# Designing Cost-Effective Cash Transfer Programs To Boost Schooling among Young Women in Sub-Saharan Africa

Sarah Baird, Craig McIntosh, and Berk Özler[1]

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Abstract

As of 2007, 29 developing countries had some type of Conditional Cash Transfer (CCT) program in place, with many others planning or piloting one. However, the evidence base needed by a government to decide *how* to design a new CCT program is severely limited in a number of critical dimensions. We present one-year schooling impacts from a CCT experiment among teenage girls and young women in Malawi, which was designed to address these shortcomings: conditionality status, size of separate transfers to the schoolgirl and the parent, and village-level saturation of treatment were all independently randomized. We find that the program had large impacts on school attendance: the *re-enrollment rate* among those who had already dropped out of school before the start of the program increased by two and a half times and the *dropout* rate among those in school at baseline decreased from 11% to 6%. These impacts were, *on average,* similar in the conditional and the unconditional treatment arms. While most schooling outcomes examined here were unresponsive to variation in the size of the transfer to the parents, higher transfers given directly to the schoolgirls were associated with significantly improved school attendance and progress – but only if the transfers were *conditional* on school attendance. We find no spillover effects within treatment communities after the first year of program implementation. Policymakers looking to design cost-effective cash transfer programs targeted towards young women should note the relative insensitivity of these short-term program impacts with respect to conditionality and total transfer size.

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[1] Baird is at George Washington University, McIntosh at UC San Diego, and Özler at the World Bank. Please send correspondence to bozler@worldbank.org. We gratefully acknowledge funding from the Global Development Network, the Bill and Melinda Gates Foundation, the Knowledge for Change Trust Fund (TF090932), World Development Report 2007 Small Grants Fund (TF055926), and Spanish Impact Evaluation Fund (TF092384). The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not nec essarily represent the views of the International Bank for Reconstruction and Development or the World Bank.

## 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. 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." (World Bank, 2009) 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 evaluations consider a policy 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.

The question of whether the observed effects of a CCT program are a result of the "income effect" associated with the transfer or the "price effect" from the condition remains largely unanswered. This issue is of much more than academic interest, because it has direct implications on program design. 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 yet been conducted anywhere. The evidence that can be gleaned so far is either from model-based simulation exercises (e.g. Bourguignon, Ferreira, and Leite, 2003; Todd and Wolpin, 2006) or from interventions with implementation glitches in Mexico (De Brauw and Hoddinot, 2007) and Ecuador (Schady and Araujo, 2008).

With regards to transfer size, while "…the key parameter in setting benefit levels is the size of the elasticity of the relevant outcomes to the benefit level" (World Bank, 2009, pp. 182), random variation in transfer size among program participants is rarely, if ever, observed. Nor has the related issue of to whom the transfer should be made been studied extensively. While there are a few studies examining the effect of making the transfer to the mother or the father, we know of only two impact evaluations assessing the impact of splitting the transfer payments between the student and his/her parent/guardian.[2]

Finally, impact evaluations of CCT programs are non-existent for Sub-Saharan Africa (SSA).[3] The fact that much of what is known about the effectiveness of CCT programs is based mainly on evaluations in Latin America (and a few countries in Asia) is not encouraging for those hoping to implement them in SSA, given that these countries are significantly poorer and may have weaker institutions.

This paper describes the schooling impacts from the first year of a two-year randomized intervention in Malawi that provides cash transfers to current schoolgirls (and young women who have recently dropped out of school) to stay in (and return to) school. While we solely focus on schooling outcomes in this paper – namely *enrolment* and *literacy in English* – we study the impacts of the program on changes in other outcomes, such as sexual behavior, in other related papers (see, e.g. Baird, McIntosh, and Özler, 2009a). Through the use of our multifaceted research design to evaluate the impact of this intervention for a wide variety of outcomes, we hope to contribute to the literature and inform policymakers as to which combination of contract parameters might allow CCT programs to deliver the largest impacts per dollar spent in the Sub-Saharan African context.

The research design features multiple overlapping layers of randomized contract variation devised to allow us to start filling the knowledge gaps in the literature that are outlined above. First, 176 enumeration areas (EA) were randomly sampled out of a total of 550 EAs using three strata in the study district of Zomba.[4] Each of these 176 EAs were then randomly assigned treatment or control status. Furthermore, each treatment EA was randomly assigned to receive either *conditional* or *unconditional*

[2] These are Ashworth et al. (2002), who study a program in the UK, and Berry (2009), who uses a randomized evaluation in India.

[3] An exception is the "Going to Scale" program in South Africa, whose economy resembles that of a Latin American country much more than a poor sub-Saharan African one.

[4] The three strata are urban, rural areas near Zomba Town, and rural areas far from Zomba Town. Rural areas were defined as being near if they were within a 16 KM radius of Zomba Town. Note that we did not sample any EAs in TA Mbiza due to safety concerns (112 EAs).

transfers. This experimental design allows the study team to isolate the impact of the *conditionality* on various outcomes of interest.

Second, two separate transfers were made to the household in which the target beneficiary lived. The household (or parental) transfer size was randomized *across* treatment EAs, and the size of the transfer that was made directly to the girl was independently randomized at the individual level *within* EAs, which allows us to estimate the elasticity of outcomes with respect to transfer size. 'Pure' income elasticity can be estimated by restricting the analysis to only those receiving *unconditional* transfers. In addition, because these two transfer sizes are independently randomized, we have experimental identification over the impact of the *split* of the transfers, conditional on the total transfer size. Therefore we can investigate whether, for a given cost, impacts can be improved by altering the recipient of the transfer. Finally, the percentage of girls assigned to the treatment group was randomized at the EA level, and hence our survey includes a group of randomly selected 'within village controls' who did not receive the treatment. Using this second control group, we can exploit the direct randomization of treatment saturations to test for the presence of spillover effects within villages.

The CCT program started at the beginning of the Malawian school year in January, 2008 and will continue for two years until November, 2009. Baseline data collection was conducted in the autumn of 2007 and follow-up data collection to assess the one-year impact of the program was conducted in the autumn of 2008. Our results are based on the first two rounds of a household survey covering 3,805 girls and young women, between the ages of 13 and 22, and never-married as of baseline. Our sample was randomly drawn (using the above eligibility criteria) using data from a full listing exercise, meaning that we are able to weight our estimates to represent the entire eligible population in the 176 study EAs.[5] We implemented a baseline survey after the listing exercise and before the selection of treatment status, and our follow-up survey comes at the end of the first school year in which the program operated. The reader

[5] We choose not to weight our estimates to represent all of Zomba given that our sampling strategy explicitly sampled very few EAs further than 16km from Zomba city and no EAs from TA Mbiza.

should note that these are therefore *one-year impacts* of the program and may change with the longer duration of treatment.

With the above caveat in mind, we find strong average impacts of the program on school enrolment, but only small marginal impacts from increased transfer size or conditionality. However, there is some evidence that schooling outcomes improve as the transfer amount given directly to the girl/young woman increases, but *only among the conditional* transfer group. Spillover effects are non-existent at the end of Year 1. We present our results by first discussing the issues regarding CCT design in Section 2, and then laying out the study design in Section 3. Section 4 presents the average impacts of the program as well as those for each source of contract variation. Section 5 concludes.

## 2. CONTRACT DESIGN IN CCT PROGRAMS

### *2.1. Disentangling the 'price effect' from the 'income effect' in CCT Programs*

From a program design standpoint, it is important to know whether the impact of CCT programs are a result of the income effects associated with the transfers, the price changes implicit in the condition, or both. Conducting randomized pilots to answer this question can be time consuming and expensive, so experimental evidence is not 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 effect of the conditionality on school enrolment 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 (2007) and Schady and Araujo (2008) both find that school enrolment was significantly lower among those who thought that the cash transfers were unconditional.

Ex-ante program evaluations provide further evidence that the impacts on various schooling related outcomes 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 enrolment; while Todd and Wolpin (2006) report that the impact of unconditional transfers on attainment would be only 20% of that of conditional transfers.

Finally, there is 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 would 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.

The evidence presented above points to the notion that the conditions under which cash transfers are made to households are important and that unconditional transfers are likely to be less effective in obtaining the desired behavioral change – at least for the outcomes examined in the literature. 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.

### *2.2. Elasticity of relevant outcomes to the benefit levels*

As World Bank (2009) convincingly argues, the key parameter in setting the benefit levels in CCT programs is the size of the elasticity of the relevant outcomes to the benefit levels. Several programs, such as PROGRESA in Mexico or PRAF in Honduras, set their transfer sizes to cover the opportunity costs of attending school and, in the case of the latter, direct costs of schooling.

To our knowledge, there are no CCT programs under which the transfers are randomly varied across beneficiary households to estimate how school enrolment, attendance, or attainment may improve as the transfer amount is increased. Again, with one exception (discussed below), the only evidence we have comes from structural models that 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. It is worth noting that these estimates are not pure elasticities as they incorporate the impact of the conditionality of the amount transferred. Pure elasticities can only be estimated by varying unconditional transfer amounts.

One study that addresses the issue of the impact of transfer size on enrolment is from Cambodia (Filmer and Schady, 2009). The program offered two different transfer amounts to students based on their poverty status at baseline. Using a regression discontinuity design, the authors find that while the difference between the impact of a $45 scholarship and no scholarship was large, the difference between the impact of a $60 scholarship and the $45 scholarship was quite small. Their findings are consistent with those from structural models reported above.

*2.3. Does it matter to whom the cash transfers are made?*

Almost all CCT programs make their payments to women (mothers or other female guardians) in the household. While there are a few studies that point to improved outcomes as a result of the transfer being made to women in the beneficiary households, there is virtually no evidence from developing countries on whether making some of the payment to the young target beneficiary can improve outcomes.

Lundberg, Pollak, and Wales (1997) provide evidence that when transfers were made to women in a British transfer program, a larger fraction of household expenditures were made to purchase children's clothing. The evaluation of another British pilot program (Education Maintenance Allowance) found that impact on enrolment doubled when the payment was made to the young person (Ashworth et.

al. 2002). Berry (2009), examining the assignment of incentives to the parent or the child on a specific reading goal in India, finds that the incentives to the child may be more effective if the children have less productive parents and lower initial test scores. Finally two programs, in Bangladesh and Colombia, make transfers to a Bank account in the student's name, which can be accessed by the student later, but no evaluation of this aspect of these programs is available. It seems plausible that paying at least a portion of the transfers to young people – either directly or into a savings account – may be worth considering.

Pilot programs in Burkina Faso, Morocco, and Yemen all have randomized treatment arms for making transfers to women/mothers vs. men/fathers. To our knowledge, no study other than the one presented in this paper explicitly evaluates the effect of making some of the payment (in the context of a cash transfer program conditional on school attendance) to the young person (student) vs. the parents/guardians.

## 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: the GNI per capita (PPP, current international $) is $750 in 2007, compared to an average of $1,870 for sub-Saharan Africa (World Development Indicators Database, 2008).[6]

Zomba district in the Southern region was chosen as the site for this study for several reasons. First, it has a large enough population within a small enough geographic area rendering field work logistics easier and keeping transport costs lower. Zomba is a highly populated district, but distances from the district capital (Zomba Town) are relatively small. Second, characteristic of Southern Malawi, Zomba

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[6] Using the Atlas method, The GNI per capita (in current US$) in Malawi is 250 in 1997, compared with 952 in sub-Saharan Africa as a whole.

has a high rate of school dropouts and low educational attainment. According to the Second Integrated Household Survey (IHS-2), the biggest reason for dropout from school is financial (National Statistical Office, 2005).

Third, unlike many other districts, Zomba has the advantage of having a true urban center as well as rural areas. As the study sample was stratified to get representative samples from urban areas (Zomba town), rural areas near Zomba town, and distant rural areas in the district, we can analyze the heterogeneity of the impacts by urban/rural areas. Finally, while Zomba in particular and the Southern region of Malawi more generally, are certainly different in some respects than Central and Northern Malawi, they are not entirely dissimilar. As mentioned above, Malawi is one of the poorest countries in the world with one of the highest rates of HIV prevalence, so any differences are relative.

EAs in Zomba were selected from the universe of EAs produced by the National Statistics Office of Malawi from the 1998 Census. The sample of EAs was stratified by distance to the nearest township or trading centre. Of the 550 EAs in Zomba 50 are in Zomba town and an additional 30 are classified as urban (township or trading center), while the remaining 470 are rural (population areas, or PAs). Our stratified random sample of 176 EAs consists of 29 EAs in Zomba town, 8 trading centers in Zomba rural, 111 population areas within 16 kilometers of Zomba town, and 28 EAs more than 16 kilometers from Zomba town.

After selecting sample EAs, all households were listed in the 176 sample EAs using a short two-stage listing procedure. The first form, Form A, asked each household the following question: ‘Are there any never-married girls in this household who are between the ages of 13 and 22?’ This form allowed the field teams to quickly identify households with members fitting into our sampling frame, thus significantly reducing the costs of listing. If the answer received on Form A was a ‘yes’, then Form B was filled to list members of the household to collect data on age, marital status, current schooling status, etc. From this we could categorize the target population into two main groups: those who were out of school at baseline (*baseline dropouts*) and those who were in school at baseline (*baseline schoolgirls*).

These two groups comprise the basis of our sampling frame. In each EA, we sampled all eligible dropouts and 75%-100% of all eligible school girls, where the percentage depended on the age of the baseline schoolgirl.[7] This sampling procedure led to a total sample size of 3,805 with an average of 5.1 dropouts and 16.7 schoolgirls per EA.[8]

### *3.2. Research Design and Intervention*

Out of these 3,805 young women, 1,225 girls in 88 EAs were sampled to receive the cash transfer intervention, receiving either *conditional* or *unconditional* cash transfers.[9] In each of the 88 treatment EAs, those who had dropped out of school as of baseline (hereafter, *baseline dropouts*) were always treated *conditionally*.[10] We refer to the stratum of treated baseline dropouts as T1, with corresponding control C1. The *baseline schoolgirls (*eligible to return to Standard 7-Form 4) are much more numerous, and were subject to a more complex research design.[11] The sample of treatment EAs was randomly divided into three groups based on how the sample of baseline schoolgirls was treated: in 46 EAs (a randomly determined share of) schoolgirls received *conditional* transfers (T2a); in 27 EAs schoolgirls received *unconditional* transfers (T2b); and in the remaining 15 EAs they received *no* transfers.

Within those EAs where schoolgirls received either conditional or unconditional transfers, we further randomly selected within-village controls. The randomly determined shares of schoolgirls in the

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[7] These percentages were lower for urban areas since the populations are much higher.

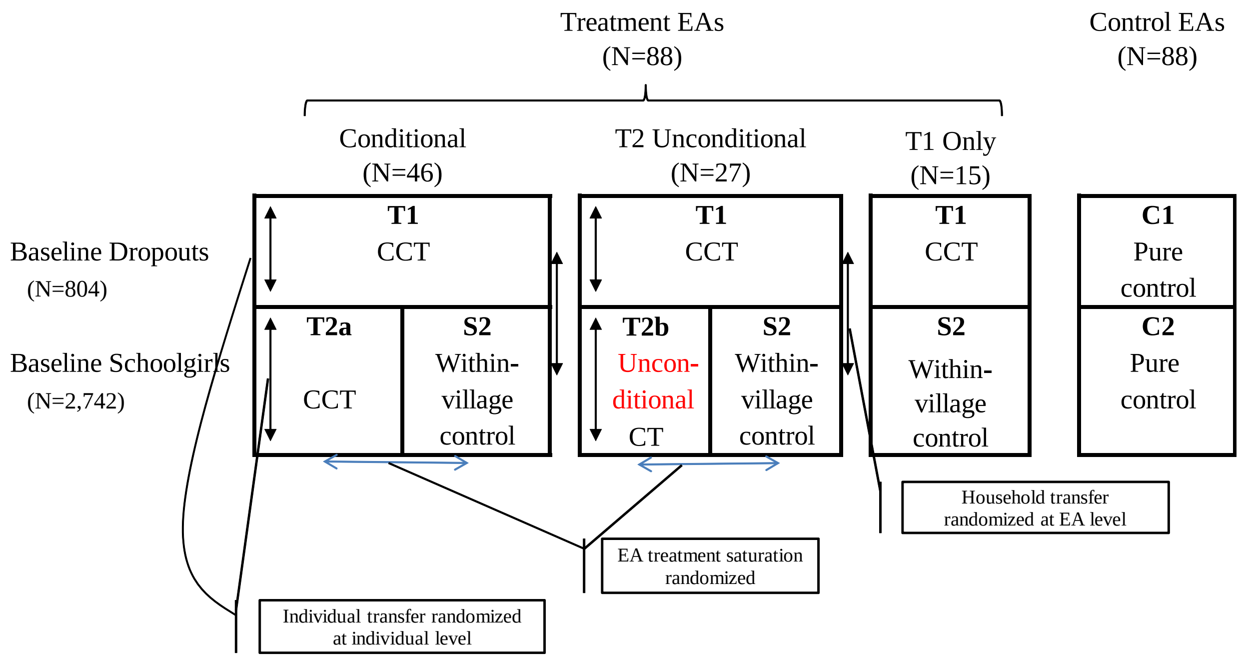
[8] We chose to target these two groups separately to ensure that we had a significant number of dropouts in our sample. Treating all dropouts allows us to focus on a subpopulation whose schooling rates are extremely sensitive to transfers.

