# Designing Cost-Effective Cash Transfer Programs to Boost Schooling in Sub-Saharan Africa

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Abstract

As of 2007, 29 developing countries had some type of Conditional Cash Transfer (CCT) program in place, while many others were planning or piloting one. However, 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. 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. The program features four independently randomized dimensions of contract variation: the conditionality, transfer size, schoolgirl/parent transfer splits, and village-level saturation of treatment are all experimentally varied. Despite this rich heterogeneity in contract terms, we find large program impacts that are surprisingly binary. While 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 generally similar regardless of the specific contract terms. If the one-year impacts were to persist, they would indicate that a bare-bones *unconditional* cash transfer program using low monthly transfers, at least some of which are directly transferred to the children would be the most cost-effective way to increase enrollment in this population.

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## 1. INTRODUCTION

A large and empirically well-identified body of evidence has demonstrated the ability of Conditional Cash Transfer programs (CCTs) to raise schooling rates in the developing world. 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 schooling 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*

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[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 little additional impact from increased transfer size or conditionality. However, there is some evidence that schooling outcomes improve as the share of the total transfer that is given directly to the girl/young woman increases. 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

[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. These randomly determined shares of schoolgirls that

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

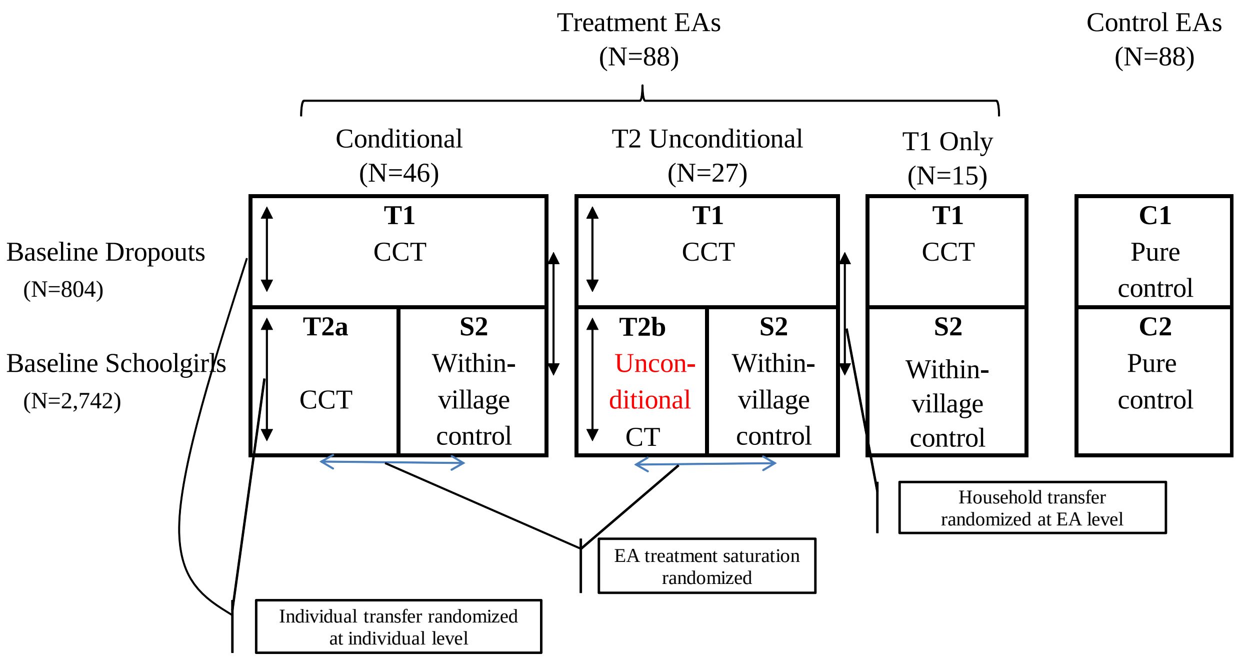
[8] We chose to target these two groups separately to ensure that we had a significant number of dropouts in our sample. Treating all dropouts allows us to focus on a subpopulation whose schooling rates are extremely sensitive to transfers.

[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.

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. 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.[13] 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 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.

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

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. 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.[14] 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 almost a 50% decrease in dropout from the control rate of 10.8%. Treatment effects on self-reported literacy are more muted, and statistically significant only among dropouts. Hence these results conform to a large body of evidence showing that the dramatic influence of CCT programs on attendance is not accompanied by similar improvements in *learning.*[15]

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

[14] The literacy is a dummy variable taking 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.
[15] 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.

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 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.

As clearly stated above, 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. We found the self-reported attendance data to be very accurate, and impacts estimated using data from the school survey are qualitatively very similar to those reported here.[16] Having shown strong attendance impacts from the treatment and relatively muted 'knowledge' impacts (in the form of English literacy), 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 successfully completes her current grade. In Table 4b, we see a strongly significant 16 percentage point increase in grade completion among *baseline dropouts*, with no impact among baseline schoolgirls. If we compare these completion impacts to the attendance impacts, however we see that the share of baseline dropouts returning to school who successfully pass (16.2/44.2=37%) is in fact smaller than to the share of baseline schoolgirls remaining in school who pass (2.9/4.6=63%). 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.

### *4.2. Impact of Transfer sizes & splits*

There is no evidence that the transfer size has a strong additional effect 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 on outcomes. 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 row 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

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

increased among *conditional* schoolgirls. For example, among conditional schoolgirls, each $1 transferred to the girl, seems to reduce her likelihood of dropout by 1.25 percentage points, implying a reduction in dropout of more than 50% if the girl is receiving the highest *individual* transfer amount of $5.

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. This finding has major cost-efficiency implications for the design of CCT programs, because it suggests that modest payments can be just 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 can be seen in Table 6, there are no significant one-year impacts of conditionality on schooling and literacy.

