enrollment is higher in the unconditional treatment arm, although this difference is statistically insignificant (column (7)). Columns (2), (4), (6), and (8) show the same estimates using baseline controls and confirm that the inferences drawn from either specification are qualitatively the same.[25]

While school enrollment is an important outcome to examine, it is certainly not the only one of interest.[26] For example, actual school attendance can vary among those enrolled in school and we would expect conditional cash transfers to matter more at the attendance threshold set by the program (80% in the program under examination here). In this study, each core respondent who reported being in school at some point during the 2008 school year – regardless of her treatment status -- was asked the name of her school, her grade, and her teachers' names. Each school identified through this process was then visited by field workers to collect additional information about the respondent's attendance and progress during the 2008 school year. Panels B and C present school attendance reported by the respondents' teacher.[27] We use two outcome indicators that reflect different levels of school attendance in 2008: "attended school regularly at least one term" and "attended school regularly all three terms".[28] The results are similar to those presented in Panel A: in fact, attending school regularly **at least one term** quite closely tracks self-reported school enrollment. The impact of the program on this outcome is 4.0 percentage points (column (1)) and is

[25] The parameter estimates for treatment impacts are slightly lower in the specifications including baseline controls, but so are the standard errors, so the statistical significances of the estimates remain unchanged.
[26] See, for example, Behrman, Sengupta, and Todd (2005) for a discussion of the impact of PROGRESA in Mexico on education on multiple schooling margins.
[27] In a majority of the cases, the attendance and progress of the respondent at the school was reported by the teacher she identified during the household survey. In the rest of the cases, another teacher who knew the student or the school headmaster provided the information. See Baird and Özler (2010) for more details on the school survey and the related measurement issues.
[28] During the school survey, the teacher identified by the student was asked the following question: "Did [name] attend more often than not during term 1?" This question was then repeated for terms 2 and 3. "Attended school regularly at least one term" takes on a value of one if the teacher responded with a "yes" to **any** of these three questions, while "attended school regularly all terms" takes on a value of one if the teacher responded with a "yes" to **all** three questions.

identical for either treatment arm (columns (3), (5), and (7)). When the attendance outcome is made stricter to require regular attendance during **all three terms**, school attendance drops among all groups (Panel C). 85% of the treatment group was attending school regularly for the entire 2008 school year, compared with 79.1% of the control group. Here, the impact is slightly higher in the conditional treatment arm than the unconditional one (6.6 vs. 4.7 percentage points), but the difference is not statistically significant (columns (7)-(8)). The specifications with baseline controls in both tables again confirm that their inclusion in the analysis does not change the overall findings.

So far, using data on self-reported enrollment and attendance information collected from the teachers, we have found that the cash transfer program led to large declines in dropout and significant improvements in regular school attendance. Comparing the two treatment groups to each other, although school enrollment is slightly higher among UCT beneficiaries while regular attendance is slightly higher in the CCT group, these differences are small and never statistically significant. This is because the 'income effect' on enrollment and attendance is large for this population, while the marginal effect of the conditionality, or the 'price effect', is, at best, relatively small.

School enrollment, and even possibly regular attendance, can be limited and misleading indicators of schooling progress (Behrman and Knowles 1999). A recent paper by Schady and Filmer (2009a) suggests that a CCT program in Cambodia had no impact on learning even though the program led to large increases in school enrollment and attendance. The authors suggest that their finding can be partly explained by the effect of the program in keeping lower-ability children in school. While simply attending school can benefit adolescent girls for a variety of reasons[29], it is important to assess cash transfer programs for schooling with regard to their impact on actual

[29] See, for example, Baird et al. (2009) for the impact of this program on the sexual behavior of adolescent girls, and Baird, de Hoop, and Özler (2010) for its impact on their mental health.

learning.[30] In Table IV, we analyze the impact of the program on what is currently available to us in terms of the progress study participants made after one year: a self-reported measure of English literacy and a variable that indicates whether the teacher reported the student to have made satisfactory progress during the 2008 school year to continue to the next grade.[31] Consistent with other recent studies on the impact of CCT programs on learning, we find no indication that the program had an effect on either outcome. Furthermore, this lack of impact is equally evident for both treatment arms.

5.2. Heterogeneity of Treatment Impacts on Schooling Outcomes

(a) By transfer size

As mentioned earlier, transfer amounts to households were randomly varied as part of the experimental design of the cash transfer intervention. The lowest amount transferred to a household was \$5 per month and households received \$6, \$7, etc. up to a maximum of \$15 per month.[32] Figure I gives mean attendance rates by transfer amount and Table V shows the marginal impact of transfers above the minimum of \$5/month. Columns (1) and (3) show the average impacts of the program that were presented earlier as points of comparison. Comparing the treatment impact in column (2) with that in column (1) in Table V, we note that the impact of the program on school enrollment when the transfer size is at its lowest is approximately the same as the impact of the

[30] Baseline data collection took place towards the end of the 2007 school year for most of the study participants and the offers to participate in the cash transfer program were made just before the start of the 2008 school year. Hence, a study assessing the schooling impacts of the program after just one year has limited scope to examine grade progress because follow-up data were collected towards or at the end of the 2008 school year – i.e. the same year as the start of the program.

[31] The dummy variable for self-reported literacy in English takes the value of one if the student answered the following question with a "yes": "Can you read a one-page letter in English?" The dummy variable for grade progress takes on the value of one if the teacher answered the following question with a "yes": "Did [name] pass this grade to continue to the next grade?"

[32] As mentioned earlier, school fees for those eligible to attend secondary school in the CCT group were paid directly to the school while the same average school fee amount was given to the same households in the UCT group as a bonus, i.e. with no mention of school fees. The randomized household transfer amounts discussed here are the randomized monthly payment amounts offered to the households, and, hence, exclude the school fee payments for CCT beneficiaries and the bonus payments for the UCT recipients.

program averaged over all transfer values (3.9 percentage points and 4.5 percentage points, respectively). The parameter estimate for the total transfer size in column (2) suggests that transferring the household an additional $10/month over and above the minimum payment of $5/month (i.e. tripling the payment) would reduce the dropout rate by only one percentage point. The estimates in column (4) show that not only the elasticity of school enrollment with respect to transfer size (above $5/month) is very small, but also that it does not vary by treatment status: increasing the total transfers to the household is not more effective when they're made conditionally. Columns (5)-(8) repeat the same analysis for regular school attendance and further confirm this finding. When it comes to school enrollment and attendance for this target population in Malawi, small unconditional transfers have roughly the same impact as significantly higher conditional transfer amounts.

(b) By baseline propensity to drop out of school

Many CCT programs are targeted to poorer households or those households in which children are more likely to drop out of school. For example, means-testing was used to identify eligible beneficiaries under PROGRESA in Mexico and Bolsa Escola in Brazil. In Cambodia, all students in the transition year from primary to secondary school filled out a questionnaire that provided information on variables known to be highly correlated with the propensity to drop out of school upon completion of primary schooling and only those below a cutoff score calculated using these data were eligible to receive conditional cash transfers (Filmer and Schady 2009b). As the transfers in the Malawi experiment were intentionally not targeted so as to assess the impact of the program across the entire distribution of income, one might ask whether the lack of a 'price effect" is due to the explicit lack of targeting in the Zomba Cash Transfer Program (ZCTP).

To answer this question, we simulate a targeting exercise that is similar to that in Cambodia's CESSP Scholarship Program. Using school enrollment data among the control group, we examine the determinants of dropping out of school between baseline and follow-up within our sample.[33] Then using these estimates, we calculate a "baseline propensity to drop out of school" for each individual in our study sample. We then examine whether our findings would change if the program was targeted at baseline using this propensity score.