[9] 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 cover the 2009 school year and that they could stay in the program upon satisfactory performance (again, only in terms of school attendance in 2008).

[10] The treatment arm that experimentally tests the impact of the *conditionality* was applied only in the stratum with baseline schoolgirls and not among the baseline dropouts. The main reason was that, given the small number of baseline dropouts who were eligible for the program, splitting the baseline dropouts into conditional and unconditional treatment groups would have low power to precisely identify treatment effects.

[11] The reason for this grade restriction was so that the treated girls could receive a certificate within two years – the proposed duration of the program. The majority of dropouts also fit within this grade range.

study sample that were treated were 33%, 66%, or 100%, and Figure 1 plots the intended saturations from the research design against the observed treatment saturations measured through the household surveys. We refer to the within-village controls as S2, and the 15 EAs, where no schoolgirls received transfers could be considered a special case where the share was set to zero. In those EAs, the only individuals treated were *baseline dropouts*. The sample of untreated schoolgirls in treatment villages allows us to identify any spillover effects of the program. This same universe of would-be-eligible baseline schoolgirls is also identified in the control communities, denoted by C2. A graphic illustration of the research design is presented below:




From December 2007 through January 2008, offers to participate in the program were made. Of the 1,225 girls in the baseline survey who were originally assigned to the treatment, 32 were subsequently deemed ineligible, 24 could not be located, and one refused. Because we continue to code all 57 of these 'non-compliers' as treated, we effectively estimate the Intention to Treat Effect of the original treatment assignment. The offer consisted of a household transfer and a transfer directly to the girl, as well as full payment of school fees for girls in secondary school.[12] The household amount was randomly varied

[12] Students have to pay school fees at the secondary level in Malawi, but not at the primary level.

across EAs from $4/month to $10/month, with all recipients in a given EA receiving the same amount. To determine the individual transfer amount, girls participated in a lottery where they picked bottle caps out of an envelope to win an amount between $1/month and $5/month. Having the girls choose their own amount both helped involve them in the process and insured that they viewed the outcome of the lottery as fair.

As part of the offer, a detailed informational sheet was given to each household that detailed the quantity of transfers that each household and girl would receive, as well as the conditions of the contract. 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.[13] The contract was then signed by both the recipients (guardian and core respondent) and the firm delivering the funds.

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 are only made to those people and one designated proxy. Recipients and parents are 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 recipient, the parent, and the designated proxy is allowed to pick up any payments.

Recipients are 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 pick up their previous transfer. The cash payment points are chosen to take place at centrally located and well-known places, such as churches, schools, etc. For each EA, they are selected so that no recipient has to travel for more than 5 kilometers to the cash payment point. Security guards are at hand to make sure that the money is secure and each recipient is given a sealed envelope with her name on it.[14] After counting the amount and making sure it is correct, each recipient signs to acknowledge the receipt of the money. In between payment dates, the

[13] The transfer amounts offered to guardians of girls who were eligible to attend secondary schools was adjusted upwards by an amount equal to the average secondary school fees without any mention of school fees. 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.

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

implementing agency collects attendance records for all the conditional students in the program to make sure that they are complying with the program requirements and attending school.

The cash transfers take place monthly and at each meeting some basic information is collected for each sample respondent, such as who is picking up the money (girl, guardian, or proxy), how far they had to travel, etc. As part of the transfer program, monthly school attendance of all the conditional cash transfer recipients is checked and payment for the following month is withheld for any student whose attendance was below 75% of the number of days school was in session for the previous month. However, no one is ever kicked out of the program, i.e. cash transfer payments are independent of each other across months.

### *3.3. Household Surveys*

The annual household survey consists of a multi-topic questionnaire administered to the households in which the selected sample 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.). Community characteristics are also collected in a separate short community questionnaire. This paper utilizes data from the baseline survey (October 2007-February 2008) and follow-up data (October 2008-February 2009) to analyze the one-year impact of the program on self-reported school enrolment and literacy.

## 4. RESULTS

Table 1 provides basic summary statistics that allow for a comparison of the baseline schoolgirls and the baseline dropouts. We see clearly that *baseline dropouts* were older, poorer, less educated, and more likely to come from female-headed households compared with *baseline schoolgirls*. Despite these differences, baseline dropouts are not located dramatically farther from the closest school and nor are they substantially more likely to have suffered from recent shocks.

Table 2a gives the number of observations by stratum, beginning from the original baseline sample and moving through the offer stage of the cash transfer program, right up to the follow-up survey. We use treatment status as originally assigned out of the baseline data for the entire analysis, because we only uncovered certain mistakes in treatment assignment through the process of attempting to make offers, and so correcting these mistakes in the treatment group only could have led to imbalance between treatment and control. Therefore our estimates should be thought of as the "Intention-to-Treat" effect of the original assignment to a treatment category.

Table 2b investigates our success at tracking individuals in the follow-up round, and the extent to which our sample attrition is balanced over the research design. We located more than 93% of the overall study sample; 90% of baseline dropouts and 94% of baseline schoolgirls. The regressions investigating differential attrition across treatment and control show that tracking was balanced perfectly across treatment and control groups.

In order to gauge the quality of the randomization itself, Table 3 uses the final analysis sample to perform balance tests for a battery of baseline covariates over every dimension of the randomization (overall balance, balance within dropouts and schoolgirls, conditionality, transfer amounts, and spillover saturations). These tests, like the impact tests to follow, take into account the design effects arising from the EA-level randomization by clustering standard errors at the EA level. Overall, very few violations of balance are detected; in a table that shows 49 tests for balance, three are significant at the 5% level and none at the 1% level, indicating a rejection rate in very much in line with what we expect from fully random comparisons. The one attribute that appears somewhat problematic in this table is the indicator for female-headed households, with a slightly lower treatment rate among schoolgirls and among the within-village controls, indicating the presence of some village-level heterogeneity.

### *4.1. Basic educational impacts by stratum*

To estimate causal impacts of the program, we estimate a difference-in-difference (DID) regression using individual fixed-effects, thereby explaining changes in educational outcomes with a dummy for the second round and a dummy that only switches on for the relevant treatment group. The regressions are weighted to be representative of the study EAs. Standard errors are clustered at the EA level to account for the design effect (see Bruhn & McKenzie, 2008). Results are reported in Table 4.

Self-reported school attendance displays a pronounced one-year improvement in the treatment relative to the control.[15] Both for attendance and for English literacy, baseline dropouts experience treatment effects that are larger in magnitude than baseline schoolgirls, as is made clear by Figure 2 that illustrates baseline and follow-up outcomes for school enrolment separately by both groups.[16] Treatment girls who were out of school at baseline re-enroll at rates two and a half times the control, and the treatment effect DID regression with no other controls has an R-squared of .51. Among girls who were enrolled as of baseline (i.e. *baseline schoolgirls*) treatment effects are smaller in absolute magnitude and significance, but the one-year dropout treatment effect of 4.6 percentage points still represents more than a 40% decrease in dropout from the control rate of 10.9%. Treatment effects on self-reported literacy are more muted, but still statistically significant among dropouts. Hence these results conform to a large body

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[15] The self-reported attendance variable takes the value of 'one' if the respondent answers 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?' The results are very similar if self-reports on other questions, such as school attendance during a particular term, are used in the analysis instead. Conditional on reporting attendance under this question, the number of days missed in the past two weeks is very low. Only 8% of the students who report being in school also report having missed more than 20% of school days in the past two weeks (or the two weeks prior to school closing date).

[16] The English literacy variable takes the value of 'one' if the respondent answers the following question with a "Yes": 'Can you read a one-page letter in English?' An educational testing component is being developed to independently assess learning for the entire study sample during second follow-up data collection at the end of 2009.

of evidence showing that the dramatic influence of CCT programs on attendance is not accompanied by similarly large improvements in *learning.*[17]

Having established the treatment effects for the average individual, we want to understand how treatment effects differ according to the highest grade completed at baseline. We may expect strongly differential effects depending on whether the individual was within two years of a ‘transition’ year (i.e. a grade at the end of which a diploma is received) because the marginal value of additional schooling without an additional diploma may be significantly lower. Schultz (2004) finds enrolment impacts of *Progresa* to be strongest in the highest year of primary school, and the Cambodian program studied by Filmer and Schady (2009) offers treatment *only* to those in the transition year from primary to secondary school. Therefore, the evidence in the existing literature that CCTs can improve enrolment in non-transition years is scant at best.

In Figures 3a and 3b we plot follow-up schooling attendance by highest grade attended at baseline for dropouts and schoolgirls, respectively. While it is true that the effects are large and relatively constant for those whose highest grade attended at baseline was between Standard 8 and Form 3 for both groups, we also see large enrolment impacts for baseline schoolgirls throughout the distribution of grades. On the other hand, while the treatment effects are very large for baseline schoolgirls between Standard 8 and Form 2, but muted otherwise. These impacts suggest that CCTs can generate impacts across a much broader range of baseline schooling status when individuals who had already dropped out as of baseline are included and examined.

Figures 4a and 4b repeat the above exercise, but use reported changes in English literacy rather than attendance as the outcome. Baseline dropouts re-enroll in school in grades at which literacy is low and improving quickly. A separate analysis of the changes among dropouts (not shown here) indicates

[17] World Bank (2009) finds that CCTs led to large increases in school enrolment, particularly among those with low enrolment rates to begin with. However, evidence on the impact of educational transfer programs (in kind or cash) on ‘final outcomes’ such as test scores, is not as encouraging – see, e.g., Miguel and Kremer (2004) or Glewwe, Kremer, and Moulin (2008). Filmer and Schady (2009) argue that the lack of any discernible effect of such programs on learning (despite large impacts on school enrolment) may be due to the fact that they draw lower ability students back to school.

that Standard 6 and 7 in primary school appear to be a time during which literacy actually erodes in the absence of the treatment, and it is in these grades that the largest treatment effects on literacy are seen. Among those in school at baseline, literacy is much higher and the only impacts are seen at the lowest grade levels (Standard 5 and 6) and thereafter literacy has achieved high enough levels that no upward treatment effects are detected. Put differently, the baseline dropouts return to grades at which literacy is increasing rapidly, whereas the baseline schoolgirls remain in school during grades at which literacy is already almost universal.

The impacts presented so far make use of self-reported enrolment and literacy. However, as part of this study, we also conducted an independent 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 separately for each school term. We found the self-reported attendance data to be fairly accurate, and impacts estimated using data from the school survey are qualitatively very similar to those reported here.[18] Having shown strong treatment impacts on enrolment and relatively muted impacts on 'learning' (in the form of English literacy), both of which are based on self-reporting, we can now use the cross-sectional data from the Round 2 school survey to measure the extent to which the treatment improved the probability that a girl attended school regularly during all three terms in 2008 and whether she successfully completed her current grade – according to her teacher.[19] The attendance impact estimates presented in the top panel of Table 4b confirm those presented above using self-reported attendance data. In the bottom panel, we see a strongly significant 16 percentage point increase in grade completion among *baseline dropouts*, but no statistically significant impact among baseline schoolgirls. If we compare these completion impacts to the attendance impacts,

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[18] For more on the relationship between self-reported attendance and the records from the school survey, see Baird, McIntosh, and Özler (2009b).

[19] **"Attended school regularly"** is equal to "1" if the student's teacher reported the student to have attended "more often than not" in each of the three school terms in 2008. **"Passed grade"** is equal to "1" if the teacher reported that the student made satisfactory progress to "pass this grade to continue to the next grade." The school attendance reported by the student's teacher is, in effect, a different variable than the self-reported attendance, and closer in spirit to the conditionality imposed by the program.

however, we see that the share of baseline dropouts returning to school who successfully pass (16.2/44.2=53%) is in fact similar to the share of baseline schoolgirls remaining in school who pass (2.5/5.9=42%). Therefore, it appears likely that the larger completion impacts of the treatment on baseline dropouts are an artifact of the larger attendance impacts, rather than indicating that baseline schoolgirls who remain in school because of the treatment are somehow uniquely predisposed to fail. The results for *baseline dropouts* suggest that the program is having at least some impact on attainment. Whether these attainment gains are resulting in improvements in relevant learning areas or not will be assessed when we conduct tests in mathematics, reading comprehension, and problem solving/life skills among the entire study sample in early 2010.

*4.2. Impact of Transfer sizes & splits*

There is no evidence that an increase is the total transfer size has a strong marginal impact on school attendance over the receipt of the minimum transfer size (US$5/month for the parents and the student *combined*) in any treatment group. Even among baseline dropouts where overall schooling impacts are large, giving more money than the lowest *total* transfer amount appears to have little effect.[20] This is borne out by visual inspection of Figures 5 & 6, which show a real schooling difference in differences between the control and the treatment group as a whole, but little apparent slope across the size of the *total* transfer. The first column of results for each group in Tables 5a and 5b give the regression output that corresponds to these images, and confirms the absence of any strong relationships over transfer size. Impacts seem, in general, more responsive to *individual* transfer amounts, but are significant only when individual transfer sizes are increased among *conditional* schoolgirls. For example, among conditional schoolgirls, each $1 transferred to the girl, seems to reduce her likelihood of dropout by 1.3 percentage points, implying a reduction in dropout of more than 50% if the girl is receiving the highest *individual* transfer amount of $5. Similar effects are found for English literacy in the entire

[20] Although, it seems that the total transfer size has some impact on improving self-reported literacy in English, especially in the *unconditional* treatment arm (Table 5b).

sample, again mostly owing to the significant effect among *baseline schoolgirls* receiving conditional transfers.

Turning our attention to the split of the *total* transfer between parents and the young girl, a policy question which bears directly on the extensive literature on intra-household allocation is how the share of the transfer going directly to the girl might alter behavior. This is a subject modeled by Berry (2009), who suggests a variation on the Eswaran & Kotwal (1984) monitoring problem to model the motivation problem faced by the parents while trying to generate good schooling outcomes for their children. It is unclear *a priori* how a given amount of money can most effectively be split between the young woman and her family. Our research design provides a rich experimental angle on this question.

In order to isolate the effect of the split, we run a difference-in-differences regression using only treatment girls (because this split is undefined in the control). We then include the total transfer size to soak up any way in which the different total amounts of household and individual transfers might enter the ratio. The strongest statistical effect in the second column of results for each group in Table 5a, statistically significant at the 90% level, is that when baseline schoolgirls receive conditional transfers, the higher the share of the transfer to the girl is, the greater are the schooling impacts. Figures 7 & 8 plot this relationship for baseline dropouts and schoolgirls, respectively, showing changes in outcomes over the distribution of transfer splits; these images visually reinforce the idea that *baseline schoolgirls* (in particular those receiving *conditional* transfers, but not *baseline dropouts*) who receive a greater share of the total transfer are somewhat less likely to drop out of school.

This lack of strong differential impacts across transfer sizes suggests that the elasticity of the total transfer amounts across the wide range used in our study, i.e. $5 to $15 per month, is not significantly different than zero. Tables 5a and 5b subtract the minimum transfer from the total transfer size, making it so that the Post-Treatment dummy estimates the impact of the minimum total transfer size. This provides an alternative way of expressing the lack of impact of transfer sizes above and beyond the minimum amount: these schooling impacts at the lowest transfer size are almost as large as the average treatment effects estimated in Table 4 – with the exception of baseline schoolgirls receiving conditional transfers.

This finding may have major cost-efficiency implications for the design of CCT programs, because it suggests that modest payments can be almost as effective at inducing attendance and improving educational outcomes as much more substantial ones.