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

While the topic of the relative effectiveness of the conditionality needs to be probed further (not least because we did not experiment with this contract parameter among *baseline dropouts*), if the current findings were to hold up after two years of program implementation for school attendance (as well as independent assessment of learning), it would have major implications for cost effectiveness of cash transfer programs, not the least because the monitoring and enforcement necessitated by the conditionality represents a substantial share of the administrative costs of a CCT program.

*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 about each girl's social networks (five closest friends) in order to get at one of these channels. A second 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. We focus on this final group in this paper to examine possible spillovers of the program, mainly because the saturation of treatment in each EA was directly randomized, and so we have fully 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 very clean source of identification, we do not detect any spillover effects at the end of the first year of the program. 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 finds none. The columns titled "Saturation" exploit the research design by including both a dummy to pick up the mean within village vs. pure control comparison and a measure of the fraction of girls treated in treatment villages. Neither in these regressions nor in Figure 10, the latter of which plots the changes studied in Table 7, do we see any evidence of spillover effects.

## 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 randomized 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 the price (or substitution) effect is zero, even though we find large income effects. Nor can we reject the hypothesis that the marginal impact of an increase in the total transfer size 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 enrolment increase when a higher share of the transfer goes directly to the girl herself, especially when the transfers are *conditional* on attending school.

Should this lack of impact over contract parameters lead us to conclude that a low-transfer, unconditional CCT program might be most cost effective in improving schooling outcomes? One objection to this interpretation is that these are one-year impacts, and that longer program duration may alter these findings. Another valid objection would be that no *baseline dropouts* received the "unconditional" treatment, and as such we cannot reject the notion that the conditionality could have an effect on the schooling outcomes of this group – over and above the "income effect".

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 second column of Table 5a has a standard error of .0035, indicating that a marginal effect of .007 would be detectable with 95% confidence. This 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.[17] 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 offering $5 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 does not appear to be producing any significant benefits in terms of schooling outcomes. Our results, if sustained in the longer run, therefore suggest that a low-payment, unconditional cash transfer program that makes at least some of the transfers directly to the girls themselves would be the most cost-effective option in designing a transfer program to improve schooling outcomes among young women in sub-Saharan Africa.

[17] 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 easily 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 | 15.227 | 1.932 |
| Aggregate consumption p/c | 1322.679 | 999.799 | 1775.047 | 1195.982 |
| Aggregate food consumption p/c | 822.475 | 507.593 | 970.581 | 541.082 |
| Household Asset Index | -0.728 | 2.377 | 0.821 | 2.619 |
| # shocks of any type over previous year | 3.882 | 2.286 | 3.759 | 2.152 |
| Highest Grade attended at baseline | 6.104 | 2.833 | 7.477 | 1.601 |
| Highest Qualification achieved at baseline* | 1.385 | 0.656 | 1.416 | 0.626 |
| Household Size | 6.098 | 2.550 | 6.396 | 2.185 |
| Mother's Education* | 2.095 | 0.859 | 2.282 | 0.864 |
| Father's Education* | 2.686 | 0.980 | 2.873 | 0.938 |
| 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 | 35.305 | 9.868 | 32.708 | 9.161 |

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

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

| | Stratum: Dropouts: T1 | Dropouts: C1 | Schoolgirls: T2a | Schoolgirls: T2b | Schoolgirls: S2 | Schoolgirls: C2 | Overall: Total Treatments: | Overall: 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 Followup | 397 | 408 | 484 | 267 | 588 | 1409 | 1148 | 3553 |
| Used for Panel Analysis | 396 | 408 | 480 | 265 | 588 | 1409 | 1141 | 3546 |

**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 |

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

**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? | 17.430 | -0.153 | 0.034 | -0.019 | 0.020 | 0.075 | -0.040 | 2958 |
| | (81.999) | (0.102) | (0.051) | (0.038) | (0.018) | (0.107) | (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? | 23.374 | -0.079 | 0.052 | -0.019 | 0.025 | 0.095 | -0.057 | 2154 |
| | (78.812) | (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 | 2154 |
| | (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 | 2154 |
| | (16.944) | (0.024) | (0.017) | (0.009) | (0.005) | (0.035) | (0.007) | |
| Spillover/Control Balanced? | 86.471 | 0.016 | -0.022 | 0.003 | 0.047 | -0.043 | -0.048 | 1997 |
| | (111.849) | (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 | 1997 |
| | (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 followup 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**

| 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 | 0.028 | 0.072 | 0.019 | 0.030 | -0.001 |
| | (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 | -0.108 | -0.108 | -0.108 | 0.046 | 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 | 5916 | 1608 | 4308 | 3778 | 3348 | 5911 | 1607 | 4304 | 3774 | 3344 |
| # unique individuals | 2958 | 804 | 2154 | 1889 | 1674 | 2958 | 804 | 2154 | 1889 | 1674 |
| 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: | 0.774 | 0 | 1 | 1 | 1 | 0.753 | 0.461 | 0.839 | 0.839 | 0.839 |

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: Grade Progression Impacts by Stratum**

| **Dependent Variable:** | **Passed Grade** | | | | |
|---|---|---|---|---|---|
| | All | Baseline Dropouts | All Baseline Schoolgirls | Conditional Schoolgirls | Uncon-ditional Schoolgirls |
| Post-Treatment Dummy | 0.053 | 0.162 | 0.029 | 0.044 | -0.002 |
| | (0.030)* | (0.028)*** | (0.037) | (0.045) | (0.043) |
| In School at Baseline | 0.428 | | | | |
| | (0.024)*** | | | | |
| Observations | 2875 | 787 | 2088 | 1833 | 1619 |