Table VI presents our findings. Instead of picking an ad hoc propensity score as a cutoff and examining the impact of the program for those below this cutoff, we weight our impact regressions by the baseline propensity score. Columns (1)-(2) present program impacts on school enrollment for the untargeted and the targeted sample, respectively. Unconditional cash transfers reduce the dropout rate by 11.8 percentage points – a reduction of more than 40% from the dropout rate of 27.3% in the control group (column (2)). However, this impact is similar in size to the 4.9 percentage point decline over the dropout rate of 10.8% in the control group for the entire study population (column (1)). As with the estimates in the whole sample, the effect of the conditionality on enrollment is again negative and insignificant in the targeted sample. When we analyze attendance instead of enrollment, we find that impacts in the UCT group continue to be large and significant and virtually identical to those in the CCT group (columns (3)-(6)). UCT and CCT are equally effective in improving school enrollment and attendance regardless of whether or not the program is targeted to those more likely to drop out of school at baseline.[34]

[33] These baseline variables include age dummies, household size, urban/rural status, household assets, highest grade attended, whether the girl is an orphan, whether she is sexually active, etc. A probit regression was used to estimate the relationship between these baseline characteristics and the probability of dropping out of school between baseline and follow-up.

[34] When we interact the treatment effect with the baseline propensity to drop out of school (results not shown here), we find that while the interaction effect is always positive, it is only significant (at the 10% level) for enrollment and insignificant for regular school attendance. Furthermore, the interaction with conditionality is zero.

(c) Heterogeneity by highest grade attended at baseline and age

The ZCTP targets a wide range of adolescent girls with respect to age and highest grade attended at baseline, by which program impacts could reasonably vary. Age is an important variable to consider while examining the impact of a cash transfer program on adolescent girls in Malawi, because many adolescent girls get married at a young age. Similarly highest grade attended at baseline is an important variable to consider for a variety of reasons. First, previous studies show that CCT programs have the highest impacts during the transition from primary to secondary as most dropouts happen during these grades. Schultz (2004) finds enrollment impacts of PROGRESA to be strongest in the highest year of primary school, and the Cambodian CCT program studied by Filmer and Schady (2009b) offers scholarships *only* to those in the transition year from primary to secondary school. Furthermore, secondary schools in Malawi charge (non-negligible) school fees while primary schooling is free, which severely exacerbates the dropout problem during the transition from primary to secondary schools.

Table VII shows the impact of the program on schooling by highest grade attended at baseline. We use a cutoff to split the sample into two groups with respect to highest grade attended at baseline: those who had attended Standard 7 or lower (Panel A), and those who had attended Standard 8 or higher (Panel B).[35] In Panel A, we detect no significant impact of the program on any schooling outcome among those returning to primary school. Program impacts in both the CCT (not shown in Table VII) and in UCT are very small and statistically insignificant. In comparison, program impacts among those eligible to attend secondary school are large and always statistically significant at the 5% level. The 6.9 percentage point increase in the enrollment rate (Panel B, column (1)) represents more than a 55% decline in the dropout rate among this group. As with the results

[35] Those who attended Standard 7 or lower can therefore only return to primary school during the first year of the program, while those with Standard 8 or higher are eligible to attend secondary school (assuming successful completion for those in Standard 8 at baseline).

on enrollment and attendance presented earlier in Table III, enrollment rate is slightly higher among the UCT group, regular school attendance for at least one term is equal in both treatment groups, while regular attendance during all three terms of 2008 is slightly higher among the CCT group. However, with the sample being split into successively smaller cells to examine heterogeneity of impacts, the power to test the equality of means for the parameter estimates in CCT and UCT is lower. We conclude that the average program effects presented in section 4.1 are largely due to the impact of the program on those eligible to attend secondary school at the start of the program.

Figure II presents the graphical counterpart to Table VII, displaying regular school attendance in 2008 across the distribution of the highest grade attended at baseline and by treatment group. Outcomes in the treatment groups are weakly higher than the control for students exiting all grades except 10th (Form 2). The impact of treatment appears to be larger for students with higher baseline attainment, with those in the last two years of secondary school appearing to experience particularly large impacts.

Table VIII presents the impact of the program on the same three schooling outcomes, but this time by age groups. Again, we split our sample into two groups: those aged 15 or younger at baseline (Panel A) and those 16 or older (Panel B). As the program's impact on those eligible to return to primary school is limited, we focus our analysis on those who had attended Standard 8 or higher at baseline.[36] Panel A shows that the dropout rate among adolescents eligible to return to secondary school who are 15 or younger at baseline is very low (4.2%, column (1)). Even though the point estimate for the joint treatment impact of UCT and CCT is quite large relative to this small dropout rate at 3.3 percentage points, it is not statistically significant (note that the cell size here is quite small). The findings are similar for attendance among this group. We also detect signs that the

[36] However, program impacts for by age group are very similar if all students are included in the analysis rather than just those eligible to attend secondary school. Program impacts do not vary by age group among those who had attended Standard 7 or lower at baseline. These results are available from the authors upon request.

conditional treatment is more effective among this small group of 'overachievers': CCT treatment has practically eliminated dropout among this group, and increased regular attendance (during all three terms in 2008) by more than 10 percentage points (p-value=0.115), while we detect no effect of UCT on these outcomes. However, ultimately, this is a small sub-sample of students among whom dropout rates are very low (less than a quarter of the 18.1% dropout rate among those 16 or older), so the importance of these findings in the overall picture remains relatively small.

Panel B shows the impact of the program for those adolescent girls 16 or older at baseline. We note that the overall program effects on enrollment and attendance are all large and statistically significant. The 10.0 percentage point increase in enrollment rates among the treatment group represents a reduction of more than 55% in the dropout rate. Again, as before, unconditional cash transfers seem as effective as conditional ones for this sub-sample.

We end this sub-section by summarizing that the impact of the program on schooling outcomes is concentrated on those eligible to attend secondary school at baseline. Within this group, the 'income effect' from the unconditional treatment is large among those aged 16 or older at baseline – with no additional (and sometimes negative) 'price effect' detected in the CCT arm. However, among younger girls, the 'income effect' is non-existent while we detect some impact in the conditional treatment arm. Why might the relative effects of the conditional and unconditional treatments among adolescent girls vary by age? An obvious candidate is the role of marriage for adolescent girls in Malawi. As we will show below, the 'hazard' of marriage increases significantly once a young girl reaches the age of 16. In the sub-section that follows, we examine the impact of the program on marriage rates and show that the impacts of the program on marriage differ significantly in the conditional and the unconditional treatment arms.

5.3. Treatment Effects on Marriage

(a) Income and substitution effects of the program

As described briefly in Section 2.2, adolescent girls in Malawi start childbearing and get married earlier than their counterparts in countries in Latin America. Table IX shows the probability of having ever been married in the control group by age at baseline. We can see that while a negligible share of girls aged 15 or younger at baseline have been married after one year (1.6%), this share jumps to 10% for girls aged 16 or older. The increase is quite discontinuous at 16, the legal age of marriage in Malawi.

Not only the age at which the average Malawian female gets married is lower, but marriage essentially precludes the possibility of further schooling.[37] The sample of adolescent girls and young women analyzed in this paper were all initially enrolled in school and had never been married. Among the control group, only 18% of the girls who reported being married at the follow-up interview had regularly attended school in 2008.[38] Girls who regularly attended school in 2008 and were married make up less than 1% of the control sample. In addition, out of the adolescent girls in the control group who had dropped out of school at the time of the follow-up interview, 38% of them had already gotten married *within that one year*. These figures indicate that marriage and schooling are essentially mutually exclusive in Malawi, and that for many girls marriage and cessation of schooling are simultaneous. Schooling impacts, therefore, cannot be considered in isolation from marriage impacts.

What would be the likely effects of UCT and CCT programs on the likelihood of teen marriages among adolescent girls in this context? In our study district of Zomba, matrilocality is

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[37] See, for example, this article in Nyasa Times on the debate on the legal age of marriage for girls in Malawi: http://www.nyasatimes.com/features/debate-over-recommended-marriage-age-for-girls-continues-in-malawi.html. In the article, women's rights groups argue that early marriage would end adolescent girls' chances of continuing school.