*4.3. Conditionality*

We directly randomized whether the offers in an EA were conditional upon school attendance among *baseline schoolgirls*. We therefore have experimental evidence that helps us to identify the 'price' effect whereby conditionality alters the relative costs and benefits of schooling versus other uses of children's time. As the average transfer to the *conditional* and the *unconditional* group is the same, any difference in outcomes between these two randomly assigned groups can be interpreted as the impact of the 'conditionality'.[21] As can be seen in Table 6, there are no significant one-year impacts of conditionality on schooling and literacy.[22]

A major advantage of our research design is that it intersects multiple forms of contract variation simultaneously, thereby providing us with experimental evidence on the impact of one contract parameter across the distribution of a different parameter. One question of interest is whether increasing transfer amounts is more effective when the transfer is conditional, compared to the same increase in transfer size for an unconditional transfer. A visual representation of such an investigation is given in Figure 9, which separately plots changes in schooling for conditional and unconditional girls, and for each group, by transfer amount. There is no obvious pattern. This two-parameter variation is exploited through an interaction analysis in Table 6. The transfer size is interacted with the dummy for conditionality, and the

[21] As mentioned in section 3.2 above, secondary school fees were directly paid to the school for *conditional* cash transfer recipients. To ensure that the average transfer size for the *unconditional* group was equal to that in the *conditional* group, we have added the average secondary school fees to the monthly transfers received by the parents in the *unconditional* group. To avoid any semblance or mention of *conditionality* or *schooling*, this was done for girls who were eligible to attend secondary school at the time of the offer and was simply included in the amount offered to the parents in *unconditional* treatment EAs.

[22] The finding that there is no statistically significant effect of the conditionality on schooling outcomes holds if attendance data from the school survey are used in the analysis (Table 4b) instead of the self-reported attendance data.

statistical evidence similarly fails to find a differential effect of transfer size by conditionality on schooling.

### *4.4. Spillover Effects*

There are several dimensions through which impacts of CCT programs could 'spill over' to alter the outcomes among non-beneficiaries. Our survey asks questions to identify the social network (five closest friends) of each of the respondents in our study sample to examine one of these channels. A second plausible channel of spillovers would be through classrooms, and yet another one, namely outcomes among the within-village controls, would form the broadest form of spillover effect. Here, we focus on this final group in this paper to examine possible spillovers of the program, mainly because the saturation of treatment within the study sample in each EA was directly randomized, providing us with experimental variation in the intensity of treatment at the EA level when we compare the pure controls (i.e. *baseline schoolgirls* in **control** villages) to **untreated** *baseline schoolgirls* in **treatment** villages.

Despite this clean source of identification, we do not detect any spillover effects at the EA level at the end of one year of program implementation. Table 7 compares the within-village controls to the pure controls. The columns titled "DID" look for a simple difference-in-difference in school enrolment or English literacy between these two groups, and find none. The columns titled "Saturation" exploit the research design by including variables to capture the (directly randomized) intensity of treatment within a village. Controlling for the number of baseline schoolgirls in an EA and the average treatment/control differences between EAs, we find no additional impact from treating more baseline schoolgirls in an EA, meaning that schooling outcomes among within-village controls are not affected by the intensity of treatment in their villages. Figure 10 confirms this lack of effect visually, and shows that there is no additional explanatory power in the intensity of treatment once the number of baseline schoolgirls in an

EA is controlled for.[23] Hence, neither in Figure 10 nor in the regressions in Table 7, do we see any evidence of spillover effects.[24]

## 5. CONCLUSION

We present evidence from one of the few experimental evaluations of CCT programs in Sub-Saharan Africa. To the best of our knowledge, this study is a first in examining the impact of simultaneous and experimental contract variation over conditionality, transfer size, intra-household transfer allocation, and treatment saturation. We find strong one-year schooling impacts for the entire sample, both among students who had already dropped out of school at baseline and for those who were still in school. Among the *baseline dropouts* – who are older, more sexually active, and come from poorer households that are more likely to be female-headed – not only school attendance, but also self-reported literacy in English improved significantly.

Generally speaking, schooling outcomes are surprisingly insensitive to the rich variation in contract parameters provided by our study design. We cannot reject the hypothesis that, among *baseline schoolgirls*, the price (or substitution) effect is zero, even though we find relatively large income effects. Nor can we reject the hypothesis that the marginal impact of an increase in the total transfer size on school enrolment is zero. These imply, as can be seen in Figure 9, that a $5/month transfer to a HH made *unconditionally* had roughly the same impact on schooling outcomes as a $15/month transfer made *conditional on school attendance*. The only variation in schooling outcomes with respect to the contract parameters comes from the identity of the HH member receiving the transfer: one-year impacts on school

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[23] To draw this figure, we first calculate average schooling rates by number of treated girls in an EA, and then run a weighted regression to remove the effects of the overall number of girls per EA. Figure 10 plots the residuals from this regression.

[24] Barrera-Osorio et al. (2008) find large peer effects from a CCT program in Colombia among friends registered in the same school and grade, but don't find any effects for the number of students treated in a student's school-grade-gender cohort and suggest that the latter might significantly underestimate the magnitude of peer effects. We have similar data on the network of friends for each of our study participants and will be able to analyze these data in the future.

enrolment are significantly higher when the transfer size given directly to the girl herself is increased, but only significant when the transfers are *conditional* on attending school.

The evidence provided in this paper is not the final word on the impact of conditionality among this target population for a variety of reasons. First, we have not experimented with conditionality among *baseline dropouts*. In fact, further analysis (not shown here) suggests that, among the sub-sample of baseline schoolgirls with a high propensity to drop out of school within one year, the relative impact of the conditionality is much larger compared with the 'income' effect from unconditional transfers.[25] Second, to make an informed decision on whether to 'condition' transfers or not, we need to examine a broader set of outcomes – not only with respect to schooling (e.g. actual learning), but also other relevant outcomes for this target population, such as early marriage, teenage pregnancy, risk of HIV infection, etc. Third, even if we were to see significant impacts of the conditionality, we need to weigh these benefits against the costs of monitoring and enforcement necessitated by the conditionality, which represent a substantial share of the administrative costs of a CCT program. Finally, the one-year results presented here may change after the second and final year of this CCT experiment. We'll probe these issues further in a separate paper (Baird, McIntosh, and Özler, 2009c) devoted entirely to the topic of 'conditionality'.

Yet another critique could be that, because some of the contract design features, such as the conditionality and the parental transfer size, were randomized at the EA level within the 88 treatment EAs, we lack the statistical power to reject meaningful differences between various treatment groups. Working against this, however, is the fact that the individual transfer amounts were randomly assigned through a lottery, and hence both total transfer amounts and transfer splits between the parents and the girls contain individual variation among the 1,168 treated girls. Furthermore, an examination of the regression outputs presented in this paper reveals little to suggest that our statistical tests are suffering from low power. For example, the insignificant coefficient on transfer amounts across all girls in the

[25] The propensity to drop out of school among *baseline schoolgirls* was calculated using a regression for the probability of dropping out between baseline and one-year follow-up on baseline characteristics among the control group. These estimates were then used to predict a probability of dropping out of school for each of the *baseline schoolgirls* in the treatment group.

second column of Table 5a has a standard error of .0034, indicating that a marginal effect of .0068 would be detectable with 95% confidence. This approximately translates into a 7 percentage point increase in schooling moving from the lowest transfer amount ($5/month) to the highest ($15/month). Seen relative to an average treatment effect of 11.5 percentage points (Table 4a, column 1), this does not seem like an unreasonably large minimum impact to be able to detect.[26] Figures 5 and 6 confirm this impression; the treatment changes in outcomes are in fact quite similar to each other across transfer size, and as a group they are very distinct from the changes in the control. Similarly, the expected positive impacts of conditionality do not fail to manifest themselves because the estimate is too noisy, but rather because the point estimate is fact negative (Table 6, column 1). Hence, the finding of no impact across different treatment groups is unlikely to be a result of the study having low statistical power.

Taken as a whole, these one-year results provide evidence that the strongly positive impacts of CCT programs, now well established in Latin America, may indeed generalize to the Sub-Saharan African context. Given that a total transfer offer of $5 per household per month induces the average girl to be 10 percentage points more likely to be in school after one year, the (insignificant) 1.4 percentage point increase in schooling rates achieved by doubling the HH transfer to $10 does not seem cost-effective. Similarly, monitoring school attendance to enforce the conditionality is costly and the cost-effectiveness of imposing a schooling conditionality for cash transfer programs needs to be examined more carefully in light of the income effects detected here. Policy-makers may also consider making at least some of the transfers directly to the target beneficiary in this context.

[26] For example, with an average impact of 11.5 percentage points for the entire study population as a whole, the impact at $5/month could have been 8 percentage points, compared with 15 percentage points at $15/month. Our study would have been able to detect such an impact with confidence.

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## TABLES

### Table 1: Summary Statistics for Dropouts and Schoolgirls at Baseline

| | **Baseline Dropouts** | | **Baseline Schoolgirls:** | |
|---|---|---|---|---|
| Baseline Values of: | Mean | SD | Mean | SD |
| Girl's Age | 17.276 | 2.469 | 15.233 | 1.931 |
| Aggregate consumption p/c | 1322.597 | 999.785 | 1774.799 | 1195.332 |
| Aggregate food consumption p/c | 822.433 | 507.586 | 971.693 | 543.135 |
| Household Asset Index | -0.728 | 2.377 | 0.826 | 2.621 |
| # shocks of any type over previous year | 3.882 | 2.286 | 3.746 | 2.132 |
| Highest Grade attended at baseline | 6.104 | 2.833 | 7.482 | 1.598 |
| Highest Qualification achieved at baseline* | 1.385 | 0.656 | 1.418 | 0.626 |
| Household Size | 6.098 | 2.550 | 6.394 | 2.190 |
| Mother's Education* | 2.095 | 0.859 | 2.282 | 0.865 |
| Father's Education* | 2.686 | 0.980 | 2.875 | 0.939 |
| Female-Headed Household | 0.417 | 0.493 | 0.292 | 0.455 |
| Household has Savings | 0.094 | 0.292 | 0.098 | 0.298 |
| Travel time to School, Minutes | 35.292 | 9.888 | 32.690 | 9.186 |

* (1=none, 2=primary, 3=some secondary, 4=completed secondary)

**Table 2a: Sample Sizes from Surveys, Treatment, and Analysis**

| | Stratum: | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Dropouts | | Schoolgirls | | | | Overall: | |
| | T1 | C1 | T2a | T2b | S2 | C2 | Total Treatments | Total Observations |
| Baseline Household Surveys | 436 | 454 | 506 | 283 | 629 | 1497 | 1225 | 3805 |
| Deemed Eligible for Treatment | 410 | | 500 | 283 | | | 1193 | |
| Found to Offer Treatment | 401 | | 492 | 276 | | | 1169 | |
| Treated 2008 | 401 | | 491 | 276 | | | 1168 | |
| Surveyed in Follow-up | 397 | 408 | 484 | 267 | 588 | 1408 | 1148 | 3552 |
| Used for Panel Analysis | 396 | 408 | 480 | 265 | 588 | 1408 | 1141 | 3545 |

**Table 2b: Determinants of Survey Attrition**

| | ALL | No S2 | SCHOOL GIRL | T2a-T2b | Dropouts | Conditional SG | Unconditional SG |
|---|---|---|---|---|---|---|---|
| =1 if Treatment Girl | -0.000 | 0.001 | 0.004 | | 0.010 | 0.008 | -0.001 |
| | (0.009) | (0.009) | (0.011) | | (0.020) | (0.013) | (0.012) |
| =1 if Conditional Schoolgirl | | | | 0.008 | | | |
| | | | | (0.013) | | | |
| =1 if Unconditional Schoolgirl | | | | -0.004 | | | |
| | | | | (0.015) | | | |
| control mean | 0.932*** | 0.931*** | 0.941*** | 0.941*** | 0.899*** | 0.941*** | 0.931*** |
| | (0.005) | (0.006) | (0.007) | (0.007) | (0.013) | (0.007) | (0.006) |
| Number of observations | 3,805 | 3,176 | 2,286 | 2,286 | 890 | 2,003 | 2,893 |

note: *** p<0.01, ** p<0.05, * p<0.1

**Table 3: Balance Tests**

| | Baseline Values of: Aggregate Consumption per person | Age | Mother's Education | Highest Educational Qualification | Household Has Savings? | Household Size | Female-Headed Household | # of obser-vations |
|---|---|---|---|---|---|---|---|---|
| Overall Treatment Balanced? | 18.714 | -0.153 | 0.034 | -0.019 | 0.020 | 0.074 | -0.039 | 2957 |
| | (81.975) | (0.102) | (0.051) | (0.038) | (0.018) | (0.108) | (0.024)* | |
| Treatment among Dropouts Balanced? | 3.016 | -0.330 | -0.009 | -0.019 | 0.008 | 0.026 | 0.026 | 804 |
| | (122.377) | (0.245) | (0.070) | (0.062) | (0.020) | (0.207) | (0.207) | |
| Treatment among Schoolgirls Balanced? | 25.189 | -0.080 | 0.052 | -0.019 | 0.025 | 0.094 | -0.057 | 2153 |
| | (78.772) | (0.096) | (0.058) | (0.041) | (0.022) | (0.119) | (0.028)** | |
| Conditionality Balanced? | -43.090 | -0.247 | -0.001 | -0.144 | 0.040 | -0.318 | -0.021 | 2153 |
| | (110.806) | (0.153) | (0.086) | (0.065)** | (0.036) | (0.181)* | (0.049) | |
| Transfer Amounts Balanced? | 8.000 | -0.005 | 0.005 | 0.013 | -0.001 | 0.021 | 0.004 | 2153 |
| | (16.944) | (0.024) | (0.017) | (0.009) | (0.005) | (0.035) | (0.007) | |
| Spillover/Control Balanced? | 88.286 | 0.015 | -0.022 | 0.003 | 0.047 | -0.044 | -0.048 | 1996 |
| | (111.820) | (0.106) | (0.062) | (0.054) | (0.022)** | (0.129) | (0.025)* | |
| EA-level Saturation Balanced? | 1.429 | 0.004 | 0.000 | -0.001 | 0.000 | 0.001 | 0.001 | 1996 |
| | (4.166) | (0.003) | (0.002) | (0.002) | (0.001) | (0.004) | (0.001) | |

* Significant at 90%, **significant at 95%, *** significant at 99%, EA-clustered standard errors in parentheses to reflect the design effect.

Balance test for Overall Treatment run using a treatment dummy and an indicator for baseline schooling status. Tests include only the units with follow-up data who are used in the rest of the analysis. Balance among Dropouts and Schoolgirls estimated with a simple treatment dummy, comparing to the relevant control group. Conditionality test based on a dummy for conditionality in a regression controlling for treatment in a comparison of treated to control schoolgirls. Transfer amount test based on coefficient on total transfer amount, with dummy for treatment included. Spillover/control test compares within-village controls (S2) to control villages, and EA-level saturation test based on the coefficient on EA-level saturation in a regression including a dummy indicating village-level treatment.

**Table 4a: Educational Impacts by Stratum (*self-reported*)**

| **Dependent Variable:** | **In School** | | | | | **English Literacy** | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All | Baseline Dropouts | All Baseline Schoolgirls | Conditional Schoolgirls | Uncon-ditional Schoolgirls | All | Baseline Dropouts | All Baseline Schoolgirls | Conditional Schoolgirls | Uncon-ditional Schoolgirls |
| Post-Treatment Dummy | 0.115 | 0.442 | 0.046 | 0.038 | 0.061 | 0.027 | 0.072 | 0.017 | 0.028 | -0.002 |
| | (0.015)*** | (0.035)*** | (0.016)*** | (0.019)** | (0.019)*** | (0.022) | (0.029)** | (0.026) | (0.031) | (0.028) |
| Round 2 Dummy | 0.333 | 0.172 | -0.109 | -0.109 | -0.109 | 0.047 | 0.025 | 0.086 | 0.086 | 0.086 |
| | (0.024)*** | (0.020)*** | (0.013)*** | (0.013)*** | (0.013)*** | (0.017)*** | (0.019) | (0.018)*** | (0.018)*** | (0.018)*** |
| In School at Baseline | -0.474 | | | | | 0.036 | | | | |
| | (0.026)*** | | | | | (0.020)* | | | | |
| Observations | 5914 | 1608 | 4306 | 3776 | 3346 | 5909 | 1607 | 4302 | 3772 | 3342 |
| # unique individuals | 2957 | 804 | 2153 | 1888 | 1673 | 2957 | 804 | 2153 | 1888 | 1673 |
| R-squared | 0.26 | 0.51 | 0.09 | 0.1 | 0.1 | 0.05 | 0.03 | 0.06 | 0.06 | 0.05 |
| Mean of Outcome in Control: | 0.832 | 0 | 1 | 1 | 1 | 0.766 | 0.463 | 0.827 | 0.827 | 0.827 |

All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs.

* Significant at 90%, **significant at 95%, *** significant at 99%, robust standard errors in parentheses.