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 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 | | 0.012 | | 0.000 | |
| | (0.005) | | (0.015) | | (0.005) | | (0.006)* | | (0.011) | |
| Share of Transfer to Girl | | 0.069 | | -0.001 | | 0.085 | | 0.136 | | -0.015 |
| | | (0.062) | | (0.178) | | (0.063) | | (0.069)* | | (0.122) |
| 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 | 0.100 | 0.579 | 0.404 | 0.576 | 0.032 | -0.097 | 0.019 | -0.122 | 0.053 | -0.053 |
| (impact when transfer size = lowest value) | (0.022)*** | (0.039)*** | (0.052)*** | (0.071)*** | (0.02) | (0.030)*** | (0.03) | (0.038)*** | (0.030)* | (0.05) |
| In School at Baseline | -0.474 | -0.677 | | | | | | | | |
| | (0.027)*** | (0.029)*** | | | | | | | | |
| Round 2 Dummy | 0.333 | | 0.172 | | -0.109 | | -0.109 | | -0.109 | |
| | (0.024)*** | | (0.012)*** | | (0.013)*** | | (0.013)*** | | (0.013)*** | |
| Observations | 5916 | 2282 | 1608 | 792 | 4308 | 1490 | 3778 | 960 | 3348 | 530 |
| # unique individuals | 2958 | 1141 | 804 | 396 | 2154 | 745 | 1889 | 480 | 1674 | 265 |
| R-squared | 0.26 | 0.44 | 0.52 | 0.61 | 0.09 | 0.06 | 0.1 | 0.08 | 0.1 | 0.05 |
| Baseline Mean of Outcome in Control: | 0.774 | 0.774 | 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**

| **Dependent Variable: Literate in English** | All | | Baseline Dropouts | | All Baseline Schoolgirls | | Conditional Schoolgirls | | Unconditional Schoolgirls | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Amounts | Share | Amounts | Share | Amounts | Share | Amounts | Share | Amounts | Share |
| Household Transfer Amount | 0.006 | | 0.004 | | 0.006 | | 0.001 | | 0.017 | |
| | (0.007) | | (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)* | | (0.019)* | | (0.018) | |
| Share of Transfer to Girl | | 0.178 | | 0.115 | | 0.192 | | 0.286 | | 0.004 |
| | | (0.130) | | (0.138) | | (0.158) | | (0.206) | | (0.179) |
| 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 | -0.038 | -0.012 | 0.025 | 0.023 | -0.052 | -0.013 | -0.038 | -0.020 | -0.081 | 0.003 |
| (impact when transfer size = 0) | (0.032) | (0.053) | (0.052) | (0.069) | (0.035) | (0.051) | (0.042) | (0.066) | (0.047)* | (0.078) |
| In School at Baseline | 0.035 | 0.007 | | | | | | | | |
| | (0.020)* | (0.027) | | | | | | | | |
| Round 2 Dummy | 0.047 | | 0.025 | | 0.086 | | 0.086 | | 0.086 | |
| | (0.017)*** | | (0.019) | | (0.018)*** | | (0.018)*** | | (0.018)*** | |
| Observations | 5911 | 2281 | 1607 | 791 | 4304 | 1490 | 3774 | 960 | 3344 | 530 |
| # unique individuals | 2958 | 1141 | 804 | 396 | 2154 | 745 | 1889 | 480 | 1674 | 265 |
| R-squared | 0.06 | 0.08 | 0.03 | 0.06 | 0.06 | 0.08 | 0.06 | 0.09 | 0.05 | 0.08 |
| Baseline Mean of Outcome in Control: | 0.774 | 0.774 | 0.461 | 0.461 | 0.839 | 0.839 | 0.839 | 0.839 | 0.839 | 0.839 |

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 | 0.032 | 0.077 | -0.004 | 0.064 |
| | (0.020) | (0.026) | (0.035) | (0.036) | (0.033) | (0.046)* | (0.055) | (0.055) |
| Conditionality * Transfer Amount | | -0.0052 | 0.000 | 0.000 | | -0.016 | -0.007 | -0.007 |
| | | (0.007) | (0.006) | (0.006) | | (0.012) | (0.010) | (0.010) |
| Transfer Amount | | 0.003 | 0.000 | 0.002 | | 0.017 | 0.015 | 0.017 |
| | | (0.005) | (0.011) | (0.005) | | (0.007)** | (0.018) | (0.007)** |
| Post-Treatment Dummy (T2a and T2b) | 0.061 | 0.052 | 0.061 | 0.051 | -0.001 | -0.050 | -0.031 | -0.082 |
| (Measures impact of T2b with transfer at lowest) | (0.019)*** | (0.021)** | (0.029)** | (0.027)* | (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 | 4308 | 4308 | 4308 | 4308 | 4304 | 4304 | 4304 | 4304 |
| # unique individuals | 2154 | 2154 | 2154 | 2154 | 2154 | 2154 | 2154 | 2154 |
| R-squared | 0.09 | 0.09 | 0.09 | 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**

| | Schooling | | English Literacy | |
|---|---|---|---|---|
| **Dependent Variable: In School in Followup** | DID | Saturation | DID | Saturation |
| Post-Treatment Dummy for Within-Village Controls: | 0.010 | 0.001 | 0.015 | 0.018 |
| | (0.020) | (0.027) | (0.028) | (0.033) |
| % Saturation Treatment in Village | | 0.000 | | 0.000 |
| | | (0.001) | | (0.001) |
| Round 2 dummy | -0.108 | -0.108 | 0.086 | 0.086 |
| | (0.013)** | (0.013)** | (0.018)** | (0.018)** |
| Observations | 3994 | 3994 | 3990 | 3990 |
| # unique individuals | 1997 | 1997 | 1997 | 1997 |
| R-squared | 0.1 | 0.1 | 0.05 | 0.05 |

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.