[38] Some, if not many, of these girls may have become married after the completion of the 2008 school year but before having been interviewed, making the share that was married and simultaneously attended school even smaller.

common, meaning that the married couple resides with or near the wife's family. Hence, the pecuniary benefits to a household from a daughter's marriage come not in the form of a one-time bride price that would be typical of many other settings, but as a flow of current and future support from the new husband.[39] Furthermore, many adolescent girls rely on boyfriends for support – for themselves and sometimes for their families – in this poor setting, which can also lead to pregnancy and marriage.[40] Hence, we posit that an unconditional cash transfer to the household may have a negative income effect on marriage for adolescent girls in this context.[41]

Cash transfers that are conditional on school attendance could generate an additional substitution effect whereby the marriage rate is driven down through a decrease in the relative price of schooling. In settings where the opportunity cost of schooling is foregone income from child labor or children's leisure (see, for example, Ravallion and Wodon 2000 or Bourguignon, Ferreira, and Leite 2003), CCTs have been shown to decrease these activities in favor of increased school attendance by altering relative prices. Where marriage forms this opportunity cost, we would expect to see similar impacts of the conditionality in discouraging marriage. Indeed, this effect may be even more pronounced. While an adolescent girl in Malawi can simultaneously attend school while working part time on the family farm, performing some household chores, or even enjoying some leisure, she is very unlikely to be married and attending school at the same time.

[39] 90% of the marriages in our control sample had no bride price promised by the husband, and the bride prices for the remaining 10% were symbolic. Among the promises for bride price were a small amount of cash, a few goats or chickens, some bags of rice, or kitchenware.

[40] See Luke (2003 and 2006), Poulin (2007), and Dupas (2009) among others on transactional sex in Kenya and Malawi. In Zomba, it would not be uncommon for the boyfriend of an adolescent girl to buy gifts for her family (such as a large bag of maize in the lean season) if he has the resources. Hence, even prior to marriage, adolescent girls may seek support from a boyfriend, which comes with the hazards of becoming pregnant, dropping out of school, and getting married. A positive income shock should reduce the likelihood of this sequence of events.

[41] Beegle and Krutikova (2007), discussing the possible effects of HIV/AIDS-related deaths of prime-aged adults in Tanzania, argue that such negative income shocks can prompt earlier marriages among surviving young adults if they're not earning enough income to support their consumption while at the same time their marriage would contribute resources to the household.

On the other hand, working against the idea of a negative substitution effect of conditionality on marriage is the possibility that the attendance threshold imposed by the CCT may actually be costly to the adolescent girl. If her disutility from regular school attendance is high enough, a girl who would attend school less regularly under an equivalent UCT offer may turn down the CCT offer because the required attendance *rate* is too onerous. In addition, for some girls, the conditionality itself may introduce additional stress into her environment, lowering her utility even if she is attending school above the threshold imposed by the program.[42] Either of these 'costs' imposed by the conditionality opens up the possibility that a CCT offer could actually trigger marriage by presenting the girl with an untenable schooling alternative and forcing the household into a decision at the time of the offer.[43] Under an equivalent cash transfer offer that is **not** conditional on regular school attendance, this choice is not forced on the household: the girl can stay at her parents' home and attend school as much as she likes while the household still receives the monthly cash payments from the program. The potential disutility from the conditionality itself, combined with the relevance of marriage for adolescent girls in this context, raises the possibility that a UCT would generate fewer marriages and, thus, dampen the enrollment impacts of a CCT relative to a UCT.

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[42] Recent literature on CCTs attempts to understand the strategic tensions between parents and children within a household when conditionality is imposed. In a setting of asymmetric information over actual attendance, Burstyn and Coffman (2009) find that Brazilian parents *value* conditionality as a way of controlling a behavior of the child that they are seeking ways to enforce. Payments to the parents conditional on regular school attendance by their daughter may cause them to increase pressure on her at home, making the environment more contentious and stressful for her. Consistent with this, Baird, de Hoop, and Özler (2010) examine the mental health impacts of the Malawi experiment and find that the program led to substantially elevated stress and psychological morbidity among adolescent girls in the conditional group relative to the unconditional arm. They also find that the mental health of the CCT recipients worsen when the transfer amount offered to the parents is larger, while the mental health of UCT recipients is uncorrelated with the transfers offered to the parents.

[43] This intra-household discussion of whether to take up the CCT offer or not can also lead to a revelation of the schooling preferences of the adolescent girl if they are imperfectly observed by her parents prior to the offer. Parents who wish their daughter to continue her education will support her to the best of their ability while she attends school. However, her refusal to comply with the attendance requirements of a CCT offer reveals her schooling preferences and paves the way towards her marriage. Field interviews conducted by the authors confirm the idea that the CCT offer imposes a moment of decision on the household that could accelerate the timing of marriage.

(b) Estimating impacts on marriage

The discussion above suggests that the marginal effect of the conditionality on the probability of marriage is ambiguous. We now turn to our experiment to help us resolve this issue. Table X presents the program effects on the probability of having been "ever married". As the study sample was never-married at baseline, this is equivalent to the likelihood of having gotten married during the 12 months between baseline and follow-up data collection. Column (1) shows that the control group got married at a rate of 4.8%. The unconditional treatment reduced the probability of marriage by 2.7 percentage points (or by 56%), whereas the marriage rate in the conditional group was identical to that in the control group. Column (2) shows that marriage rates are negligible among those 15 or younger at baseline, for which there is no program effect – conditional or unconditional. Column (3) makes it clear that the effect of the program on marriage is among those 16 or older and only present in the UCT group. The results confirm a large, negative income effect on marriage. Furthermore, we see a significant difference between the two treatment arms: adolescent girls who received CCT offers are as likely to get married after one year as the control group and significantly more likely than those who received UCT offers.

Table XI attempts to bring the schooling and marriage impacts of the program together in as transparent a manner as possible. This 3x3 table presents share of each treatment arm in different schooling and marriage categories at follow-up.[44] Confirming earlier regression results, we see that the UCT depresses marriage relative to the control or the CCT. Interestingly, because the CCT led to a significant increase on regular school attendance and had no detectable average impact on the probability of getting married, it caused a significant decline in the share of girls who remain unmarried but are not attending school regularly. The decline in the share of these girls among the UCT group is much smaller and insignificant.

[44] As the number of girls that got married and attended school regularly in 2008 is very small (under 1% in each group), we don't include this as a separate category. They're instead included under the "ever-married" category.

Given the lack of impact of the CCT on average marriage rates, the simplest explanation is that the conditional offer did not effect this decision at all. This would be the case if every girl intending to marry had such strong preferences for matrimony so as to be completely insensitive to even the largest conditional transfer amount. An alternative explanation is that the CCT does in fact dissuade some girls from getting married, but that it also encourages (or accelerates) marriage for others. We can distinguish these two scenarios from each other because, in the first case, the determinants of marriage should be the same in the CCT and the control groups, while they should differ in the latter.

To see which scenario is more plausible, we regress the probability of being married at follow-up on baseline attributes and then interact these with a dummy for the CCT treatment. These interaction terms are jointly significant at the 1% level, implying that different types of girls got married under the conditional transfer than in the control (see Appendix Table B.1 for the regression results). Specifically, adolescent girls not residing with their mothers are significantly more likely to get married when they receive a CCT offer than the same girls in the control group.[45] In other words, a conditional offer made to a girl with relatively weak ties to her household can actually prompt her to get married earlier and drop out of school.[46] Because the marriage rate for the CCT group is identical to that of the control group in our study sample, elevated marriage rates in this sub-sample imply that the CCT treatment must have had the more obvious substitution effect of discouraging marriage (and encouraging regular school attendance) among girls with stronger attachments to their households.