**Table 4b: School Attendance and Grade Progression Impacts by Stratum (*reported by the teacher*)**

| **Dependent Variable:** | **Attended Regularly All 3 Terms** | | | | |
|---|---|---|---|---|---|
| | All | Baseline Dropouts | All Baseline Schoolgirls | Conditional Schoolgirls | Uncon-ditional Schoolgirls |
| Post-Treatment Dummy | 0.103 | 0.306 | 0.059 | 0.066 | 0.047 |
| | (0.019)*** | (0.038)*** | (0.022)*** | (0.027)** | (0.028)* |
| In School at Baseline | 0.556 | | | | |
| | (0.026)*** | | | | |
| Mean of Outcome in Control: | 0.674 | 0.114 | 0.791 | 0.791 | 0.791 |

| **Dependent Variable:** | **Passed Grade** | | | | |
|---|---|---|---|---|---|
| | All | Baseline Dropouts | All Baseline Schoolgirls | Conditional Schoolgirls | Uncon-ditional Schoolgirls |
| Post-Treatment Dummy | 0.049 | 0.162 | 0.025 | 0.039 | -0.005 |
| | (0.030) | (0.028)*** | (0.037) | (0.044) | (0.043) |
| In School at Baseline | 0.428 | | | | |
| | (0.024)*** | | | | |
| Mean of Outcome in Control: | 0.497 | 0.087 | 0.582 | 0.582 | 0.582 |
| Observations | 2874 | 787 | 2087 | 1832 | 1618 |

**“Attended school regularly”** is equal to “1” if the student’s teacher reported the student to have attended “more often than not” in each of the three school terms in 2008. **“Passed grade”** is equal to “1” if the teacher reported that the student made satisfactory progress to “pass this grade to continue to the next grade.”
All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs.

* significant at 90%, **significant at 95%, *** significant at 99%,

Standard errors are in parentheses.

**Table 5a: Schooling Impacts of Transfer Sizes and Splits**

| | All | | Baseline Dropouts | | All Baseline Schoolgirls | | Conditional Schoolgirls | | Unconditional Schoolgirls | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Dependent Variable: In School in Followup** | Amounts | Share | Amounts | Share | Amounts | Share | Amounts | Share | Amounts | Share |
| Household Transfer Amount | 0.001 | | 0.007 | | -0.001 | | -0.002 | | 0.003 | |
| | (0.004) | | (0.012) | | (0.004) | | (0.005) | | (0.005) | |
| Individual Transfer Amount | 0.006 | | 0.008 | | 0.008 | | 0.013 | | 0.000 | |
| | (0.005) | | (0.015) | | (0.005) | | (0.006)** | | (0.011) | |
| Share of Transfer to Girl | | 0.070 | | 0.000 | | 0.087 | | 0.137 | | -0.011 |
| | | (0.062) | | (0.178) | | (0.063) | | (0.070)* | | (0.123) |
| Total Transfer Amount | | 0.003 | | 0.008 | | 0.002 | | 0.002 | | 0.002 |
| | | (0.003) | | (0.009) | | (0.003) | | (0.004) | | (0.005) |
| Post-Treatment Dummy | 0.099 | 0.578 | 0.404 | 0.576 | 0.031 | -0.099 | 0.018 | -0.123 | 0.052 | -0.055 |
| (impact when transfer size = lowest value) | (0.022)*** | (0.039)*** | (0.052)*** | (0.071)*** | (0.02) | (0.030)*** | (0.03) | (0.039)*** | (0.030)* | (0.05) |
| In School at Baseline | -0.474 | -0.677 | | | | | | | | |
| | (0.026)*** | (0.030)*** | | | | | | | | |
| Round 2 Dummy | 0.333 | | 0.172 | | -0.109 | | -0.109 | | -0.109 | |
| | (0.024)*** | | (0.020)*** | | (0.013)*** | | (0.013)*** | | (0.013)*** | |
| Observations | 5914 | 2282 | 1608 | 792 | 4306 | 1490 | 3776 | 960 | 3346 | 530 |
| # unique individuals | 2957 | 1141 | 804 | 396 | 2153 | 745 | 1888 | 480 | 1673 | 265 |
| R-squared | 0.26 | 0.44 | 0.52 | 0.61 | 0.09 | 0.07 | 0.1 | 0.08 | 0.1 | 0.05 |
| Baseline Mean of Outcome in Control: | 0.832 | 0.832 | 0 | 0 | 1 | 1 | 1 | 1 | 1 | 1 |

Monetary units are all in US Dollars. All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs.

* significant at 90%, **significant at 95%, *** significant at 99%, robust standard errors in parentheses.

**Table 5b: Literacy Impacts of Transfer Sizes and Splits**

| | All | | Baseline Dropouts | | All Baseline Schoolgirls | | Conditional Schoolgirls | | Unconditional Schoolgirls | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Dependent Variable: Literate in English** | Amounts | Share | Amounts | Share | Amounts | Share | Amounts | Share | Amounts | Share |
| Household Transfer Amount | 0.006 | | 0.004 | | 0.006 | | 0.001 | | 0.017 | |
| | (0.006) | | (0.009) | | (0.007) | | (0.010) | | (0.007)** | |
| Individual Transfer Amount | 0.024 | | 0.018 | | 0.025 | | 0.031 | | 0.015 | |
| | (0.012)** | | (0.014) | | (0.014)* | | (0.018)* | | (0.018) | |
| Share of Transfer to Girl | | 0.176 | | 0.115 | | 0.189 | | 0.281 | | 0.009 |
| | | (0.128) | | (0.138) | | (0.156) | | (0.203) | | (0.180) |
| Total Transfer Amount | | 0.011 | | 0.008 | | 0.012 | | 0.010 | | 0.017 |
| | | (0.005)** | | (0.008) | | (0.006)** | | (0.008) | | (0.007)** |
| Post-Treatment Dummy | -0.040 | -0.012 | 0.025 | 0.023 | -0.054 | -0.014 | -0.040 | -0.020 | -0.083 | 0.000 |
| (impact when transfer size = 0) | (0.032) | (0.052) | (0.052) | (0.068) | (0.035) | (0.050) | (0.042) | (0.065) | (0.048)* | (0.078) |
| In School at Baseline | 0.034 | 0.005 | | | | | | | | |
| | (0.020)* | (0.027) | | | | | | | | |
| Round 2 Dummy | 0.048 | | 0.025 | | 0.086 | | 0.086 | | 0.086 | |
| | (0.017)*** | | (0.019) | | (0.018)*** | | (0.018)*** | | (0.018)*** | |
| Observations | 5909 | 2281 | 1607 | 791 | 4302 | 1490 | 3772 | 960 | 3342 | 530 |
| # unique individuals | 2957 | 1141 | 804 | 396 | 2153 | 745 | 1888 | 480 | 1673 | 265 |
| R-squared | 0.05 | 0.08 | 0.03 | 0.06 | 0.06 | 0.08 | 0.06 | 0.08 | 0.05 | 0.08 |
| Baseline Mean of Outcome in Control: | 0.766 | 0.766 | 0.463 | 0.463 | 0.839 | 0.827 | 0.827 | 0.827 | 0.827 | 0.827 |

Monetary units are all in US Dollars. All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs.

* significant at 90%, **significant at 95%, *** significant at 99%, robust standard errors in parentheses.

**Table 6: Conditionality and Interactions with Transfer Size among *Baseline Schoolgirls***

**Regression compares T2a (conditional schoolgirls) to T2b (unconditional schoolgirls) and C2 (control schoolgirls)**

| | **Schooling** | | | | **English Literacy** | | | |
|---|---|---|---|---|---|---|---|---|
| | | Conditionality Interacted with: | | | | Conditionality Interacted with: | | |
| **Dependent Variable: In School in Followup** | Effect of Conditionality Alone | Household Transfers | Individual Transfers | Total Transfers | Effect of Conditionality Alone | Household Transfers | Individual Transfers | Total Transfers |
| Conditionality | -0.023 | -0.008 | -0.049 | -0.023 | 0.030 | 0.075 | -0.003 | 0.064 |
| | (0.020) | (0.026) | (0.035) | (0.036) | (0.033) | (0.046) | (0.054) | (0.055) |
| Conditionality * Transfer Amount | | -0.005 | 0.012 | 0.000 | | -0.016 | 0.016 | -0.007 |
| | | (0.007) | (0.012) | (0.006) | | (0.012) | (0.026) | (0.010) |
| Transfer Amount | | 0.003 | 0.000 | 0.002 | | 0.017 | 0.015 | 0.017 |
| | | (0.005) | (0.010) | (0.005) | | (0.007)** | (0.018) | (0.007)** |
| Post-Treatment Dummy (T2a and T2b) | 0.061 | 0.052 | 0.061 | 0.051 | -0.002 | -0.051 | -0.033 | -0.084 |
| (Measures impact of T2b with transfer at lowest) | (0.019)*** | (0.021)** | (0.030)** | (0.028)* | (0.028) | (0.031) | (0.046) | (0.042)** |
| Round 2 dummy | -0.109 | -0.109 | -0.109 | -0.109 | 0.086 | 0.086 | 0.086 | 0.086 |
| | (0.013)*** | (0.013)*** | (0.013)*** | (0.013)*** | (0.018)*** | (0.018)*** | (0.018)*** | (0.018)*** |
| Observations | 4306 | 4306 | 4306 | 4306 | 4302 | 4302 | 4302 | 4302 |
| # unique individuals | 2153 | 2153 | 2153 | 2153 | 2153 | 2153 | 2153 | 2153 |
| R-squared | 0.09 | 0.09 | 0.1 | 0.09 | 0.06 | 0.06 | 0.06 | 0.06 |

Monetary units are all in US Dollars. All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs.

* significant at 90%, **significant at 95%, *** significant at 99%, robust standard errors in parentheses.

**Table 7: Spillover Effects**

| Dependent Variable: In School in Followup | Schooling DID | Schooling Saturation | English Literacy DID | English Literacy Saturation |
|---|---|---|---|---|
| Post-Treatment Dummy for Within-Village Controls: | 0.011 | -0.047 | 0.012 | 0.067 |
| | (0.020) | (0.026)* | (0.028) | (0.041) |
| # of Treated Baseline Schoolgirls in Village | | -0.002 | | -0.005 |
| | | (0.003) | | (0.003) |
| # of Baseline Schoolgirls in Village | | 0.002 | | 0.000 |
| | | (0.001)*** | | (0.001) |
| Round 2 dummy | -0.109 | -0.109 | 0.086 | 0.086 |
| | (0.013)*** | (0.013)*** | (0.018)*** | (0.018)*** |
| Observations | 3992 | 3992 | 3988 | 3988 |
| # unique individuals | 1996 | 1996 | 1996 | 1996 |
| R-squared | 0.1 | 0.11 | 0.05 | 0.05 |

representative of all study EAs.

* significant at 90%, **significant at 95%, *** significant at 99%, robust standard errors in parentheses.