**FIGURES**
**Figure 1**

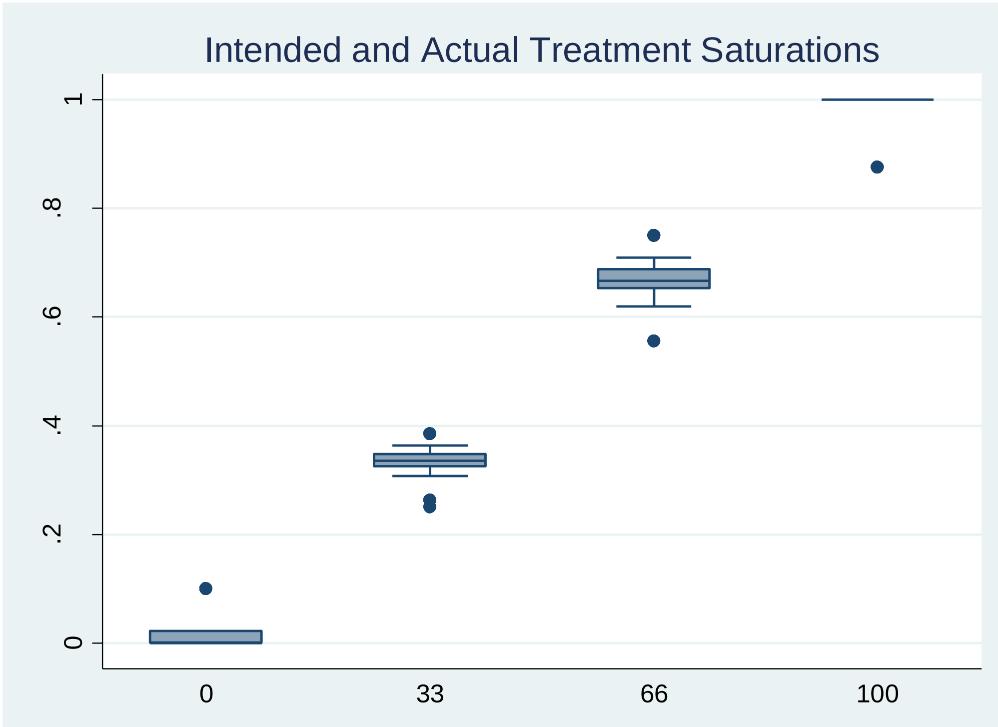


**Figure 2**

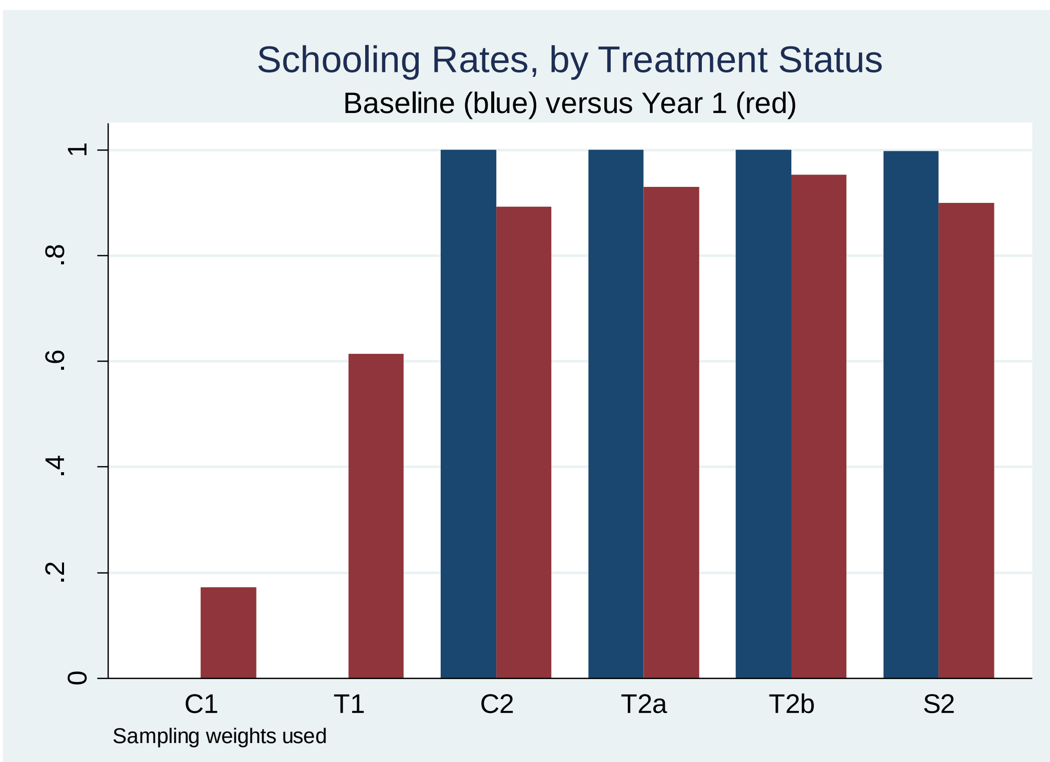