[45] Girls not residing with their mothers may be maternal orphans, being fostered, or living away from their mothers for some other reason. Beegle and Krutikova (2007) show that, in the patrilocal setting of Kagera, Tanzania, girls are more likely to get married early if they are paternal orphans. The authors attribute this finding to the loss of an income-earning adult in the household. In the matrilocal setting of Zomba, the loss of a mother has a heightened importance.
[46] Given that more than a third of the sample is not residing with their mothers and a differential CCT point estimate on their marriage propensity of .0325, this group alone should have been responsible for elevating the overall CCT sample marriage rate by roughly one percentage point.

Unconditional transfers, on the other hand, strongly discourage marriage and hence expand the fraction of girls who retain the option of continuing their schooling. Those UCT beneficiaries, who would have been married in the absence of the program, could be in either of the other two cells at follow-up: some are attending school regularly while some others are not, but remain unmarried at home. Many of the girls who are unmarried but not attending school regularly are nonetheless enrolled in school and attending school some of the time in 2008. This creates the critical link between marriage and the differential CCT/UCT impacts seen in our sample: because there were more marriages under CCT than UCT, the pool of girls that could have been enrolled in school (attending regularly or less) under CCT was smaller. Therefore the marriage channel appears to be at least partially responsible for the minimal 'price effect' observed in our sample.

6. Robustness Checks

6.1. Spillover Effects of Local Monitoring

In this study, treatment status with respect to conditionality was assigned at the EA level. One might worry that the lack of an additional impact of conditionality is due to the proximity of EAs to each other, in which case intermingling of students with different treatment statuses could have led to some contamination of each treatment.[47] In particular, if the conditional treatment spills over to affect the behavior of those receiving unconditional transfers, then this would undermine our ability to interpret the UCT impact as pure income elasticity in the standard sense.

One way of addressing this issue is to exploit the variation in treatment status across the locations at which the transfer payments were made (Cash Transfer or CT points). The CT point is

[47] In the Zomba Cash Transfer Program, the UCT beneficiaries were repeatedly told that they did not have to do anything to receive their monthly payments other than showing up at the cash transfer point. This is in contrast to the CCT beneficiaries who were informed of their attendance record monthly and penalized for attendance below the 80% threshold imposed by the program. The significant differences between CCT and UCT beneficiaries in other outcomes, such as marriage or mental health, indicate that these efforts were successful in helping program beneficiaries distinguish the terms of their contracts.

the primary interface between beneficiaries and the program, and so provides a natural place to examine heterogeneity of program impacts. The locations of the CT points were determined entirely by logistical concerns, and in many cases beneficiaries from multiple EAs were assigned to the same CT point. This variation is informative because we would expect spillovers on the UCT group to be stronger as the share of CCT beneficiaries at the CT point, for whom attendance is monitored and payments are withheld for poor attendance, increases.

To test for this spillover of monitoring intensity, we calculate the number of girls attending each CT point (which is endogenous), the number of EAs that are serviced by each CT point (as a control for the heterogeneity of treatment status at a CT point, also endogenous), and the number of conditional girls at each CT point (which is exogenous and random conditional upon the other two). Under the hypothesis that UCT beneficiaries behaved in a manner driven by the average status of their peers at the CT point, we should find higher schooling rates among UCT beneficiaries when they were exposed to more conditional girls whose school attendance levels were being monitored.

The UCT sample is small and standard errors are correspondingly large, but the evidence from Table XII fails to reject the null: unconditional girls do not appear to behave differently when there is more intense monitoring around them. Both attendance and enrollment show a slight upward trend over the intensity of monitoring, but neither this slope nor the CCT/UCT difference in slopes is significant. This is confirmed visually in Figure III, which plots raw attendance rates across the share of a CT point that is conditional (we do not show enrollment here because the spillover effect in Table XII appears to be exactly zero using this outcome). A final very simple way to test whether CT point spillover effects are driving our results is to compare the 270 'purely monitored' CCT girls for whom 100% of their CT point is conditional with the 77 'purely unmonitored' UCT girls where 100% of the CT point is unconditional. The difference in attendance rates between these two groups is less than 2 percentage points, and t-statistics on a test of the difference are around .4 whether we use a simple t-test or a regression with weights, clustering, and covariates. Given the lack of obvious spillovers from the intensity of conditionality, it appears unlikely that the mixing of conditional and unconditional girls is causing a Type II error, i.e. finding no impact of conditionality when, in fact, one exists.

6.2. Differential Impacts on Secondary School Girls

Virtually everywhere in the world that CCTs have been implemented, public education was free to both primary and secondary school pupils. Like many countries in Sub-Saharan Africa, Malawi has undertaken Universal Primary Education, abolishing school fees for grades 1-8 during the period 1991-1994 (although families are typically still required to purchase uniforms, which can remain a barrier; see Evans, Kremer, and Ngatia 2008). Secondary schools, by contrast, still charge fees, and this presents a special design issue in the administration of a cash transfer program.

Operating in an environment with school fees, a CCT program could compensate households either by adjusting the size of the transfers to the household or by paying the fees directly to the schools. To make the CCT treatment as comparable as possible to programs in other settings, it was decided that the ZCTP would pay the school fees directly to the school on behalf of each CCT beneficiary.[48] While it would have been logistically simpler to transfer these funds along with the monthly household transfer, it was felt that removing school fees more closely replicated the manner in which a scaled-up, national CCT program would naturally function.

Paying fees directly at the schools, however, created a design issue when it came to creating an unconditional treatment arm, for which intention-to-treat effects could be estimated using simple differences. Offering to pay school fees for 'unconditional' beneficiaries attending school would,

[48] In practice, the fees were either paid directly to the school after confirming enrollment at the start of each term, or if the household decided to pay the school fee on their own, they were reimbursed upon producing a receipt from the school.

obviously, constitute a conditional transfer, and yet if school fee payments were ignored, then the conditional treatment arm would be receiving higher net transfers. To ensure that the average transfer amounts were identical in each treatment arm while protecting the integrity of the unconditional treatment arm, the transfer size for all UCT girls who were eligible to return to secondary school was adjusted upwards, regardless of whether they actually attended school or not – as described in detail in Section 3.2. This adjustment, which was set to equal the average school fee in the CCT group, made treatment across the two arms budget-neutral, meaning that raw differences in outcomes could be compared across the two arms to assess relative impacts without the need to adjust for differences in transfer size.[49]

The decision to deal with this issue was made precisely to make the conditional and unconditional treatments as distinct as possible in the minds of the program beneficiaries. Nonetheless, the possibility remains that these adjusted payments for the UCT beneficiaries (the normal total household transfers varied between 700 and 2,100 Malawian Kwacha – i.e. \$5 and \$15 – per month, and the additional payment UCT beneficiaries eligible to return to secondary school was 1,000 Malawian Kwacha per month) could be influencing the differences in schooling outcomes between conditional and unconditional treatments.

We are missing the counterfactual to analyze this question experimentally, because we do not have a randomized CCT arm, in which the same payment adjustments were added to the household transfers instead of the fee payments made directly to the schools each term. We are, however, given some empirical traction on this question by the fact that primary schools do not charge fees and, therefore, the CCT/UCT difference cannot have the school fee differential in it at the primary

[49] This is true under the assumption that the effect of these payment adjustments is the same whether they're paid per month or per term.

level.[50] This means that we can examine the jump in the CCT/UCT differential that arises when a UCT beneficiary becomes eligible for secondary school – i.e. reaches Standard 8. This discontinuity does not provide clean identification of the impact of the transfer adjustment because there could be other reasons why the CCT/UCT differential may change exactly upon entering secondary school. However, it still provides an obvious place to look for signs for whether the addition of the secondary school payments may be driving down the observed impact of the conditionality.