**FIGURES**

**Figure 1 Intended and Actual Treatment (Within the Study Sample)**

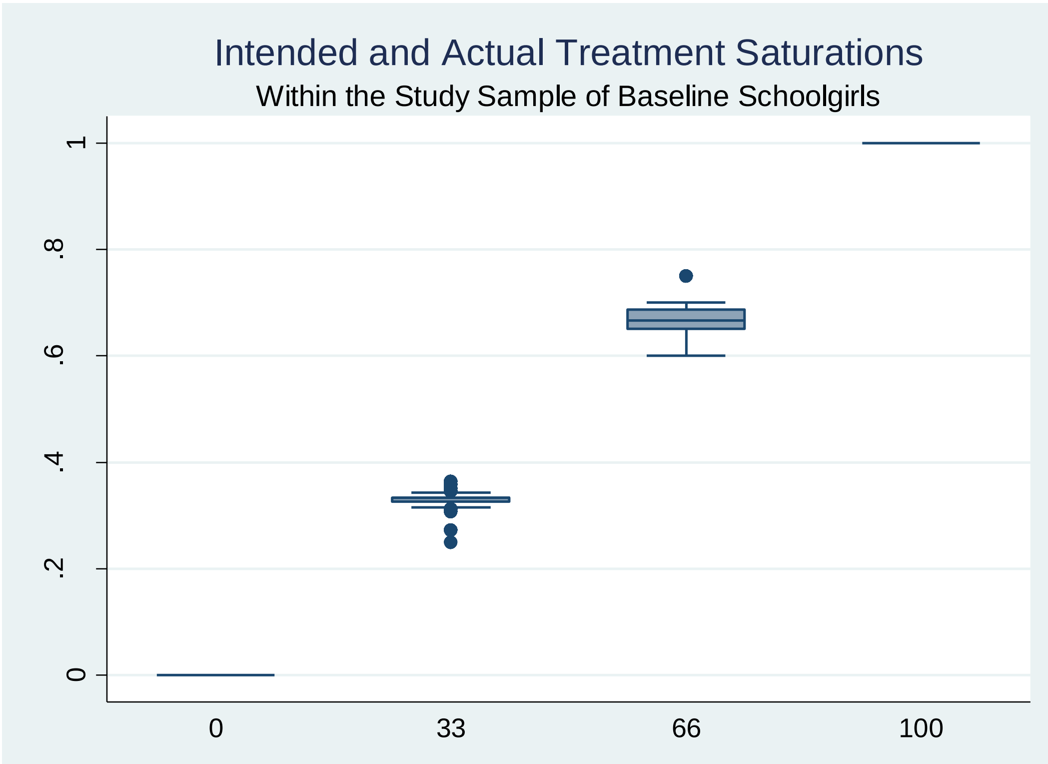



**Figure 2**

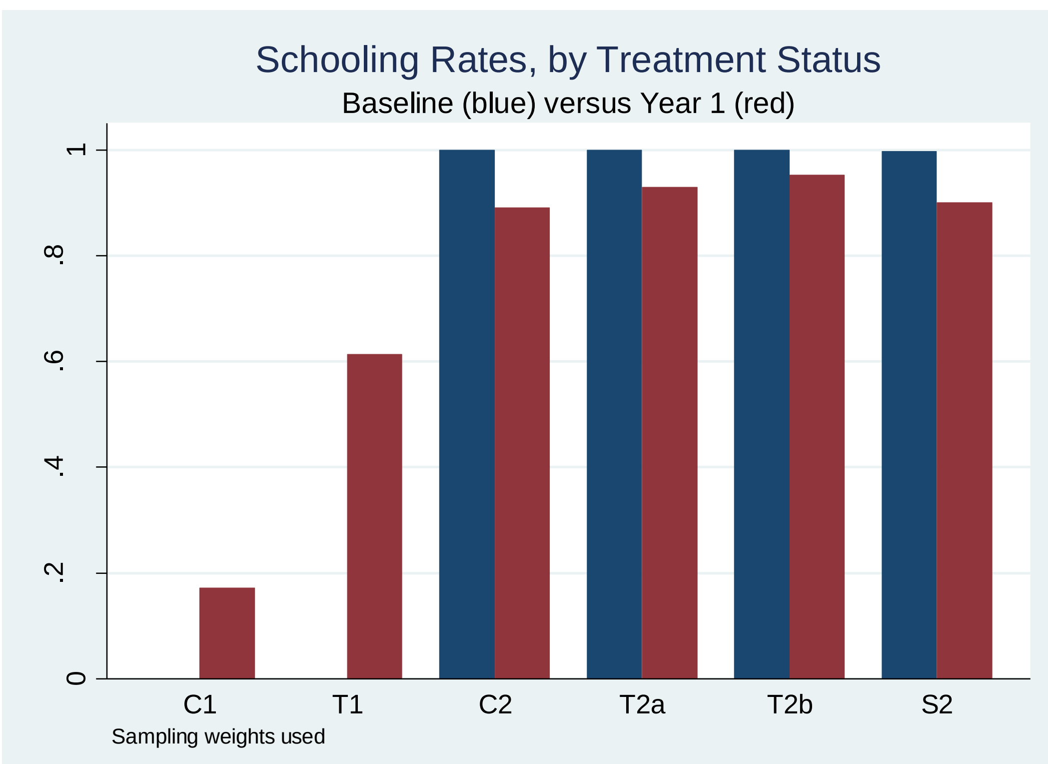


**Figure 3a**

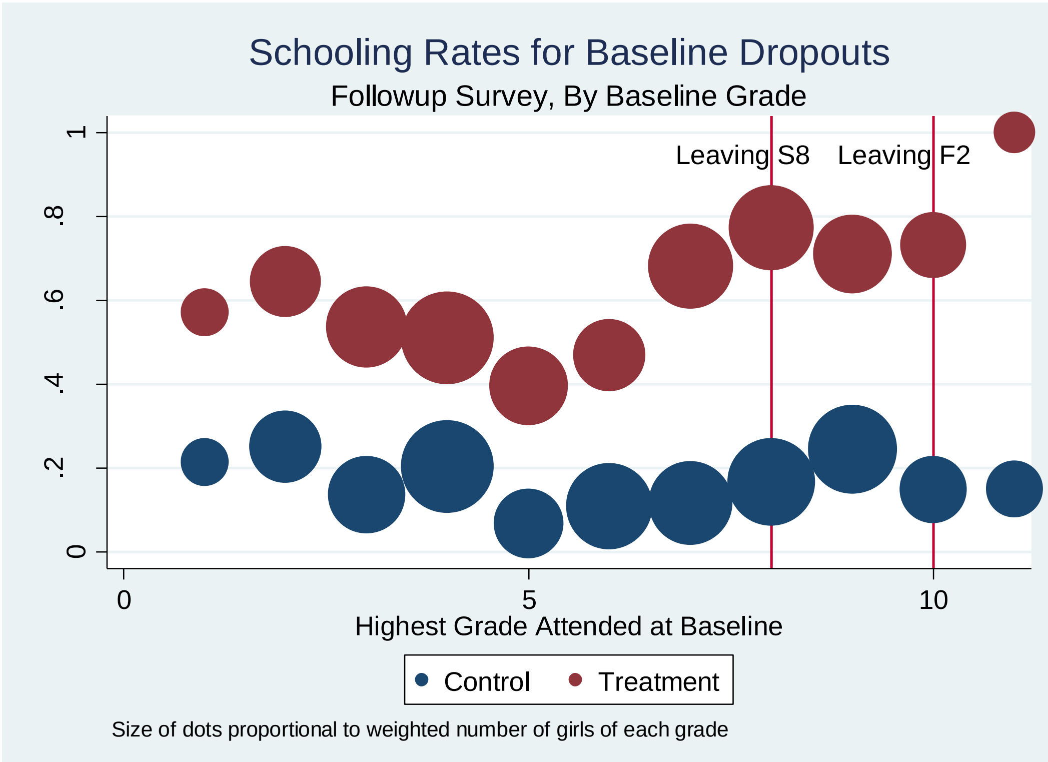



**Figure 3b**

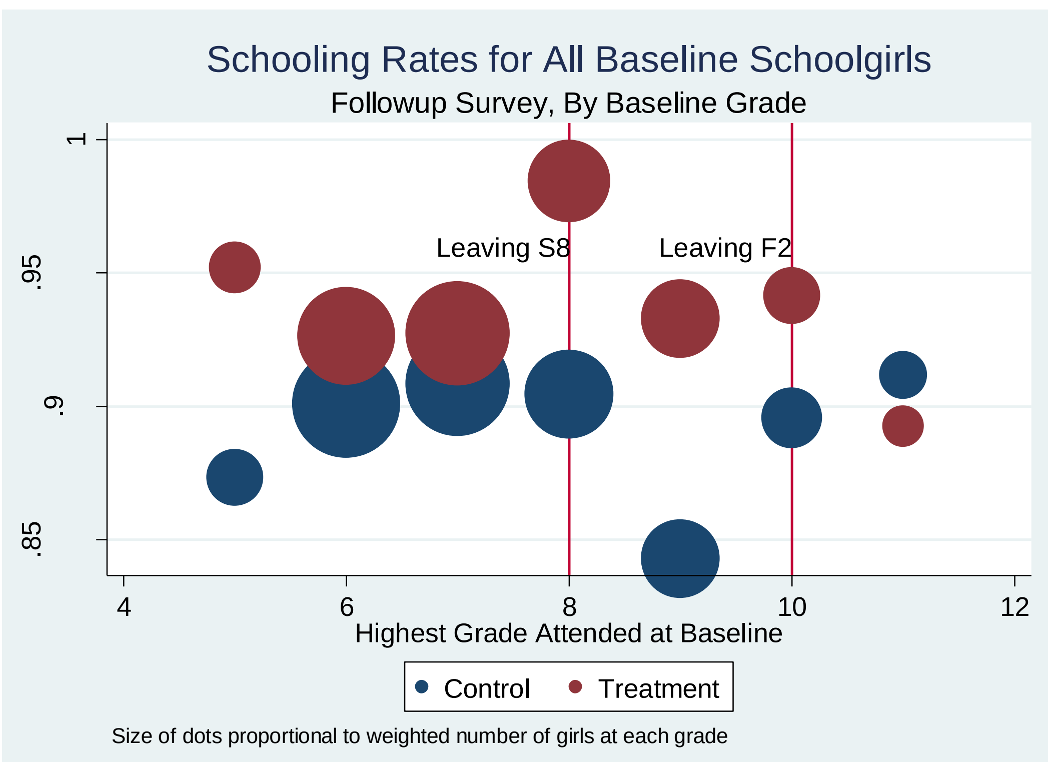


**Figure 4a**

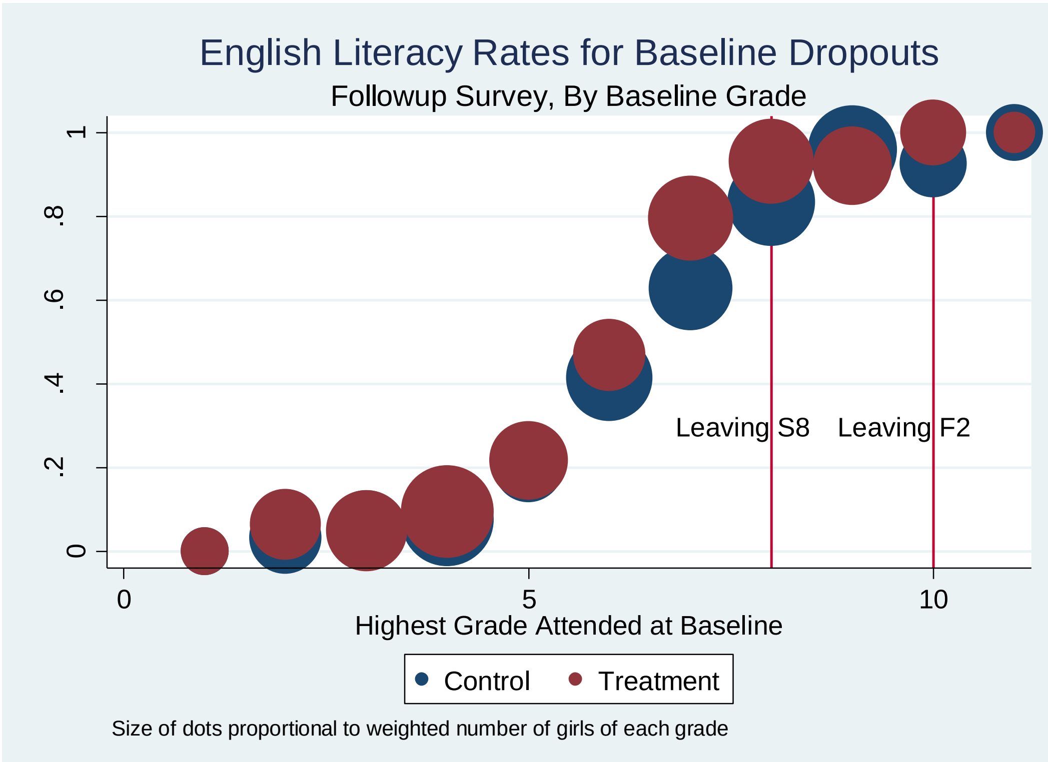



**Figure 4b**

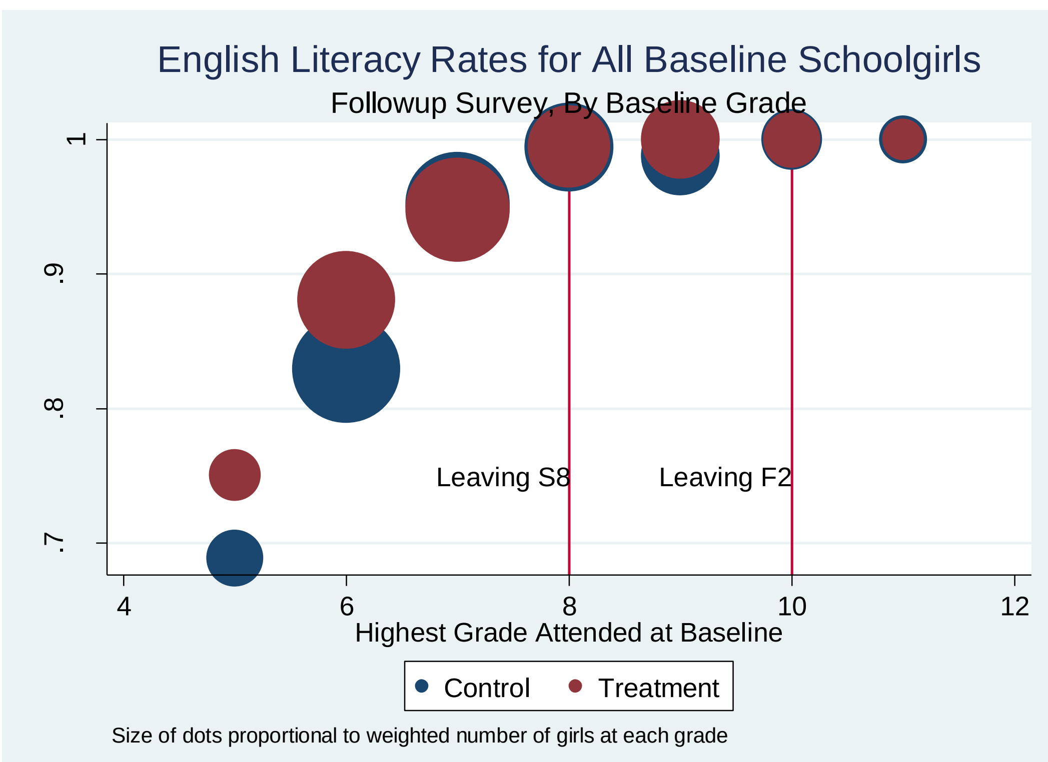


**Figure 5**

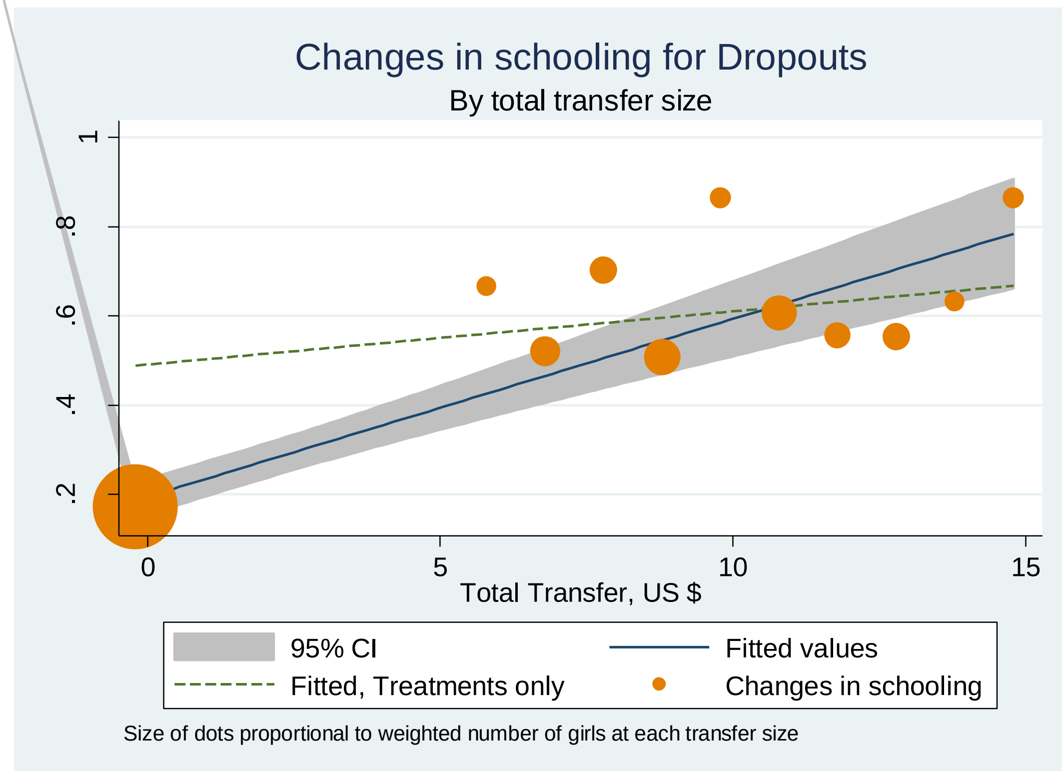



**Figure 6**

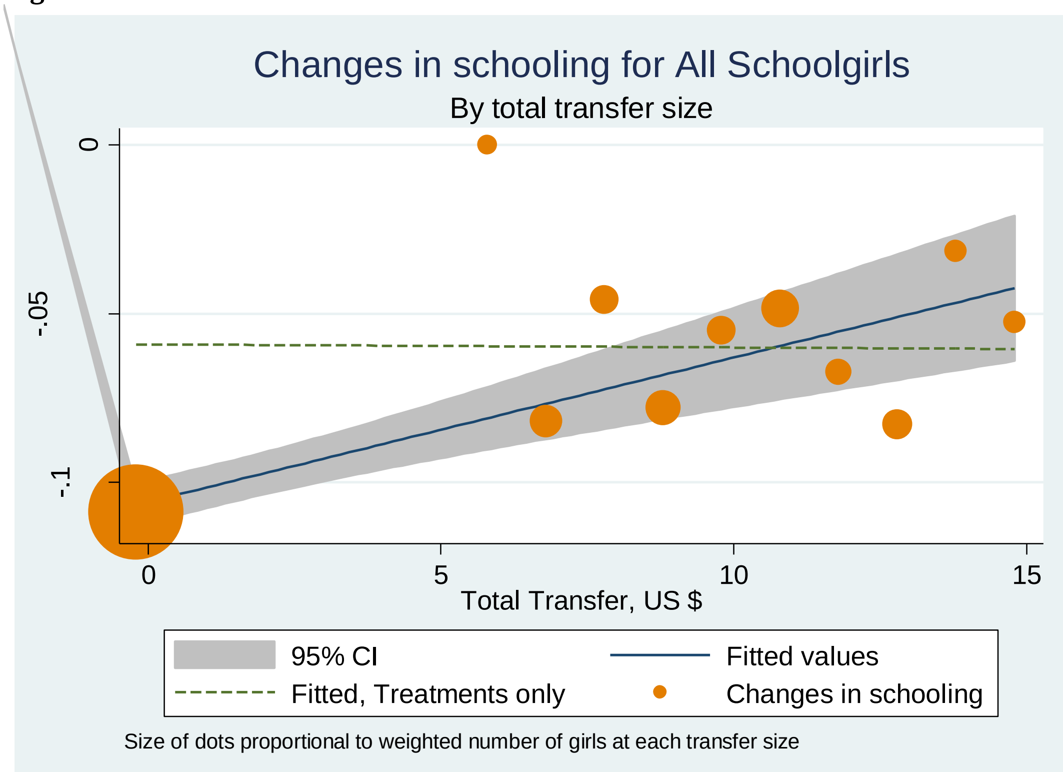


**Figure 7**

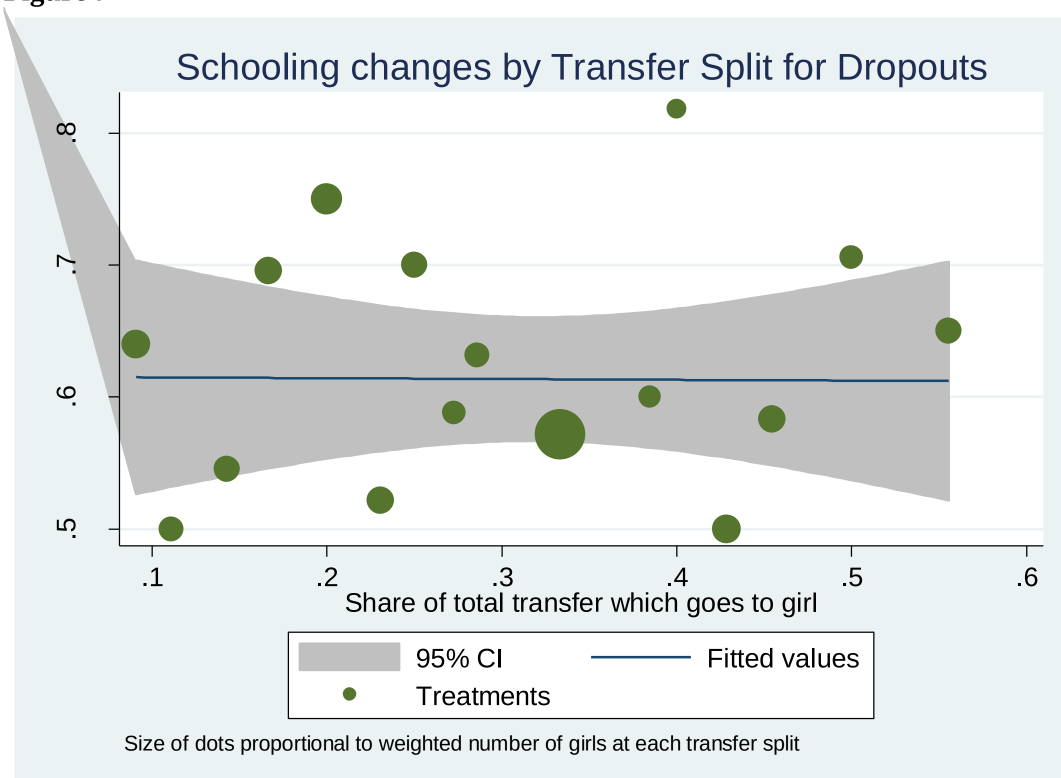



**Figure 8**

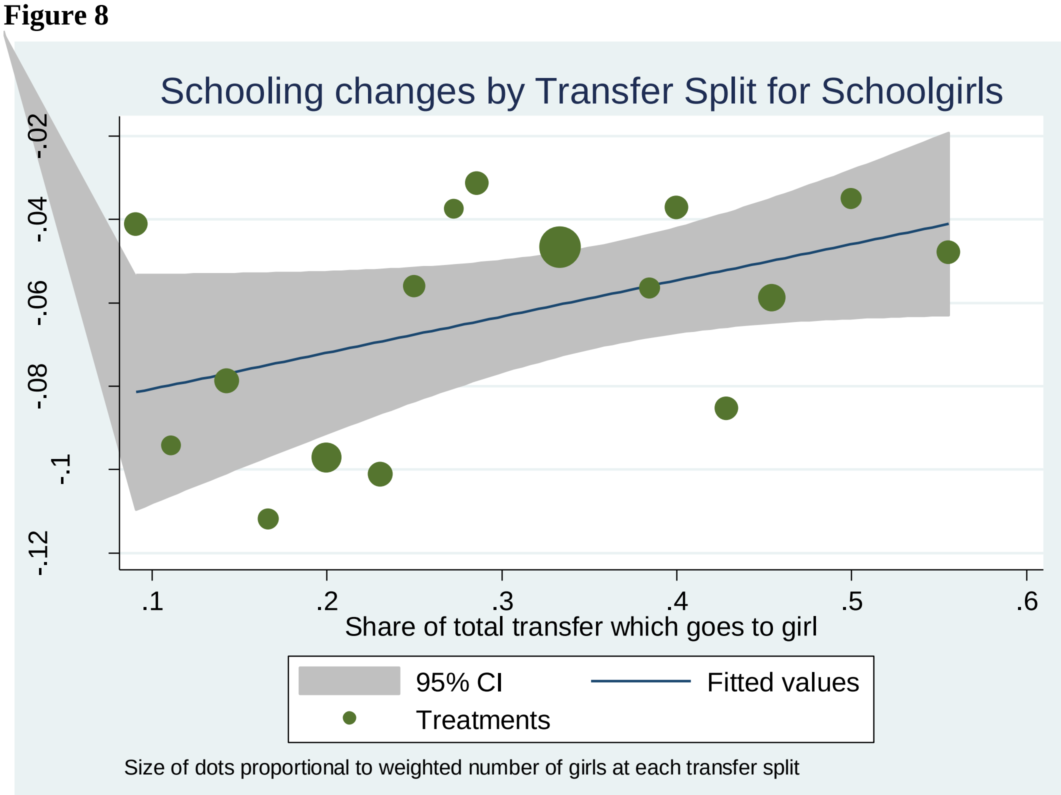


**Figure 9**

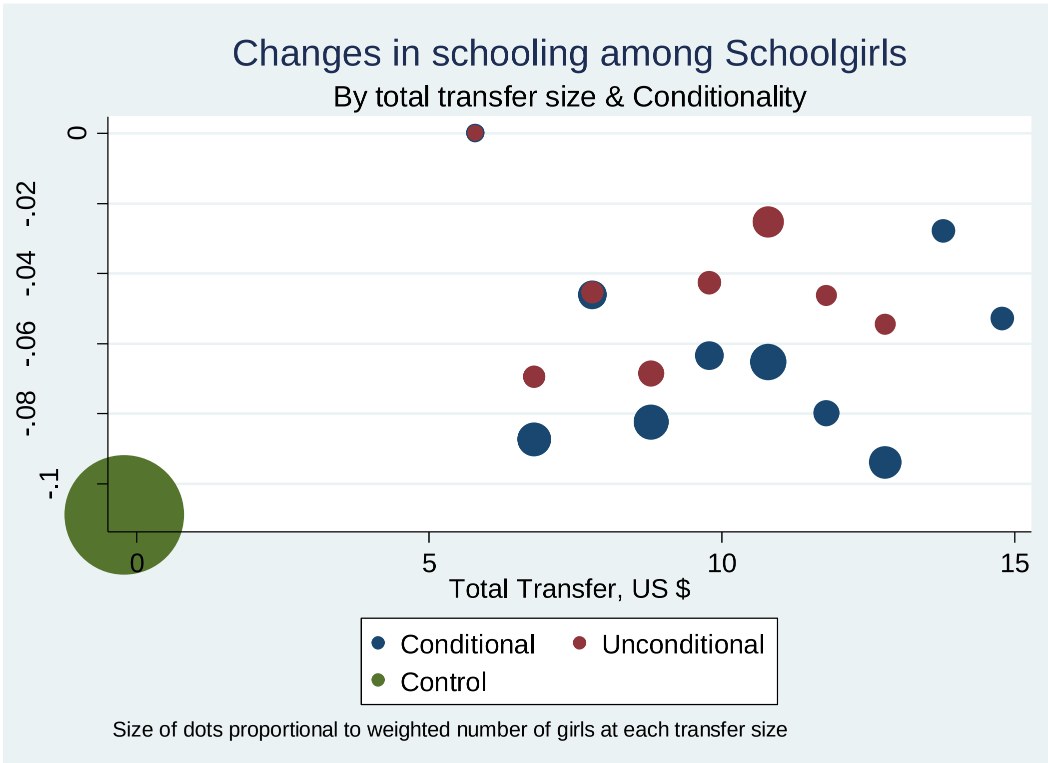
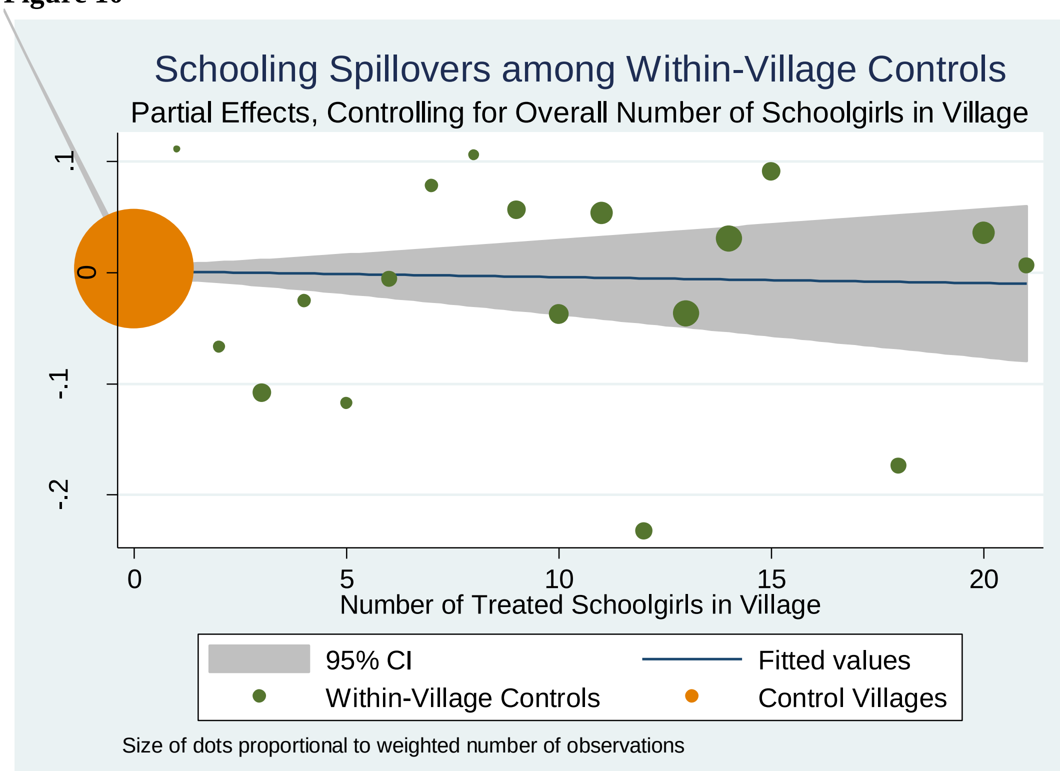



**Figure 10**



# The Short-Term Impacts of a Schooling Conditional Cash Transfer Program on the Sexual Behavior of Young Women[1]

Sarah Baird, George Washington University
Ephraim Chirwa, University of Malawi
Craig McIntosh, University of California at San Diego
Berk Özler[2], World Bank[3]

Recent evidence suggests that Conditional Cash Transfer Programs (CCTs) for schooling are effective in raising school enrolment and attendance. However, there is also reason to believe that such programs can affect other outcomes, such as the sexual behavior of their young beneficiaries. Zomba Cash Transfer Program (ZCTP) is a randomized ongoing CCT intervention targeting young women in Malawi that provides incentives (in the form of school fees and cash transfers) to current schoolgirls and recent dropouts to stay in or return to school. An average offer of US$10/month conditional on satisfactory school attendance – plus direct payment of secondary school fees – led to significant declines in early marriage, teenage pregnancy, and self-reported sexual activity among program beneficiaries after just one year of program implementation. For program beneficiaries who were out of school at baseline, the probability of getting married and becoming pregnant declined by more than 40% and 30%, respectively. In addition, the incidence of the onset of sexual activity was 38% lower among all program beneficiaries than the control group. Overall, these results suggest that CCT programs not only serve as useful tools for improving school attendance, but may also reduce sexual activity, teen pregnancy, and early marriage.

Keywords: cash transfers; education; sexual behavior; gender; randomized intervention

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[2]Correspondence to: Berk Ozler, The World Bank, 1818 H St. NW, Washington, DC 20433. Phone: 202-458-5861. Fax: 202-522-1153. E-mail: bozler@worldbank.org.
[3] The authors have no conflicts of interest to disclose. The study has been approved by the National Health Sciences Research Committee in Malawi (protocol number: 569) and by the Human Research Protections Program Committee at University of California, San Diego (protocol number: 090378).

## 1. Introduction

Conditional Cash Transfers (CCTs) can be an important component of social protection policy and there is "…considerable evidence that CCTs have improved the lives of poor people" (World Bank, 2009). Early CCT pilots such as Mexico's *Progresa,* Brazil's *Bolsa Escola*, and Nicaragua's *Red de Protección* have been popular and became national programs a few years later. 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." (World Bank, 2009) It seems that CCT programs are here to stay – at least for the foreseeable future.

However, such programs have been largely evaluated on a small set of outcomes, which have more to do with the behavior that the program is being "conditioned" on (such as school enrolment), rather than, say, learning or labor market outcomes. Naturally, there is now an increased focus, from policy-makers and researchers alike, on examining a broader set of outcomes that might be plausibly affected by these programs and that are pertinent for policy design. The contribution of this paper is to extend the analysis of CCT programs to a new context and a novel set of outcomes; namely their potential to prevent risky sexual behavior among school-age girls and young women in Sub-Saharan Africa.

When available, evidence as to the impact of CCT programs on "final" educational outcomes such as test scores, shows only modest effects (World Bank, 2009). More importantly for sub-Saharan Africa (SSA), very little is known about the possible effect of these programs on the sexual behavior of the target beneficiaries, including age of marriage and fertility decisions.[4] Given the close link between sexual behavior and HIV infection, and given the burden HIV poses on these economies, this is potentially a very important impact to document.

There are good reasons to think that CCT programs for schooling may affect the sexual behavior of young people. Education has been suggested as a "social vaccine" to change sexual behavior and prevent the spread of HIV (Jukes, Simmons, and Bundy, 2008), but almost all of the evidence we have on this comes from cross-sectional studies. Furthermore, the role of income (especially that of women's poverty) has often been cited as a significant factor in the spread of HIV in SSA, but again there is little credible evidence showing a causal link between income, sexual behavior, and HIV risk. Given the higher prevalence of HIV infection among young women in SSA (3.2% among women aged 15-24 vs. 1.1% among men in the same age group (UNAIDS, 2008)), the policy importance of identifying any potentially large impacts of CCT programs for schooling on sexual behavior cannot be overstated.