**Figure 3a**

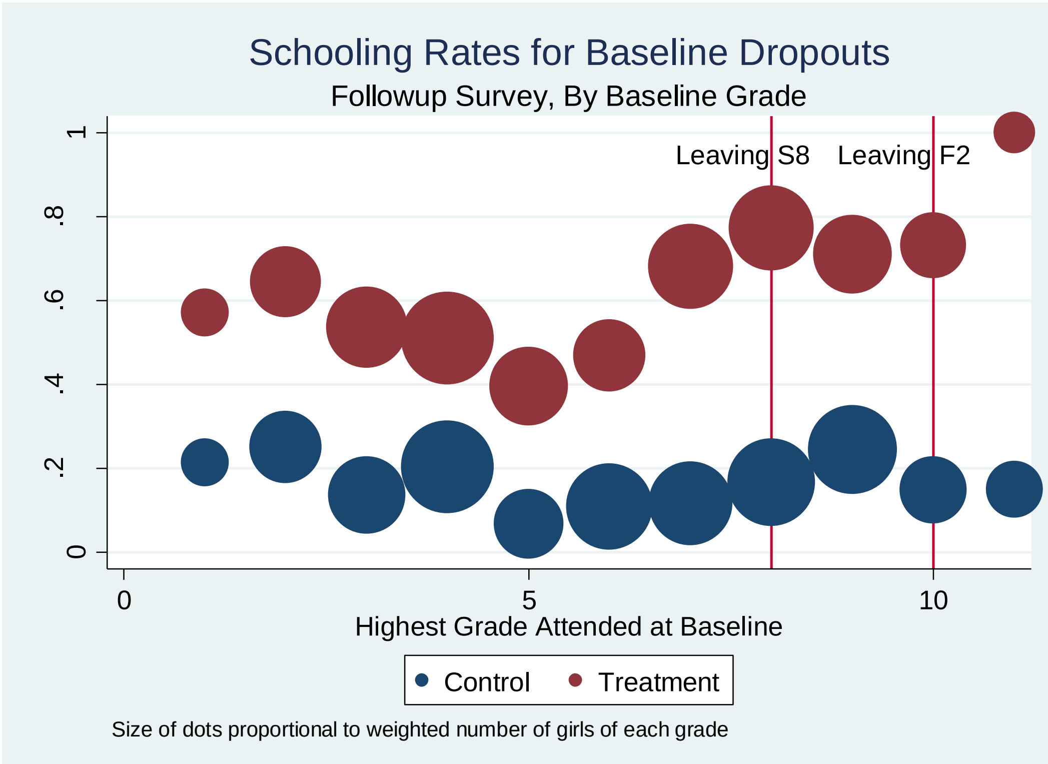


**Figure 3b**

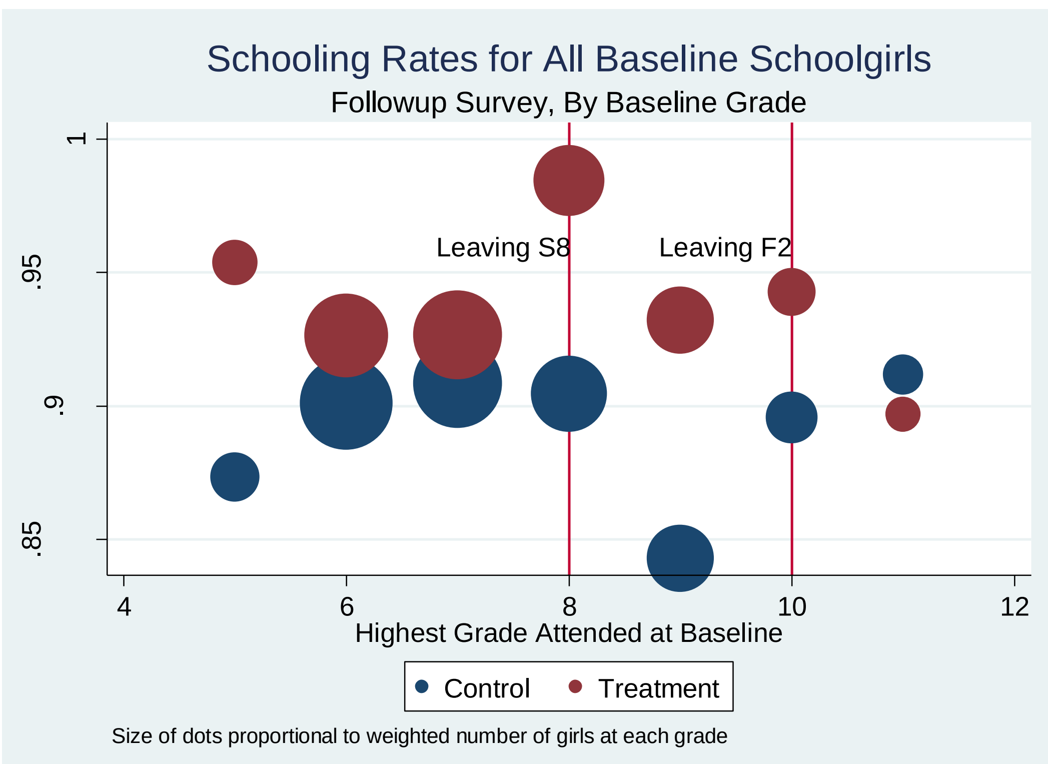

**Figure 4a**