Table XIII considers this problem in the spirit of a regression discontinuity estimator, including linear and quadratic functions of highest baseline grade and a dummy variable indicating eligibility to attend secondary school **and** being a CCT beneficiary.[51] While the negative point estimates for enrollment indicate that there is a slight decline in the relative impact of CCT moving from the primary to the secondary level (columns (1)-(2)), they are statistically insignificant. Furthermore, columns (3)-(4) show that the same double-difference is almost exactly zero when we examine regular school attendance.[52] We uncover no evidence that the adjusted payments to UCT beneficiaries eligible to attend secondary schools are responsible for the findings in this paper.

6.3. External Validity of the Results

A question that arises in any study with a focused sampling strategy is the external validity of the results. The conditionality experiment was conducted entirely within the sample of girls who

[50] We have already seen in Table VII that program effects are small at the primary level and that there is no significant difference between CCT and UCT impacts.

[51] The analysis in Table XIII is analogous to comparing the even-numbered columns on the CCT-UCT difference between the top panel of Table VII (primary) and the bottom panel (secondary), or to a visual inspection of the secondary-school differential attendance rates across the two arms in Figure I.

[52] Using the household data to assign eligibility for the higher transfer amount in the UCT predicts the true transfer amount correctly 93% of the time; to check that this 'fuzziness' in the discontinuity is not driving impacts we replicated Table XIII instrumenting for actual receipt of the higher transfer with eligibility using the household data; again the results show no differential jump in the UCT/CCT difference because of the higher payments.

were enrolled in school at baseline. Girls who had already dropped out school as of baseline were all offered CCTs, and hence we cannot estimate the additional effect of conditionality in this sample.[53]

As a final empirical extension of the results, we can reweight our sample of baseline schoolgirls to look like the sample of baseline dropouts and simulate what the impact of the conditionality experiment would have been in this population. This analysis is similar to that presented in Table VI, but rather than using a set of weights that predict future dropout in the control sample enrolled in school at baseline, we use weights based on the probability of being a school dropout at the time of the baseline. The simulation results (not shown) do not indicate a significant impact of conditionality among this group of adolescent girls, who are substantially poorer, older, and much more likely to have started sexual activity and childbearing as of baseline.

These robustness checks suggest that our main finding – that there is no additional effect of conditionality on schooling outcomes over and above an unconditional transfer of the same amount – is consistent across monitoring intensity, transfer amounts, payment regimes, and baseline schooling status. At the very minimum, we find no strong evidence of heterogeneity in our conclusion that CCTs fail to out-perform UCTs.

7. CONCLUDING REMARKS

We present the first direct experimental evidence on the impact of conditionality in cash transfer programs. Using a program designed specifically to answer this question and implemented among adolescent girls in Malawi, we find that unconditional cash transfers have strong effects on schooling rates, and that the additional impact of making these payments conditional on school attendance is surprisingly limited. These findings stand in contrast to previous non-randomized studies on the topic in Latin America. The fact that this study is the only one with experimental

[53] As mentioned before, the baseline dropouts constitute less than 14% of the study population.

identification could explain the divergent findings. However, the results are also consistent with the substantially poorer profile of Malawian households, meaning that the reason adolescent girls drop out of school may have much more to do with financial constraints than their counterparts in Latin America. Our results also indicate that the role of teen marriages cannot be overlooked in the analysis of schooling impacts in this context: while the probability of marriage is substantially reduced under the unconditional treatment, we detect no such effect in the conditional treatment group. This differential response creates a channel, new to the literature on CCT programs, through which an intervention that discourages marriage will indirectly encourage schooling.

One critique of these results could be that because the conditionality was randomized at the EA level with 46 EAs given conditional and 27 unconditional treatment, we lack the statistical power to reject meaningful differences between the treatment groups. However, an examination of the regression output presented in this paper reveals little to suggest that our statistical tests are underpowered. For example, the negative coefficient on the marginal impact of conditionality on enrollment in column (8) of Table III has a standard error of 0.0163, indicating that an impact size of 3.25 percentage points would be detected with 95% confidence. Seen relative to a UCT impact of 4.9 percentage points, this does not seem like an unreasonably large minimum impact to be able to detect.[54] Furthermore, the point estimate for the marginal impact of conditionality on enrollment is actually negative; suggesting that power alone cannot explain why we fail to find a sizeable and significant price effect. The similarity in the impacts of the conditional and unconditional treatment arms in our study comes from the fact that, unlike in previous studies, the impacts of UCT on schooling are large and significant.

[54] As a comparison, Todd and Wolpin (2006) find that the impact of a UCT payment that is close to the maximum benefit allowed under PROGRESA in Mexico would be approximately 20% of that of the average CCT payment. Bourguignon, Ferreira, and Leite (2003) find that unconditional transfers would have no effect on school enrollment in Brazil.

While the randomized assignment of treatment status was conducted at the community (EA) level and every effort was made to distinguish the terms offered to beneficiaries of the conditional and unconditional treatment arms, UCT beneficiaries were still living in a district where adolescent girls in other EAs were receiving the same benefits conditional on school attendance. It is inevitable that some beneficiaries with different treatment statuses would come in contact with each other – be it at school, at a monthly cash transfer meeting, or elsewhere. While we searched for direct evidence of such spillovers in Section 6 and found little, we would still suggest a conservative reading of our results in this respect. The unconditional treatment arm of ZCTP is perhaps somewhat akin to a UCT program accompanied by a light social marketing/information campaign that "promotes" schooling. Even this conservative interpretation of the 'income effect' presented here should be of real use to policymakers trying to design cost-effective cash transfer programs for schooling.

While this is only one study, we believe that the findings here are relevant for other developing countries, especially those in Sub-Saharan Africa where dropout and marriage rates among adolescent girls are similarly high. Regardless, similar experiments in different settings would be useful; not only to see whether the results can be replicated, but especially to see whether the impacts observed here are similar among boys or younger children.

Taken as a whole, this study suggests that the strong enrollment impacts of cash transfer programs, now well established in Latin America, may indeed generalize to Sub-Saharan Africa. Furthermore, given that the marginal impact of imposing a schooling conditionality is at best low, and that monitoring school attendance to enforce the conditionality is costly, it seems that policy-makers can consider unconditional cash transfers as a viable alternative. The implication of our results is that relatively small unconditional cash transfers targeted to adolescent girls with high-dropout propensities may have a substantial effect on delaying marriage and improving schooling rates in the African context.

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THE WORLD BANK

APPENDIX A: OFFER LETTERS

***CCT* Offer Letter**

The Zomba Cash Transfer Program (ZCTP) with funding from the World Bank, would like to offer you, ___[NAME]___, a cash transfer to help you and your family with the burdens of school attendance for the 2008 school year. By accepting this offer, in return for going to school you will be given ___[AMOUNT]___ kwacha per month. If you attend secondary school, your fees for the 2008 school year will be paid directly to your school at the beginning of each term.

The payments to you and your family will be made on a monthly basis beginning in February, and will continue for 10 months through November 2008.

You are receiving this money in order to help you return to school or stay in school. In order to receive this money you **MUST** attend school at least **80% of the days for which your school is in session**.

***UCT* Offer Letter**

The Zomba Cash Transfer Program (ZCTP), with funding from the World Bank, would like to offer you, ___[NAME]___, a cash transfer to help you and your family. By accepting this offer you will be given ___[AMOUNT]___ kwacha per month.

The payments to you and your family will be made on a monthly basis beginning in February, and will continue for 10 months through November 2008.

These monthly transfer amounts specified above are given to you as a result of a lottery. You are not required to do anything more to receive this money. You will receive this money for 10 months between February and November, 2008.