This paper aims to provide new causal evidence on the effects of a CCT program (with **only** school attendance used as a condition to receive the transfers) on the self-reported sexual behavior of the young, female beneficiaries of the program. It does so by examining the one-year impacts of an ongoing two-year randomized intervention in Malawi that provides cash transfers to young women to stay in (or return to) school. Conditional cash transfers blend 'income' effects (through the transfers themselves) with 'price' effects (through the conditionality), and our study estimates the joint effect of these two policy parameters. As such, this paper provides the first experimental evidence

[4] We know of one other study (Duflo et. al., 2006) that examines the effect of providing school uniforms on the likelihood of teen marriage and childbearing. The implied transfer size for that study is significantly lower than the transfers in the program being evaluated here.

on the impact of a CCT program for schooling on age at first marriage, childbearing, frequency of sexual activity, and risky sexual behaviors in Sub-Saharan Africa.

The remainder of this paper is structured as follows. Section 2 provides a brief review of the literature on conditional cash transfer programs and on the relationship between schooling, sexual behavior and HIV risk. Section 3 describes the survey setting and why this study is particularly pertinent for Malawi, while Section 4 details the research design and the intervention. Section 5 describes the impact of the program and Section 6 concludes.

## 2. Literature Review

CCT programs are utilized around the world with two main objectives: to provide poor households with a minimum threshold of income (reduce poverty in the very short-run) and to improve the accumulation of human capital for the next generation (reduce poverty in the longer-run). There is a large body of evidence supporting the success of CCTs throughout most of the developing world, particularly in relationship to schooling (de Janvry and Sadoulet, 2004; Schultz, 2004).[5] Moreover, several evaluations show that these programs are both technically feasible (i.e. the stated goals of the program are actually met in practice) and are politically acceptable in that successive governments are willing to continue and even expand program coverage (Das, Do, Özler, 2005).

CCTs targeted at education generally consist of giving cash to poor parents under the condition that they send their children to school. Households are generally targeted using means testing based on observable characteristics. In Mexico's Progresa, for example, cash transfers were offered to poor mothers in rural communities conditional on

[5] See World Bank (2009) for a recent and thorough examination of CCT programs.

their children using health facilities on a regular basis and attending school between the third year of primary school and the third year of secondary school (de Janvry and Sadoulet, 2004).

World Bank (2009) finds that CCTs led to large increases in school enrolment, particularly among those with low enrolment rates to begin with. However, evidence on the impact of educational transfer programs (in kind or in cash) on ‘final outcomes’ such as test scores, is not as encouraging (Miguel and Kremer, 2004; Glewwe, Kremer, and Moulin, 2008). Filmer and Schady (2009) argue that the lack of any discernible effect of such programs on learning (despite large impacts on school enrolment) may be due to the fact that they draw lower ability students back to school.

To our knowledge, no CCT program for schooling has been evaluated to assess its possible impact on the sexual behavior of the young people benefiting from the program. This is the case even though there are good reasons to think that the impacts of such programs on the sexual behavior of young people may be substantial. The World Bank (2009) argues that among the areas that should receive high priority in impact evaluations (and, more generally, research) on CCTs is the role they play in reducing the transmission of HIV, most likely through changes in sexual behavior. Both schooling and poverty reduction (especially for women) are seen by many as key components in a comprehensive strategy to combat HIV/AIDS. However, causal evidence that links increased schooling or income to changes in behaviors associated with HIV is very limited. Most of what we know about the relationship between schooling (attendance or attainment) or income and HIV risk factors come from cross-sectional studies.

While several studies find a positive, cross-sectional correlation between school attendance and HIV status (e.g. Hargreaves et. al., 2008; Beegle and Özler, 2007), there is only one study that points to a possible causal link between school attendance and reduced HIV risk factors. A study in Kenya finds that reducing the cost of schooling (by paying for uniforms) reduced dropout rates, teen marriage, and childbearing (Duflo et. al. 2006). Commenting on the lack of clear and credible evidence addressing the relationship between education and HIV, Jukes, Simmons, and Bundy (2008) suggest that long-term, follow-up experimental interventions to improve educational access, such as conditional cash transfer programs, offer the potential to examine the causal relationship between educational attainment, sexual behavior and subsequent risk of HIV infection.