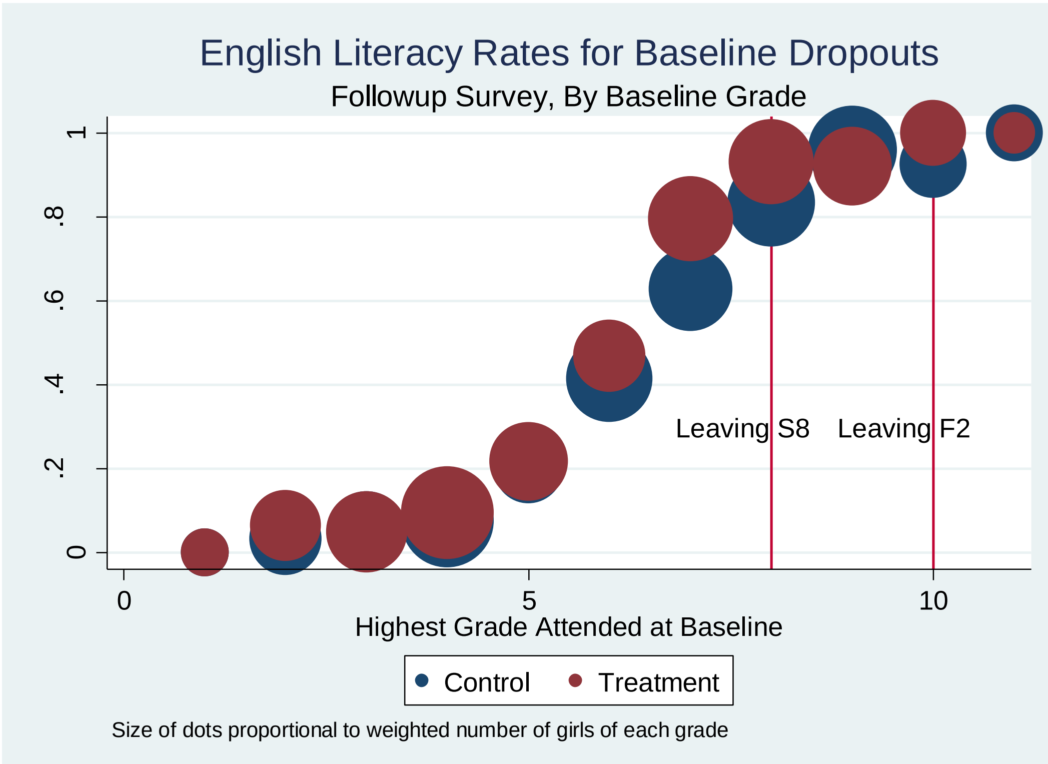


**Figure 4b**

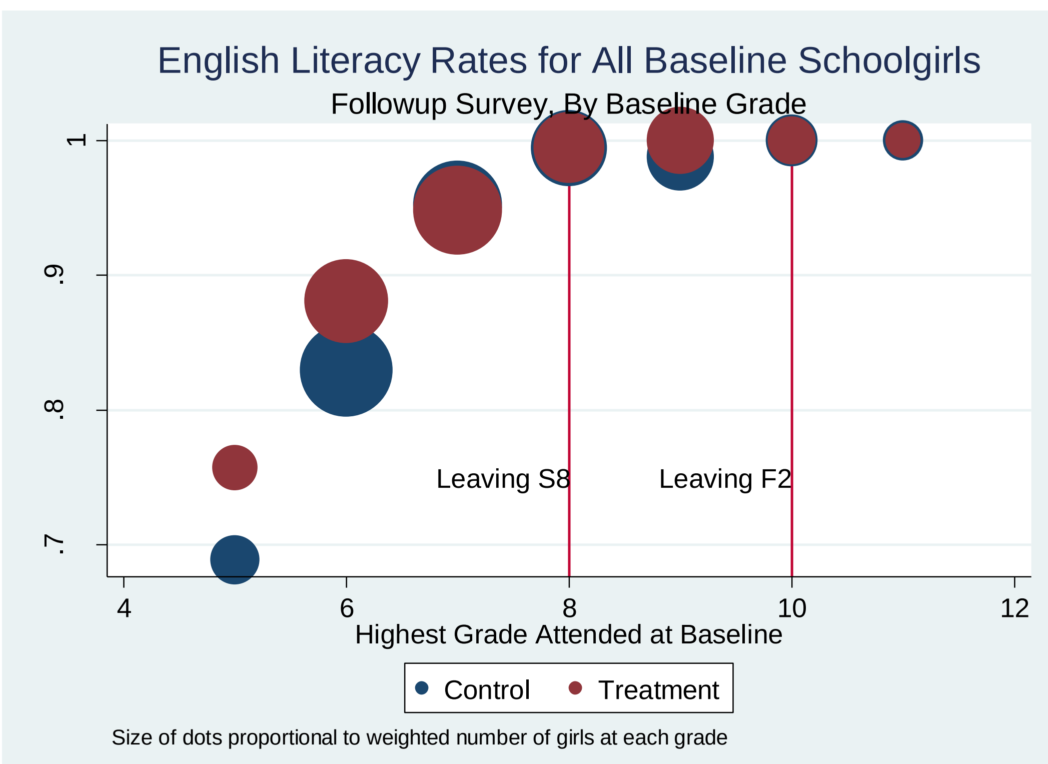

**Figure 5**

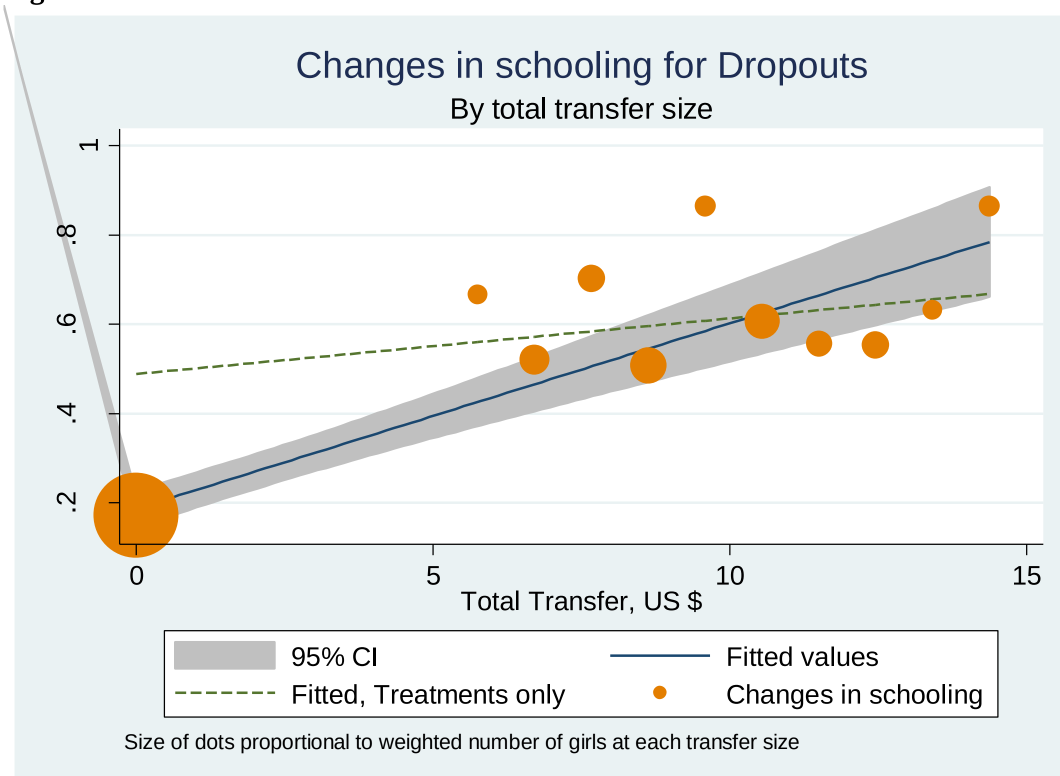