Appendix Table B.1: Determinants of Marriage

	(1)
living in urban area	-0.015*
	(0.008)
household asset index	-0.001
	(0.001)
residing with biological father	0.004
	(0.007)
residing with biological mother	0.004
	(0.006)
highest grade attended	-0.006***
	(0.002)
never had sex	-0.029**
	(0.012)
tested for HIV	0.019
	(0.013)
self-perceived risk of HIV infection is medium or high	0.008
	(0.010)
total amount spent by household on girl's consumption	-0.000
	(0.000)
household size	0.001
	(0.001)
age: 16-17	0.062***
	(0.021)
age:18 or older	0.084**
	(0.036)
Interaction with Conditional Treatment	
living in urban area	0.015
	(0.021)
household asset index	-0.005*
	(0.003)
residing with biological father	0.025
	(0.022)
residing with biological mother	-0.033***
	(0.007)
highest grade attended	0.002
	(0.003)
never had sex	-0.008
	(0.008)
tested for HIV	-0.013*
	(0.007)
self-perceived risk of HIV infection is medium or high	-0.010
	(0.007)
total amount spent by household on girl's consumption	0.000
	(0.000)
household Size	0.001
	(0.002)
age: 16-17	0.013
	(0.019)
age:18 or older	0.055
	(0.056)
Number of observations	1,881
Probability that interaction terms are jointly zero:	0.0003
	(chi-squared(12)=36.48)

The dependent variable in this probit regression takes a value of one if the individual has ever been married at follow-up. The entire study sample was never-married at baseline. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*).

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Table I: Attrition

Panel A: Panel Attrition from Household Survey

	ALL	CCT	UCT	CCT vs. UCT
Treatment	-0.011	-0.010	-0.013	-0.013
	(0.020)	(0.026)	(0.019)	(0.019)
Conditional Treatment				0.003
				(0.028)
Number of observations	2,286	2,003	1,780	2,286

Panel B: Panel Attrition from Household & School Survey

	ALL	CCT	UCT	CCT vs. UCT
Treatment	0.004	-0.012	0.034	0.034
	(0.029)	(0.029)	(0.055)	(0.055)
Conditional Treatment				-0.046
				(0.058)
Number of observations	2,286	2,003	1,780	2,286
Observations in Household panel	2,153	1,888	1,673	2,153
Final Usable number of observations	2,087	1,832	1,618	2,087

Dependant variable in **Panel A** is an indicator for attrition from the household survey, while the dependant variable in **Panel B** is an indicator for attrition from the household **and** the school survey.Weighted **total** attrition rate in the control group is 11.3%. All regressions are weighted to make the results representative of the target population in study EAs. Standard errors reported in parentheses, clustered at the EA level. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*).

Table II: Balance Tests

	Mean (Control Group)	Mean (Treatment - Control)	Mean (Conditional - Unconditional)
Panel A: Household-level variables			
Household Size	6.4	0.052	-0.353
Household Expenditures, USD monthly	74.70	15.468	17.141
Asset Index	0.641	0.568*	-0.385
Female-headed household	0.35	-0.087***	-0.012
Mobile phone access	0.62	-0.006	-0.090
Panel B: Individual-level variables			
Age	15.2	-0.156	-0.419**
Highest grade attended	7.5	-0.047	-0.687***
Maternal Orphan	0.17	0.017	0.021
Paternal Orphan	0.30	-0.037	0.025
Amount girl spends on self, USD monthly	1.30	-0.112	-0.308
Amount spent on girl by HH, USD monthly	3.81	1.266	1.858
Never had sex	0.79	0.005	-0.004
Monthly discount rate	84.2	-4.323	-14.011
Trustingness	0.32	-0.027	0.024

Mean differences statistically different than zero at 99% (***), 95% (**), and 90% (*). Standard errors (not shown here for brevity) are clustered at the EZ level in to account for the design effect. Means are weighted to make them representative of the target population in study EAs.

Balance is estimated with a simple treatment dummy, comparing to the control group. Conditionality test is based on a dummy for conditionality in a regression controlling for treatment in a comparison of the treated to the control group. Discount rate calculated from the answer to a standard discounting question on amount preferred in one month versus 500 Malawian Kwacha today, and Trustingness is the answer to the question "Generally speaking, do you believe that most people can be trusted?"

Table III: Program Impacts on Enrollment and Attendance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: School Enrollment (*self-reported*)								
	ALL		**CCT**		**UCT**		**CCT vs. UCT**	
Treatment	0.045***	0.039***	0.039**	0.033**	0.055***	0.046***	0.055***	0.049***
	(0.015)	(0.013)	(0.017)	(0.014)	(0.019)	(0.016)	(0.019)	(0.016)
Conditional Treatment							-0.016	-0.015
							(0.020)	(0.016)
Mean (control group)	-0.108	-0.108	-0.108	-0.108	-0.108	-0.108	-0.108	-0.108
Panel B: Attended school regularly at least one term in 2008 (*reported by teacher*)								
	ALL		**CCT**		**UCT**		**CCT vs. UCT**	
Treatment	0.040***	0.036***	0.041**	0.034**	0.040**	0.038***	0.040**	0.037***
	(0.014)	(0.013)	(0.017)	(0.016)	(0.017)	(0.013)	(0.017)	(0.013)
Conditional Treatment							0.001	-0.001
							(0.019)	(0.016)
Mean (control group)	0.893	0.893	0.893	0.893	0.893	0.893	0.893	0.893
Panel C: Attended school regularly all three terms in 2008 (*reported by teacher*)								
	ALL		**CCT**		**UCT**		**CCT vs. UCT**	
Treatment	0.059***	0.048**	0.066**	0.052**	0.047*	0.042	0.047*	0.039
	(0.022)	(0.021)	(0.027)	(0.024)	(0.028)	(0.028)	(0.028)	(0.027)
Conditional Treatment							0.019	0.015
							(0.033)	(0.030)
Mean (control group)	0.791	0.791	0.791	0.791	0.791	0.791	0.791	0.791
Number of observations	2,087	2,082	1,832	1,827	1,618	1,613	2,087	2,082
Baseline controls included?	NO	YES	NO	YES	NO	YES	NO	YES

Dependent variable in Panel A is the change in enrollment status between baseline and follow-up. At baseline, the entire sample was enrolled in school. Dependent variable in Panel B (C) takes a value of one if the teacher reported the student to have attended school "more often than not" for at least one term (all three terms) in 2008 and zero otherwise. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). Specifications with baseline controls include a household asset index, highest grade attended by the girl, a dummy variable for whether the girl is sexually active, as well as age and strata dummies.

Table IV: English Literacy and Grade Progress

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	English Literacy (*self-reported*)				Passed grade (*reported by teacher*)			
	ALL	CCT	UCT	CCT vs. UCT	ALL	CCT	UCT	CCT vs. UCT
Treatment	0.014	0.015	0.013	0.017	0.003	0.001	0.009	0.002
	(0.026)	(0.033)	(0.028)	(0.029)	(0.032)	(0.037)	(0.041)	(0.043)
Conditional Treatment				-0.004				0.003
				(0.034)				(0.047)
Mean (control group)	0.093	0.093	0.093	0.093	0.582	0.582	0.582	0.582
Number of observations	2,078	1,823	1,609	2,078	2,082	1,827	1,613	2,082

English literacy reports the change between baseline and follow-up in a dummy variable that takes a value of one if the student answered the following question with a "yes": "Can you read a one-page letter in English?". **Passed grade** reports the level of a dummy variable at follow-up that is equal to one if the teacher answered the following question with a "yes": "Did [name] pass this grade to continue to the next grade?" Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). All regressions control for baseline levels for a household asset index, highest grade attended by the girl, a dummy variable for whether the girl is sexually active, as well as age and strata dummies.