Credible causal evidence regarding the effect of increased income on risky sexual behavior among young people is also practically non-existent. The evidence on whether poorer individuals are more likely to conduct risky sexual behavior and contract HIV, virtually all of which is cross-sectional, is mixed. Many are quick to assert that poverty is a determinant of HIV status for women because poor women are more likely to engage in risky sexual activities, such as commercial or informal sex work (Wojcicki, 2002; World Bank, 2005; Shelton, Cassell, and Adetunji, 2005), have multiple partners (Wines, 2004; Halperin and Epstein, 2004; Hallman, 2004), or have riskier types of sex for money (Robinson and Yeh, 2006). On the other hand, Swidler and Watkins (2007) argue that it's not women's poverty but the relative wealth of men that is the cause of transactional sex, and as such improving women's economic circumstances are unlikely to decrease women's vulnerability to HIV infection.

However, many of the same sources asserting the plausibility of the relationship between poverty and HIV are puzzled to report evidence to the contrary. For example, Shelton, Cassell, and Adetunji (2005) report a positive correlation between household possessions and HIV prevalence in Tanzania. Examining the determinants of HIV in five countries with DHS data in sub-Saharan Africa, De Walque (2006) finds that wealth (measured by an asset index) is positively correlated with HIV status in three of the five countries, especially for females.[6] Finally, using prime-age adult mortality as a proxy measure for HIV/AIDS affected households; several studies find that higher income households are more likely to suffer an adult death (Yamano and Jayne, 2004; World Bank, 2006, among others).

HIV/AIDS is an important problem in sub-Saharan Africa, especially among young women. CCT programs are now starting to be seriously considered for implementation in the region, with a number of countries piloting or implementing such schemes. There are good reasons to think that such programs may play a role in affecting the incidence of HIV infection among young people through behavioral change, as sexual behavior might reasonably be linked to both schooling and income. However, credible empirical evidence is lacking to establish such a causal relationship. This paper, which reports the findings from a prospective evaluation of a randomized CCT program, is well-suited to attempt to fill some of this knowledge gap.

## 3. Study Setting

Malawi, the setting for this research project, is an impoverished small country in southern Africa. Its population of almost 14 million in 2007 is overwhelmingly rural,

[6] De Walque and Corno (2007) report a similar positive conditional correlation in Lesotho.

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: the GNI per capita (PPP, current international $) is $750 in 2007, compared to an average of $1,870 for sub-Saharan Africa (World Bank, 2008).[7] Malawi also has the eighth-highest HIV prevalence in the world with 14 percent of the adult population infected (UNAIDS, 2007).[8] The gender gap in HIV prevalence among young adults, aged 15-24, is startling: prevalence was more than *four* times higher for females than males in 2004.

Primary schools in Malawi are free through Standard 8, and upon completing this grade, students take the Primary School Leaving Certificate (PSLC) exam. Access to secondary schools is rationed, meaning that only those who obtain sufficiently high scores on the PSLC will gain access, and only if they can pay the tuition fees. Secondary students sit one exam after two years (the Junior Certificate Exam, or JCE) and a final exam on completion of secondary school (the Malawi Schools Certificate Exam, or MSCE). Each of these exams is nationally recognized and considered an important qualification on the labor market.

The CCT intervention that is the subject of this paper takes place in one district of Malawi, which both reduces project costs (lower fixed costs of office infrastructure and transport) and increases data quality through more careful supervision. Zomba district in the Southern region of Malawi was chosen as the site for this study for several reasons. First, it has a large enough population within a small enough geographic area rendering

[7] Using the Atlas method, The GNI per capita (in current US$) in Malawi is 250 in 1997, compared with 952 in sub-Saharan Africa as a whole.
[8] The UNAIDS HIV estimate of 14.1 percent is close to the Demographic and Health Survey 2004 (NSO, 2005) estimate of 12.7 percent (National Statistical Office and ORC Macro, 2005).

field work logistics easier and keeping transport costs lower. Second, characteristic of Southern Malawi, Zomba has a high rate of school dropouts and low educational attainment. Finally, HIV/AIDS rates of women aged 15-49 in Zomba are the highest in the country at 24.6% (NSO, 2005).

Because of Zomba district's particular characteristics with respect to its relatively high poverty and HIV prevalence, one might worry that the findings from this study may not be relevant for other parts of Malawi or for neighboring countries. We feel that several factors mitigate these concerns. First, while Zomba district may be different than the rest of the country, it certainly is quite representative of the Southern Region (one of the three major regions of Malawi), which is home to two of the country's three biggest cities (Blantyre and Zomba). As the Southern Region is the poorest one in the country with low educational outcomes and high HIV rates, it would be a natural place for the government to implement a similar program were it to consider geographic targeting. Second, unlike many other districts, Zomba has the advantage of having a true urban center as well as rural areas.[9] Finally, Zomba and the Southern region of Malawi are not atypical of the many other environments in Southern Africa which share high poverty and HIV rates. Therefore, while we study a particular population (never married girls, aged 13-22 at baseline), we feel confident that our results are meaningful for this population in other contexts in Southern Africa.

## 4. Research Design and Intervention

[9] The study sample was stratified to get random representative samples from urban areas (Zomba town), rural areas near Zomba town, and distant rural areas in the district.

This paper is evaluating the impact of a randomized conditional cash transfer intervention targeting young women in Malawi that provides incentives (in the form of school fees and cash transfers) to current schoolgirls and young women who have recently dropped out of school to stay in or return to school.[10] Between October 2007 and January 2008, baseline surveys were conducted with 3,805 girls in 176 Enumeration Areas (EAs) in Zomba district of Malawi. These EAs were selected from the universe of EAs produced by the National Statistics Office of Malawi from the 1998 Census. The sample of EAs was stratified by distance to the nearest township or trading centre. The random sample of 176 EAs consists of 29 EAs in Zomba town, 8 trading centers in Zomba rural, 111 population areas within 16 kilometers of Zomba town, and 28 EAs more than 16 kilometers from Zomba town.

The 3,805 girls were selected based on information collected during a listing exercise, which involved going door to door to *all* households in these 176 EAs. This listing exercise identified all never-married, 13-22 year-old females living in the area. This age group was chosen because it represents the period during which school dropout coincides with the onset of sexual activity, and therefore maximizes the power for a causal study of the relationship between these two phenomena. For the study, we sampled **all** dropouts and 75-100% of current school girls, where the exact percentage sampled depended on the age of the school-girl; regression results are re-weighted to make them representative of the entire eligible frame.[11] A baseline 'dropout' was any girl or young

[10] We selected only females for the treatment because the expected HIV prevalence among males of the same age cohort is much smaller, making it unlikely that we could detect program impacts with any kind of precision. The HIV prevalence for males aged 15-24 is 2.7%, compared with 8.4% for females of the same age (UNAIDS, 2008).

[11] These percentages were lower for urban areas since average population in urban areas is much higher than that in rural areas.

woman, who was out of school at baseline – regardless of how long she had been out of school prior to the study as long as she had never been married and was between the ages of 13-22. This sampling procedure led to an average sample size of 5.1 dropouts and 16.6 school girls at baseline in each EA.[12]

Out of these 3,805 young women, 1,225 girls in 88 randomly selected EAs were sampled to be part of the CCT program.[13] Treatment status was assigned at the EA level and therefore we cluster all standard errors to reflect this design effect. A household questionnaire (not unlike a Living Standards Measurement Survey, or LSMS) was administered to our entire core sample – both treatment and control – at baseline and follow-up, which were conducted 12 months apart. This survey, described in more detail below, includes information on household characteristics, school enrolment, sexual behavior, and social networks.

From December 2007 through January 2008 offers to participate in the CCT program were made to the selected girls in treatment villages. Of the 942 girls in the baseline survey to whom we attempted to make *conditional* offers, 17 could not be located, 32 were deemed ineligible subsequent to the offer process, and one refused. Because we continue to code all 50 of these 'non-compliers' as treated, we effectively estimate the Intention to Treat Effect of the original intent to offer the treatment[14]. As

[12] We chose to target these two groups separately to ensure that we had a significant number of dropouts in our sample. Treating all dropouts gives our study the statistical power to focus on a sub-population whose school enrolment rates are more sensitive to the offer to participate in the program.

[13] 283 of these girls resided in EAs where the offers for baseline schoolgirls were not *conditional* on school attendance, and, as such, are not part of the analysis for this paper. In addition, 629 girls in treatment EAs were randomly selected to not receive cash transfers in order to examine possible spillover effects of the program. These girls are also not part of the analysis in this paper. This leaves us with a final sample size of 2,893 girls at baseline, for whom the treatment is always *conditional* on school attendance.

[14] Due to uncertainties regarding funding, the initial offers were only made for the 2008 school year. However, upon receipt of more funds for the intervention in April 2008, all the girls in the program were

part of the offer, a detailed informational sheet was given to each household that described the conditions of the contract. This information sheet also informed recipients that secondary school fees would be paid directly to their school in full.[15] The contract was then signed by both the recipients (parent/guardian and core respondent) and the NGO delivering the funds.

The average offer to the households consisted of $10/month – for a total of $100 for the school year transferred in equal amounts for 10 months.[16] $10/month represents roughly 15% of total monthly household consumption in our sample households at baseline, which places this program in the middle-to-high end of the range of relative transfer sizes for conditional cash transfer programs elsewhere.[17] In addition to the transfers to the household, secondary school fees were paid directly to the schools upon confirmation of enrolment.[18] The transfer was split into a component that went to the student's guardian and a component that went directly to the girl herself, with an average of 30% of the transfer given to the schoolgirl.

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informed that the program would be extended to cover the 2009 school year and that they could stay in the program upon satisfactory school attendance .

[15] This was the case only for public schools. An upper limit for school fee payments was established for those attending private schools, which was set to equal the average public school fees in the program sample.

[16] The intervention being evaluated here is part of a larger experiment with multiple treatment arms, wherein transfer size was randomly varied across treatment units as well as the transfers randomly split between parents and the young women. To measure possible spillover effects, the percentage of young women treated in each EA was also randomly varied. Finally, baseline schoolgirls in a randomly selected small percentage of the EAs received *unconditional* offers, meaning that the transfers were not conditional on school attendance, or any other behavior other than showing up to collect monthly payments, for these beneficiaries in those EAs. The analysis of the heterogeneity of the impacts with respect to each of these design features is beyond the scope of this paper. Here, we aim to establish the average effect of the conditional treatment arms, which may not equal the treatment effect of the average treatment if these impacts are nonlinear. For a more detailed discussion of this contract variation, see Baird et al (2009).

[17] For example, Cambodia transfers as little as 2-3% of total monthly household consumption under its CESSP Scholarship Program (Filmer and Schady, 2009), while Mexico provides over 20% under Progresa.

[18] Students have to pay school fees at the secondary level in Malawi, but not at the primary level.

### *4.1. Implementation of transfers*

The cash payments take place monthly at centrally located and well-known places, such as churches and schools. The cash transfer points were selected so that no recipient has to travel for more than 5 kilometers to the cash payment point.[19] At each meeting some basic information is collected for each sample respondent, such as who is picking up the money (girl or parent/guardian), how far they had to travel, etc. In between payment dates, the NGO collects attendance records for all the students in the program to make sure that they are complying with the program requirements and attending school.[20] Each household receives the transfer only if the young woman attended school for at least 75% of the days that their school was in session in the previous month.

### *4.2. Survey Instrument*

The annual SIHR Household Survey consists of a multi-topic questionnaire that is administered to the households in which the selected sample respondents reside. Although it is described as a household questionnaire, the primary goal of the SIHR Household Survey is to collect detailed information from the individual respondents selected for the survey. The survey consists of two parts: Part I is administered to the head of the household, while Part II is administered to the core respondent, i.e. the

[19] Some recipients who still live in locations that are remote are visited door-to-door by the NGO implementing the transfer scheme.

[20] The total cost of the program consists of the cash transfers themselves, as well as the administrative costs of running the program. For every $1 that is transferred to a program beneficiary, approximately $0.50 is spent on administrative costs. The main items under the administrative costs are delivering the cash payments and monitoring attendance, both of which are underlined by large costs of transportation. We estimate that a similar program implemented by the government itself would spend significantly less on administrative costs. This is because the cash transfers could be conducted at schools and the program administers could rely on school records (with spot checks) to monitor attendance, significantly reducing transport costs and producing scale economies. Furthermore, the government would benefit from collecting less research-oriented data during cash transfers, which takes significant time to collect and enter.

sampled girl from our target population. Part I collects information on the household roster, dwelling characteristics, household assets and durables, consumption (food and non-food), household access to safety nets, and shocks (economic, health, and otherwise) experienced by the household. In Part II, the core respondent provides further 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.). This paper utilizes baseline and follow-up data to analyze the one-year impact of the program on the marital status, childbearing, and the detailed sexual behavior for the program participants. The first round of the survey was administered from October 2007 to February 2008, and the second wave from October 2008 to February 2009.

## 5. Program Impacts

### 5.1. Balance and Attrition

Before examining the short-term impacts of the CCT program on sexual behavior, it is important to first confirm that our randomization, with respect to key outcomes and controls, was successful. Table 1a provides some basic summary statistics from baseline data. Table 1b shows the success of the randomization. As per our research design, we always compare treatment and control groups for dropouts and schoolgirls at baseline separately, and hence the equality of means at baseline is also examined within each of these two important sub-groups. Across the thirteen variables that are most pertinent for this paper, there are no significant differences at baseline between the treatment and control groups for those who were dropouts at baseline. Among baseline school girls, the only variable that is significantly different between treatment and control is whether the girl resides in a female headed household, where those in treatment are significantly less likely to reside in a female headed household. The fact that these variables look very similar across treatment and control is strong evidence that the randomization procedure was implemented successfully.[21]

Table 2 shows that the success rate in tracking our respondents in the study sample was more than 93% in the one-year follow-up. Since the small attrition from the panel data is balanced across treatment and control groups, it will not introduce any bias into the estimation of treatment impacts. Given these attrition rates, we move to the analysis with a panel sample consisting of 396 treatment and 408 control girls who had dropped out of school as of baseline, and 480 treatment and 1,408 control girls in school as of baseline. This gives us a total sample size of 2,692.

We now turn to the impacts of the program, using a standard difference-in-difference estimation strategy. The specification used for estimation is:

$$Y_{idt} = \alpha_i + \delta_t + \beta(T_d * \delta_t) + \varepsilon_{idt},$$

where *i* indexes individuals, *d* indexes Enumeration Areas, and *t* indexes each of the two waves of the survey. Then $\alpha_i$ represents a set of individual-level fixed effects, $\delta_t$ is a dummy variable for the second round, and the interaction term $(T_d * \delta_t)$ is another dummy variable that is equal to unity only for units offered the treatment in the second round. Standard errors are clustered at the EA (village) level because this is the unit at

[21] The reader may also take note of the significant differences between schoolgirls and dropouts at baseline in Table 1. Dropouts at baseline are older, less literate, and more likely to have started childbearing. As described in Section 4, the intervention is randomly assigned within each of these two strata.

which the treatment is administered (see Bruhn & McKenzie, 2008), and observations are weighted by the inverse of their village-level probability of being sampled. The coefficient $\hat{\beta}$ therefore gives the intention-to-treat effect of the program on the average girl in our study EAs.[22]

### 5.2. School Enrolment

We start by showing the impact of the program on self-reported schooling outcomes since we would be much less likely to find impacts on early marriage, fertility, and the sexual behavior of the young beneficiaries of the CCT program in the absence of any program impacts on school attendance and attainment. The simple act of attending school may be enough to cause sexual behavior change among the study beneficiaries – for example by raising the opportunity cost of pregnancy (Jukes, Bundy, and Simmons, 2008; Duflo et. al. 2006).[23]

Table 3 shows that the program led to large increases in school enrolment, especially among those who were not in school at baseline. Column 2 of Table 3 shows that the percentage of initial dropouts who returned to school (and were in school at the end of the 2008 school year) was 17.2% among the control group compared with 61.4%

22 While the transfer amounts were randomly varied for the parents (from \$4/month to \$10/month across EAs) and the students (from \$1/month and \$5/month within each EA), here we present the average impacts of these heterogeneous treatments for the sample that received the transfers conditional on school attendance. Under a linearity assumption, this average effect will give the intention-to-treat effect (ITE) of the average CCT amount (\$10/month) and the average share of the transfer going to the girl (30%).