**Figure 6**

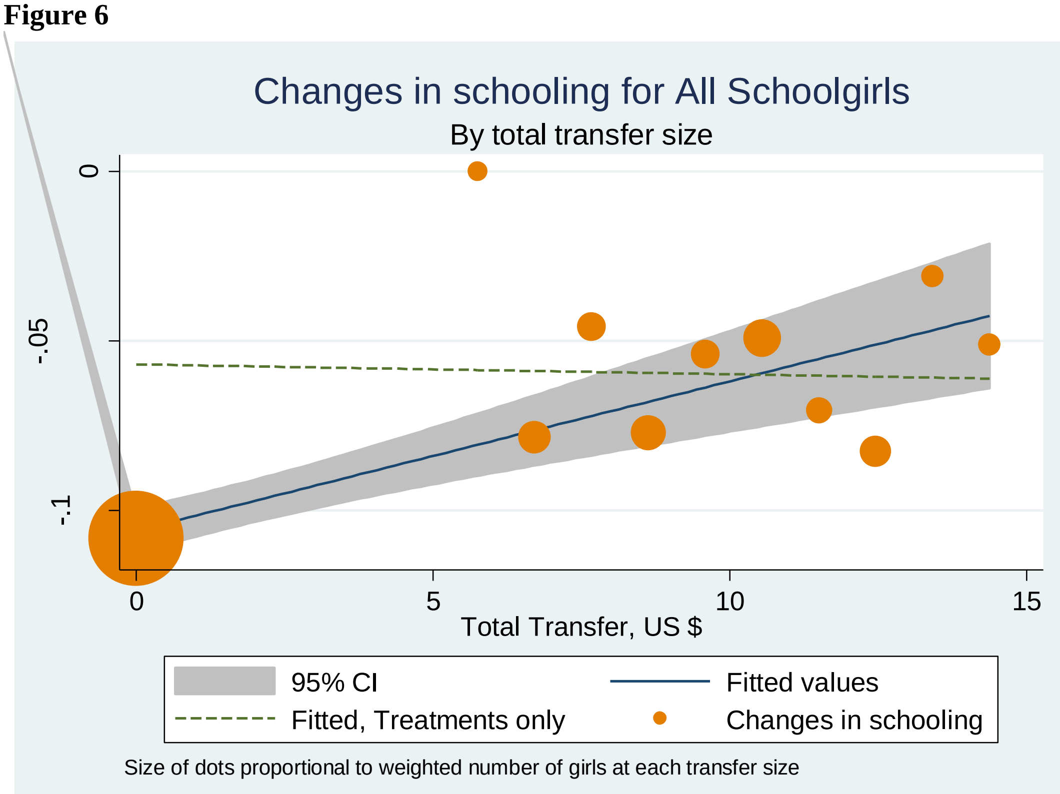

**Figure 7**

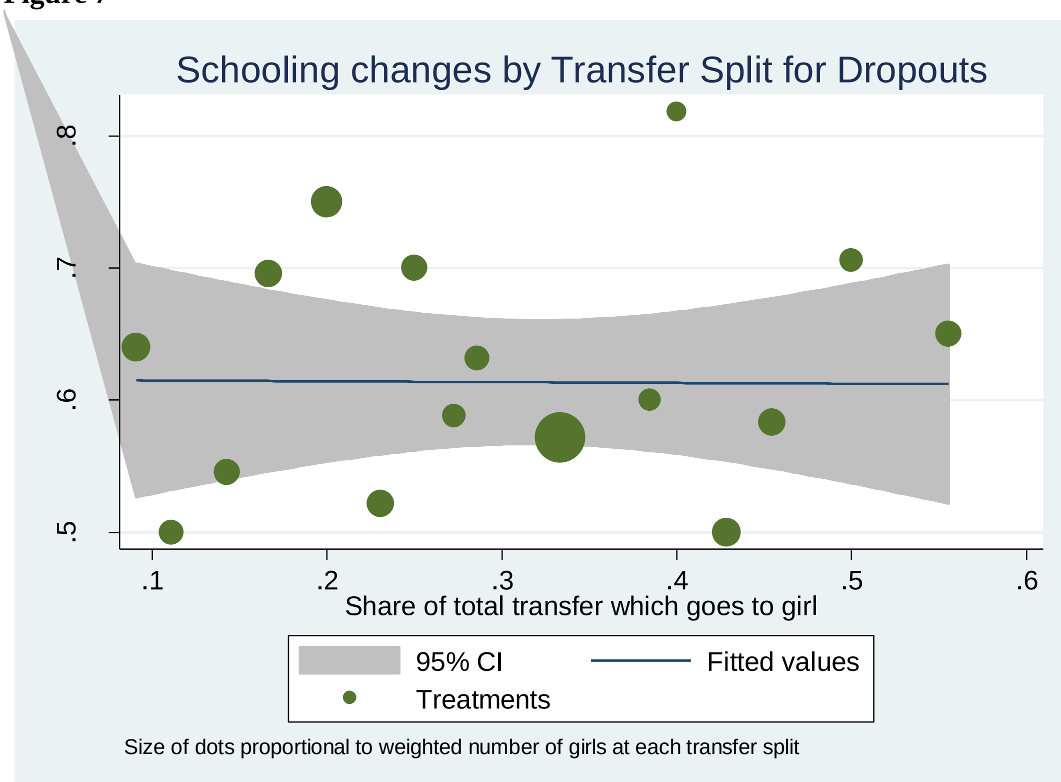