Table V: Heterogeneity of Impacts by Transfer Size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	School Enrollment				Attended school regularly all three terms			
	ALL		CCT vs. UCT		ALL		CCT vs. UCT	
Treatment	0.045***	0.039*	0.055***	0.046*	0.059***	0.055	0.047*	0.049
(when transfer size = $5/month)	(0.015)	(0.021)	(0.019)	(0.028)	(0.022)	(0.038)	(0.028)	(0.051)
Total transfer amount		0.001		0.002		0.001		-0.001
		(0.003)		(0.005)		(0.006)		(0.008)
Conditional Treatment			-0.016	-0.013			0.019	0.010
			(0.020)	(0.035)			(0.033)	(0.067)
Total transfer amount*Conditional Treatment				-0.001				0.002
				(0.007)				(0.011)
Mean (control group)	-0.108	-0.108	-0.108	-0.108	0.791	0.791	0.791	0.791
Number of observations	2,087	2,087	2,087	2,087	2,087	2,087	2,087	2,087

For **school enrollment,** the dependent variable is the change in enrollment status between baseline and follow-up. At baseline, the entire sample was enrolled in school. For **attended school regularly all three terms**, the dependent variable takes a value of one if the teacher reported the student to have attended school "more often than not" for all three terms in 2008 and zero otherwise. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*).

Table VI: Heterogeneity of Impacts by Baseline Propensity to Drop out of School

	(1)	(2)	(3)	(4)	(5)	(6)
	school enrollment		**attendance (at least one term)**		**attendance (all three terms)**	
	Untargeted	**Targeted**	**Untargeted**	**Targeted**	**Untargeted**	**Targeted**
Treatment	0.049***	0.118***	0.037***	0.086***	0.039	0.088*
	(0.016)	(0.038)	(0.013)	(0.032)	(0.027)	(0.047)
Conditional Treatment	-0.015	-0.040	-0.001	-0.009	0.015	-0.004
	(0.016)	(0.043)	(0.016)	(0.036)	(0.030)	(0.050)
Mean (control group)	-0.108	-0.273	0.893	0.775	0.791	0.625
Number of observations	2,082	2,082	2,082	2,082	2,082	2,082

Targeted regressions are weighted using the baseline propensity to drop out of school as weights. For **school enrollment**, the dependent variable is the change in enrollment status between baseline and follow-up. At baseline, the entire sample was enrolled in school. For **attendance at least one term (all three terms)**, the dependent variable takes a value of one if the teacher reported the student to have attended school "more often than not" for at least one term (all three terms) in 2008 and zero otherwise. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). All regressions control for baseline levels for a household asset index, highest grade attended by the girl, a dummy variable for whether the girl is sexually active, as well as age and strata dummies.

Table VII: Heterogeneity of Impacts by Highest Grade Attended at Baseline

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Impact among students eligible to attend *primary* school						
	school enrollment		**attendance (at least one term)**		**attendance (all three terms)**	
	ALL	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**
Treatment	0.019	0.013	0.017	0.020	0.034	0.025
	(0.016)	(0.020)	(0.015)	(0.019)	(0.023)	(0.027)
Conditional Treatment		0.010		-0.004		0.012
		(0.022)		(0.023)		(0.032)
Mean (control group)	-0.095	-0.095	0.914	0.914	0.820	0.820
Number of observations	1,231	1,231	1,231	1,231	1,231	1,231
Panel B: Impact among students eligible to attend *secondary* school						
	school enrollment		**attendance (at least one term)**		**attendance (all three terms)**	
	ALL	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**
Treatment	0.069***	0.090***	0.063**	0.058*	0.080**	0.057
	(0.024)	(0.029)	(0.025)	(0.030)	(0.035)	(0.051)
Conditional Treatment		-0.034		0.010		0.037
		(0.029)		(0.028)		(0.051)
Mean (control group)	-0.124	-0.124	0.868	0.868	0.752	0.752
Number of observations	851	851	851	851	851	851

Students are defined as eligible to return to **primary** school if their highest grade attended at baseline was Standard 7 or lower and to **secondary** school if their highest grade attended at baseline was Standard 8 or higher. For **school enrollment**, the dependent variable is the change in enrollment status between baseline and follow-up. At baseline, the entire sample was enrolled in school. For **attendance at least one term (all three terms)**, the dependent variable takes a value of one if the teacher reported the student to have attended school "more often than not" for at least one term (all three terms) in 2008 and zero otherwise. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). All regressions control for baseline levels for a household asset index, highest grade attended by the girl, a dummy variable for whether the girl is sexually active, as well as age and strata dummies.

Table VIII: Heterogeneity of Impacts by Age (eligible to return to *secondary* school)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Impact among students aged *15 or younger* at baseline						
	school enrollment		**attendance (at least one term)**		**attendance (all three terms)**	
	ALL	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**
Treatment	0.033	0.002	0.018	0.019	0.066	-0.026
	(0.029)	(0.044)	(0.042)	(0.048)	(0.056)	(0.081)
Conditional Treatment		0.045		-0.002		0.133
		(0.039)		(0.056)		(0.090)
Mean (control group)	-0.042	-0.042	0.941	0.941	0.831	0.831
Number of observations	273	273	273	273	273	273
Panel B: Impact among students aged *16 or older* at baseline						
	school enrollment		**attendance (at least one term)**		**attendance (all three terms)**	
	ALL	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**	**ALL**	**CCT vs. UCT**
Treatment	0.100***	0.129***	0.100***	0.085**	0.092*	0.096
	(0.029)	(0.030)	(0.034)	(0.038)	(0.049)	(0.062)
Conditional Treatment		-0.055		0.028		-0.006
		(0.040)		(0.036)		(0.067)
Mean (control group)	-0.181	-0.181	0.817	0.817	0.697	0.697
Number of observations	578	578	578	578	578	578

Students are defined as eligible to return to **secondary** school if their highest grade attended at baseline was Standard 8 or higher. For **school enrollment**, the dependent variable is the change in enrollment status between baseline and follow-up. At baseline, the entire sample was enrolled in school. For **attendance at least one term (all three terms)**, the dependent variable takes a value of one if the teacher reported the student to have attended school "more often than not" for at least one term (all three terms) in 2008 and zero otherwise. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). All regressions control for baseline levels for a household asset index, highest grade attended by the girl, a dummy variable for whether the girl is sexually active, as well as age and strata dummies.

Table IX: Probability of Marriage by Age at Baseline

Age at baseline	Ever-married at follow-up	Number of observations
13	0.3%	212
14	1.3%	260
15	3.4%	289
15 or under	**1.6%**	**761**
16	8.0%	211
17	10.7%	184
16-17	**9.3%**	**395**
18	13.7%	95
19	7.5%	66
20	12.3%	30
21	7.0%	13
22	33.3%	3
18 or older	**11.3%**	**207**
Total	**4.8%**	**1363**

The entire study sample was never-married at baseline. Means are among the control group and are weighted to make them representative of the target population in study EAs.

Table X: Program Impacts on Marriage

	(1)	(2)	(3)
	ALL	**15 or under**	**16 or older**
Treatment	-0.027**	0.005	-0.057***
	(0.013)	(0.011)	(0.019)
Conditional Treatment	0.027**	-0.009	0.064**
	(0.013)	(0.011)	(0.027)
Mean (control group)	0.048	0.016	0.100
Number of observations	2,082	1,163	919

Dependent variable is the change between baseline and follow-up for the dummy variable that takes on a value of one if the individual has ever been married. The entire study sample was never-married at baseline. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). All regressions control for baseline levels for a household asset index, highest grade attended by the girl, a dummy variable for whether the girl is sexually active, as well as age and strata dummies.

Table XI: Marital Status and Regular School Attendance at Follow-up

	(1)	(2)	(3)
	Control	CCT	UCT
Ever married	4.78	4.43	1.73
Never married and **not** attending school regularly	17.03	10.62	15.05
Never married and attending school regularly	78.19	84.96	83.22
TOTAL (%)	100.00	100.00	100.00
TOTAL (N)	1,111	646	329

Table XII: Spillover Effects of Local Monitoring

	(1)	(2)	
	school enrollment	**attendance (all three terms)**	Weighted mean of covariate
# of Conditional Girls at CT Point	0.0004	0.003	17.2
	(0.0011)	(0.0027)	
# of Baseline Schoolgirls at CT Point	0.0002	-0.0017	46.6
	(0.0005)	(0.0017)	
# of EAs served by CT Point	0.001	0.002	5.1
	(0.0014)	(0.0035)	
Observations	255	255	

The regressions are among the **unconditional treatment group only**. Dependent variable in column (1) is the change in enrollment status between baseline and follow-up. At baseline, the entire sample was enrolled in school. Dependent variable in column (2) takes a value of one if the teacher reported the student to have attended school "more often than not" for all three terms in 2008 and zero otherwise. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). Baseline controls include a household asset index, highest grade attended by the girl, a dummy variable for whether the girl is sexually active, as well as age and strata dummies.