[23] What is learned at school and schooling attainment can also influence sexual behavior of young people through a variety of channels. While we find some evidence that baseline dropouts show a significant improvement in self-reported literacy in English, we don't find any evidence that their knowledge of HIV/AIDS or their likelihood of being tested for HIV improved. We conclude that the reduction in self-reported sexual activity in the upcoming sub-sections is likely not a result of what is learned at school, but the incentives associated with staying in school. This is consistent with Duflo et al. (2006) who suggest that young women want to delay childbearing and marriage until after they complete their desired level of schooling.

among treatment. Thus, program beneficiaries were 3-4 times more likely to be in school at the end of the 2008 school year than the control group.[24] Since those enrolled in school actually report attending school over 90% of the time, these treatment effects on enrolment do translate into improvements in actual days of school attended.

For the stratum containing baseline schoolgirls, i.e. those who were still in school at baseline, while the absolute numbers are smaller (due to high rates of continued schooling among this group), the relative impact is still impressive (column 3). Among the control group, 89.1% of initial schoolgirls were still enrolled in school at the end of the 2008 school year, compared with 93% in the treatment group. Thinking of these as dropout rates, the CCT program reduced the dropout rate among this group by 35% -- from 10.9% among controls to 7% among treatments.

### 5.3. Marriage and Fertility

We now turn to early marriage and teen pregnancy as indicators of sexual activity.[25] Table 4 presents the impact of the program on having **never** been married. Early marriage increases coital frequency, significantly decreases condom use, and virtually eliminates the ability to abstain from sex (Clark, 2004). As described earlier, the study sample was selected to be never-married at baseline, so levels of marriage are equal to the incidence during 2008. We see that 27.7% of initial dropouts in the control group have gotten married during the past year, compared with only 16.4% of the same group in

[24] The school enrolment and attainment data are self-reported by the study respondents. However, the school enrolment and attendance of program beneficiaries, i.e. the treatment group, was monitored as part of the program and can be confirmed. Full enrolment, attendance, school grades, and performance at national examinations will become available for the entire study sample after we complete conducting a school census in Zomba between February and May, 2009.

[25] The reader may object to marriages in this study being described as ‘early’ and pregnancies as ‘teenage’. While it is true that the study sample does include some over the age of 19, this is a small percentage (less than 12% at the end of Year 1) of the sample.

treatment (column 2). This is a reduction in the marriage rate of more than 40% among those who were not in school at baseline. However, we also note that the program had no effect on the propensity to get married among the baseline schoolgirls – 4.7% of whom got married both among the controls and treatments.

Table 5 describes the impact of the program on the incidence of childbearing – i.e. the likelihood of ever being pregnant. Column 2 shows that baseline dropouts among the treatment group are 5.1 percentage points less likely to have become pregnant over the past year, a reduction of more than 30% that is statistically significant at the 5% level. Again, as with marriage, the CCT program had no impact on the incidence of childbearing at follow-up for baseline schoolgirls.

### 5.4. Sexual Activity and Risk Behaviors:

Finally, we present impacts on self-reported sexual activity and risky behaviors. Table 6a examines onset of sexual activity and the number of sexual partners in the past 12 months. At baseline, 29.6% of initial dropouts and 79.4% of initial schoolgirls reported having **never** had sex. Columns 2-3 of Table 6a indicate that the reduction in the onset of sexual activity is 5.5 percentage points among initial dropouts (significant at the 1% level) and 2.5 percentage points among initial schoolgirls (with a p-value of 0.112), which represent reductions in the onset of sexual activity of 46.6% and 31.3%, respectively. Columns 5-6 complement this finding and show that the change in the number of self-reported number of lifetime partners from baseline to follow-up is smaller for the program beneficiaries. The decrease in the number of lifetime partners is approximately 25% lower for both initial dropouts and schoolgirls, although the difference is only statistically significant among baseline dropouts. These results suggest that program beneficiaries reduce their (self-reported) sexual activity by both delaying sexual activity and reducing the number of sexual partners. Table 6b extends the analysis by examining the sub-sample of young women who haven't yet gotten married by Round 2 and finds similar impacts – suggesting that the reduction in self-reported sexual activity is not due solely to delayed marriage in this population.

Table 7 reports the impact of the program on the sexual behavior of those who are sexually active at both baseline and follow-up: condom use, frequency of sexual activity, and having sex with older partners. As the program has both an effect on the extensive margin, i.e. on being sexually active in follow-up, and on the intensive margin, i.e. the safety of the sexual activity conditional on being sexually active, we face an identification problem for the latter. Hence, we ask the following question: "For the population of young women who would be sexually active in the absence of the program, what would the effect of the program have been on their sexual behavior?" However, the young women we observe to be sexually active in both rounds include both this group, and the group who would have stopped being sexually active had they received the intervention, which introduces a selection bias that prevents us from interpreting the simple difference-in-differences estimates that are presented in Table 7 as the marginal effect of treatment on the population in question.

In columns 1-3 of Table 7, we examine self-reported condom use and find no discernible impact of the program. In columns 4-6, we present the likelihood of having sexual intercourse at least once a week. We find that treatment baseline schoolgirls are significantly less likely to have sexual intercourse on a weekly basis, but we find no significant impact for baseline dropouts. Similarly, the likelihood of having an older sexual partner is lowered significantly for baseline schoolgirls in treatment (columns 7-9). If we believe that the treatment girls who stopped having sex had a lower propensity to engage in risky sexual behaviors, then the protective effects of the program found here are likely to be stronger, and vice versa.[26]

## 6. Conclusions

While there have been several evaluations of the impact CCT programs have on school attainment, learning, early childhood development, as well as adult health, no one, to our knowledge, has studied the possible effect of these programs on the sexual behavior of the young target beneficiaries. This is potentially a very important impact to document in sub-Saharan Africa, where CCT programs are likely to become more common in the near future and the risk of HIV infection is disproportionately high among young women and school-aged girls.

Causal evidence that links increased schooling or income to changes in sexual behavior is very limited. While most of what we know about these relationships comes from cross-sectional studies, the existing evidence is still at least suggestive of the possibility that CCT programs for schooling may also affect the sexual behavior of their young beneficiaries. This paper aims to shed some light on this question by analyzing the short-term impacts of such a randomized CCT program implemented in Malawi.

The results are promising. After one year, the program led to large increases in self-reported school enrolment, as well as declines in early marriage, teenage pregnancy,

[26] We have also tried to 'bound' our estimates using "Lee bounds" (Lee, 2005) to deal with this selection effect, but as the impact of the program on the extensive margin is substantial, the bounds are too wide to be useful.

sexual activity, and risky sexual behavior. The evidence presented here suggests that as girls and young women returned to (or stayed in) school, they significantly delayed the onset (and, for those already sexually active, reduced the frequency) of their sexual activity. The program also delayed marriage – which is the main alternative for schooling for young women in Malawi – and reduced the likelihood of becoming pregnant. As the treatment/control differences in schooling become starker during the second year of the program, the treatment impacts on marriage, fertility, and risky sexual behavior are likely to become stronger.

One should not assume that the changes in self-reported sexual behavior will result in a decline in HIV incidence among this cohort of program beneficiaries. Future rounds of household survey and Biomarker data collection will shed light on these questions. For now, however, schooling CCTs for young women in the context of poor sub-Saharan countries with high HIV rates seem like "win-win" programs, as they may not only increase schooling for young women, but also significantly reduce their risk of HIV infection. Furthermore, increases in age at first marriage and pregnancy, as well as improved educational attainment may lead to improved outcomes for the next generation, as there are a host of negative externalities for children that are associated with early marriage, such as higher child mortality or lower educational attainment (Morrison and Sabarwal, 2008). The evidence presented in this paper provides impetus for the expansion of CCT programs (which already cover much of Latin America) to Sub-Saharan Africa.

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Table 1a: Summary Statistics (N=2893)

| | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| Age | 15.611 | 2.235 | 12 | 22 |
| Father Alive | 0.692 | 0.462 | 0 | 1 |
| Mother Alive | 0.808 | 0.394 | 0 | 1 |
| Female Headed | 0.338 | 0.473 | 0 | 1 |
| Age Household Head | 47.133 | 13.083 | 13 | 110 |
| Household Size | 6.333 | 2.300 | 1 | 15 |
| Muslim | 0.187 | 0.390 | 0 | 1 |
| Urban Household | 0.358 | 0.480 | 0 | 1 |
| Read English | 0.741 | 0.438 | 0 | 1 |
| No Qualification | 0.669 | 0.471 | 0 | 1 |
| Ever pregnant | 0.107 | 0.309 | 0 | 1 |
| Never had sex | 0.694 | 0.461 | 0 | 1 |
| Number of partners | 0.449 | 0.809 | 0 | 6 |

Table 1b: Equality of Means at Baseline

| | Dropouts (N=889) | | School Girl (N=2003) | |
|---|---|---|---|---|
| | Control Mean | Treatment Difference | Control Mean | Treatment Difference |
| Age | 17.434 | -0.301 | 15.249 | -0.199 |
| Father Alive | 0.646 | 0.004 | 0.696 | 0.019 |
| Mother Alive | 0.784 | -0.038 | 0.834 | -0.042 |
| Female Headed | 0.420 | -0.004 | 0.351 | -0.095*** |
| Age Household Head | 46.855 | -0.502 | 47.775 | -1.323 |
| Household Size | 6.091 | 0.011 | 6.433 | -0.105 |
| Muslim | 0.225 | -0.006 | 0.194 | -0.045 |
| Urban Household | 0.194 | -0.010 | 0.362 | 0.110 |
| Read English | 0.469 | -0.065 | 0.829 | -0.024 |
| No Qualification | 0.673 | 0.012 | 0.659 | 0.021 |
| Ever pregnant | 0.436 | -0.020 | 0.021 | 0.008 |
| Never had sex | 0.309 | -0.017 | 0.794 | 0.008 |
| Number of partners | 1.135 | 0.031 | 0.270 | -0.015 |

Notes: The entire sample was never married at baseline, so the control and treatment means were both zero. Dropout and school girl refer to schooling status at baseline. The sample was split into dropouts (girls not in school) and school girls at baseline, so the control and treatment means of schooling status were identical at baseline (dropouts were 100% not in school while school girls were 100% in school). These means are weighted to make results representative of all study EAs. Standard errors are clustered at the EA level.
*Denote significance at the 10% level, ** at the 5% level and *** at the 1% level

Table 2: Determinants of Survey Attrition

| | ALL | Dropouts | School Girls |
|---|---|---|---|
| =1 if Treatment Girl | -0.001 | 0.010 | 0.008 |
| | (0.012) | (0.020) | (0.013) |
| Tracking Success | 0.931*** | 0.899*** | 0.941*** |
| | (0.006) | (0.013) | (0.007) |
| Number of observations | 2,893 | 890 | 2,003 |

Note: Each column represents an OLS regression with robust standard errors. Standard errors in parentheses.

*Denotes significance at the 10% level, ** at the 5% level and *** at the 1% level

Table 3: Dependent Variable is Enrolled in School

| | All | Dropouts | School Girls |
|---|---|---|---|
| Post-Treatment Indicator | 0.124*** | 0.442*** | 0.038** |
| | (0.018) | (0.035) | (0.019) |
| Round 2 Indicator | -0.141*** | 0.172*** | -0.109*** |
| | (0.013) | (0.020) | (0.013) |
| Baseline mean of outcome in control | 0.832 | 0.000 | 1.000 |
| Number of observations | 5,384 | 1,608 | 3,776 |
| Number of individuals | 2,692 | 804 | 1,888 |

clustered at the EA level, and are weighted to make results representative of all study EAs. Standard errors in parentheses. The first column controls for schooling status at baseline.

*Denotes significance at the 10% level, ** at the 5% level and *** at the 1% level

Table 4: Dependent Variable is Never Married

| | **All** | **Dropouts** | **School Girls** |
|---|---|---|---|
| Post-Treatment Indicator | 0.023* | 0.113*** | -0.001 |
| | (0.013) | (0.027) | (0.013) |
| Round 2 Indicator | -0.056*** | -0.277*** | -0.047*** |
| | (0.008) | (0.019) | (0.008) |
| Baseline mean of outcome in control | 1.000 | 1.000 | 1.000 |
| Number of observations | 5,384 | 1,608 | 3,776 |
| Number of individuals | 2,692 | 804 | 1,888 |

Note: All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs. Standard errors in parentheses. The first column controls for schooling status at baseline.

*Denotes significance at the 10% level, ** at the 5% level and *** at the 1% level

Table 5: Dependent Variable is Ever Pregnant

| | **All** | **Dropouts** | **School Girls** |
|---|---|---|---|
| Post-Treatment Indicator | -0.011 | -0.051** | -0.001 |
| | (0.013) | (0.024) | (0.015) |
| Round 2 Indicator | 0.073*** | 0.162*** | 0.070*** |
| | (0.008) | (0.016) | (0.008) |
| Baseline mean of outcome in control | 0.093 | 0.444 | 0.022 |
| Number of observations | 5,382 | 1,608 | 3,774 |
| Number of individuals | 2,691 | 804 | 1,887 |

Note: All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs. Standard errors in parentheses. The first column controls for schooling status at baseline.

*Denotes significance at the 10% level, ** at the 5% level and *** at the 1% level

Table 6a: Sexual Activity

| **Dependent Variable:** | =1 if Never Had Sex | | | Number of partners in the past 12 months | | |
|---|---|---|---|---|---|---|
| | **All** | **Dropouts** | **School Girls** | **All** | **Dropouts** | **All School Girls** |
| Post-Treatment Indicator | 0.031** | 0.055*** | 0.024 | -0.053** | -0.112** | -0.038 |
| | (0.013) | (0.020) | (0.015) | (0.027) | (0.048) | (0.029) |
| Round 2 Indicator | -0.082*** | -0.118*** | -0.080*** | 0.176*** | 0.428*** | 0.170*** |
| | (0.010) | (0.016) | (0.010) | (0.015) | (0.031) | (0.015) |
| Baseline mean of outcome in control | 0.709 | 0.302 | 0.791 | 0.413 | 1.120 | 0.270 |
| Number of observations | 5,382 | 1,606 | 3,776 | 5,382 | 1,606 | 3,776 |
| Number of individuals | 2,691 | 803 | 1,888 | 2,691 | 803 | 1,888 |

Table 6b: Sexual Activity for Unmarried Subsample

| **Dependent Variable:** | =1 if Never Had Sex | | | Number of partners in the past 12 months | | |
|---|---|---|---|---|---|---|
| | **All** | **Dropouts** | **School Girls** | **All** | **Dropouts** | **All School Girls** |
| Post-Treatment Indicator | 0.023** | 0.032* | 0.021 | -0.041 | -0.076* | -0.033 |
| | (0.011) | (0.017) | (0.013) | (0.025) | (0.045) | (0.029) |
| Round 2 Indicator | -0.063*** | -0.073*** | -0.062*** | 0.145*** | 0.326*** | 0.142*** |
| | (0.008) | (0.014) | (0.008) | (0.014) | (0.029) | (0.014) |
| Baseline mean of outcome in control | 0.735 | 0.310 | 0.805 | 0.366 | 1.090 | 0.247 |
| Number of observations | 4,882 | 1,320 | 3,562 | 4,882 | 1,320 | 3,562 |
| Number of individuals | 2,441 | 660 | 1,781 | 2,441 | 660 | 1,781 |

Note: All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs. Standard errors in parentheses. The first column controls for schooling status at baseline.

*Denotes significance at the 10% level, ** at the 5% level and *** at the 1% level

Table 7: Risky Sexual Activity

| Dependent Variable: | Average Condom Use | | | =1 if Sexually Active at Least Once a Week | | | Share of Partners who are at Least One Year Older | | |
|---|---|---|---|---|---|---|---|---|---|
| | All | Dropouts | School Girls | All | Dropouts | School Girls | All | Dropouts | All School Girls |
| Post-Treatment Indicator | -0.088 | -0.254 | 0.039 | -0.136* | -0.048 | -0.204* | -0.071 | 0.043 | -0.159* |
| | (0.284) | (0.266) | (0.463) | (0.075) | (0.088) | (0.106) | (0.062) | (0.079) | (0.095) |
| Round 2 Indicator | 0.079 | 0.356** | 0.031 | 0.067 | 0.178*** | 0.093 | 0.023 | -0.035 | 0.057 |
| | (0.194) | (0.174) | (0.201) | (0.054) | (0.063) | (0.058) | (0.046) | (0.049) | (0.053) |
| Baseline mean of outcome in control | 2.842 | 2.389 | 3.150 | 0.162 | 0.251 | 0.102 | 0.235 | 0.581 | 0.165 |
| Number of observations | 671 | 351 | 320 | 671 | 351 | 320 | 672 | 352 | 320 |
| Number of individuals | 336 | 176 | 160 | 336 | 176 | 160 | 336 | 176 | 160 |

Note: All regressions use individual fixed effects with standard errors clustered at the EA level, and are weighted to make results representative of all study EAs. Standard errors in parentheses. The first column controls for schooling status at baseline.

*Denotes significance at the 10% level, ** at the 5% level and *** at the 1% level