**Figure 8**

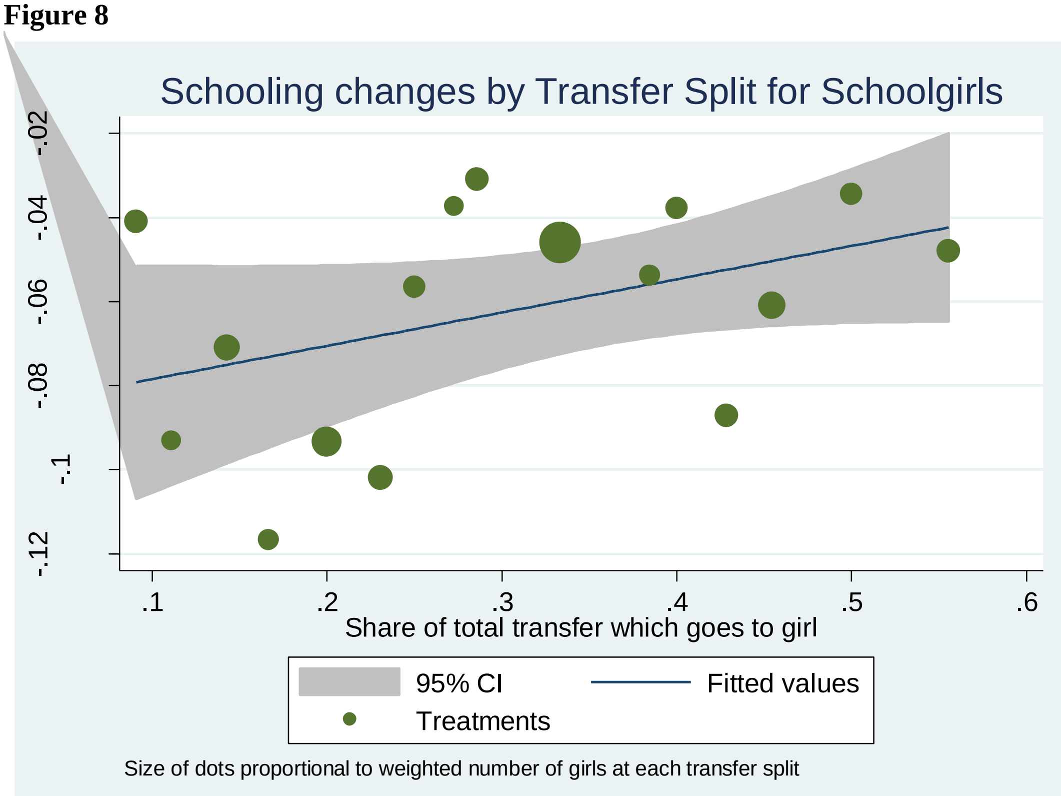

**Figure 9**

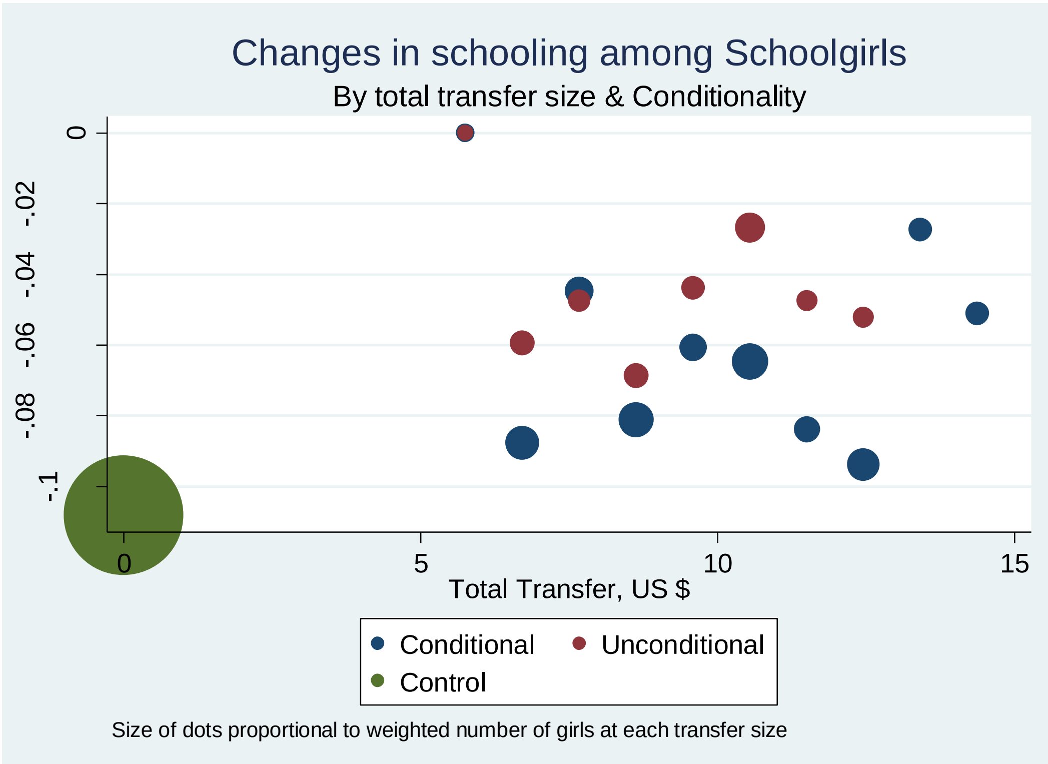


**Figure 10**