Table XIII: Differential Impacts on Secondary School Girls

	(1)	(2)	(3)	(4)
	school enrollment		**attendance (all three terms)**	
Secondary * Conditional	-0.0557	-0.0476	-0.0017	0.0108
	(0.0359)	(0.0368)	(0.0580)	(0.0596)
Secondary * Treatment	0.082	0.0766	0.0489	0.0406
	(0.0357)**	(0.0361)**	(0.0595)	(0.0591)
Conditional	0.0132	0.0091	0.0232	0.0169
	(0.0215)	(0.0218)	(0.0320)	(0.0325)
Treatment	0.0094	0.0125	0.0121	0.0168
	(0.0200)	(0.0199)	(0.0281)	(0.0281)
Secondary	-0.0268	-0.0247	-0.0692	-0.0659
	(0.0268)	(0.0261)	(0.0430)	(0.0417)
Observations	2,082	2,082	2,082	2,082
Control for Baseline Highest Grade:	Linear	Linear, Quadratic	Linear	Linear, Quadratic

Dependent variable in columns (1 & 2) is the change in enrollment status between baseline and follow-up. At baseline, the entire sample was enrolled in school. Dependent variable in columns (3 & 4) takes a value of one if the teacher reported the student to have attended school "more often than not" for all three terms in 2008 and zero otherwise. Robust standard errors, clustered at the EA level, are in parentheses. All regressions are weighted to make the results representative of the target population in study EAs. Parameter estimates statistically different than zero at 99% (***), 95% (**), and 90% (*). Baseline controls include a household asset index, a dummy variable for whether the girl is sexually active, as well as age and strata dummies, along with Linear or Linear plus Quadratic controls for highest grade attended.

Figure I: Regular School Attendance in 2008 by Transfer Amount

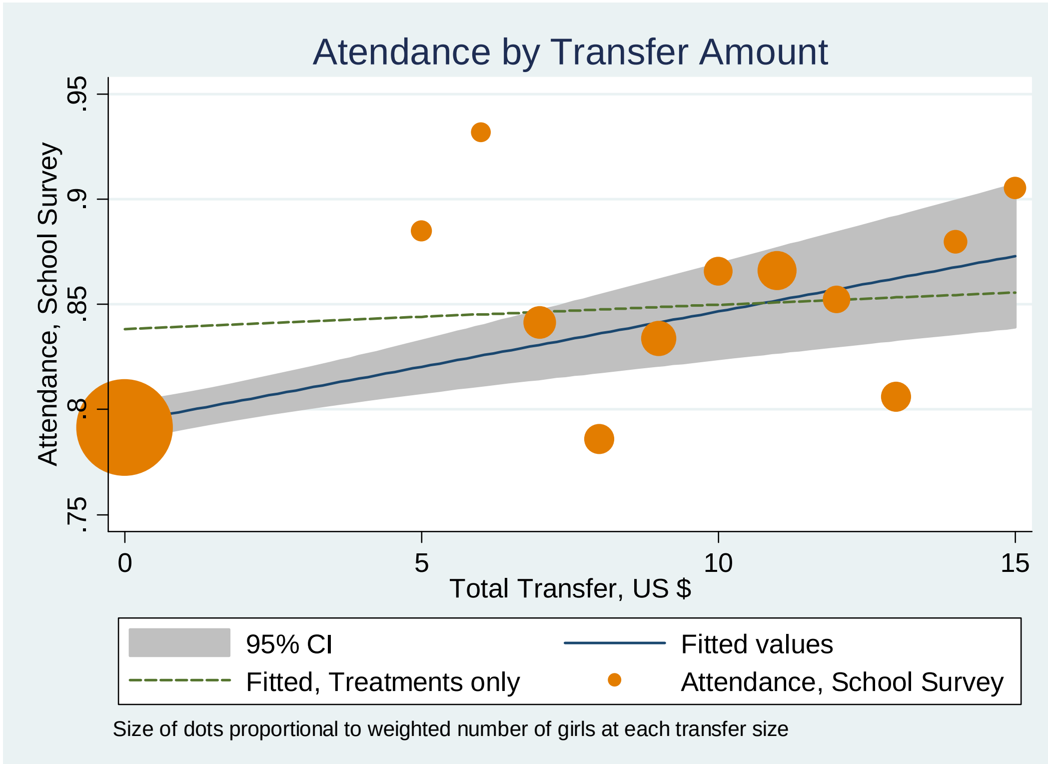

Figure II: Regular School Attendance in 2008 by Highest Grade Attended at Baseline

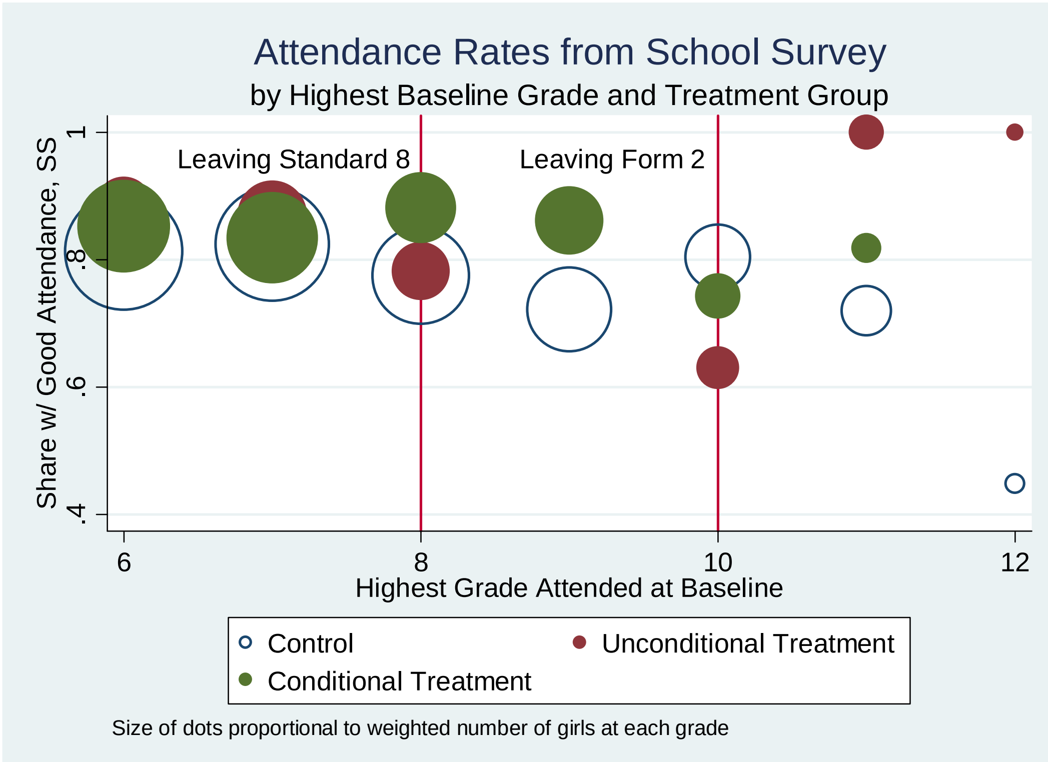

Figure III: Regular School Attendance in 2008 by Fraction of CT Point Conditional