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

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Abstract

As of 2007, 29 developing countries had some type of conditional cash transfer 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 conditional cash transfer program is severely limited in a number of critical dimensions. This paper presents one-year schooling impacts from a conditional cash transfer 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. The authors 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 percent. These impacts were, *on average*, similar in the conditional and the unconditional treatment arms. Although 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. There were no spillover effects within treatment communities after the first year of program implementation. Policymakers looking to design cost-effective cash transfer programs targeted toward young women should note the relative insensitivity of these short-term program impacts with respect to conditionality and total transfer size.

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

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

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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 *enrollment* 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, *baseline schoolgirls* in each treatment EA was randomly assigned to receive either

[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).

conditional or *unconditional* 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 enrollment, 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 enrollment points us in favor of the conditions. Based on the fact that some households in Mexico and Ecuador did not think that the cash transfer program in their respective country was conditional on school attendance, de Brauw and Hoddinott (2007) and Schady and Araujo (2008) both find that school enrollment 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 enrollment; 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 enrollment, 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 enrollment 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 enrollment 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 Southern Malawi, which includes Zomba, is poorer, has lower levels of education, and higher rates of HIV than Central and Northern Malawi, these differences are relative considering that Malawi is one of the poorest countries in the world with one of the highest rates of HIV prevalence.

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) were 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

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

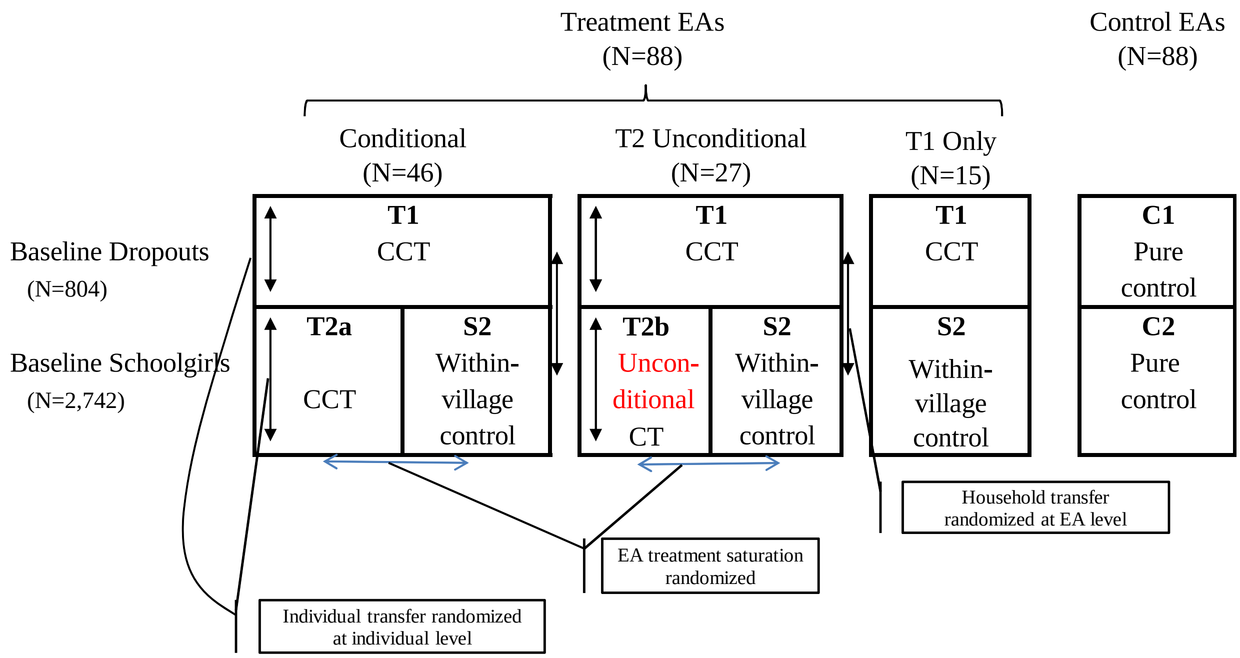
[8] We chose to target these two groups separately to ensure that we had a significant number of dropouts in our sample. Treating all dropouts allows us to focus on a subpopulation whose schooling rates are extremely sensitive to transfers.

[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 were only made to those people and one designated proxy. Recipients and parents were asked to bring such proxies to the first cash payment point for them to be identified and photographed. For the rest of the program, no one other than the recipient, the parent, and the designated proxy was allowed to pick up any payments.

Recipients were informed of the location and the timing of the first monthly transfer payment during the offer stage, and about the next transfer date when they picked up each transfer. The cash payment points were chosen to take place at centrally located and well-known places, such as churches, schools, etc. For each EA, they were selected so that no recipient has to travel for more than 5 kilometers to the cash payment point. Security guards were at hand to make sure that the money was secure and each recipient was given a sealed envelope with her name on it.[14] After counting the amount and making sure it was correct, each recipient signed a piece of paper to acknowledge the receipt of the money. In between

[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.

payment dates, the implementing agency collected attendance records for all the conditional students in the program to make sure that they were complying with the program requirements and attending school.

The cash transfers took place monthly and at each meeting some basic information was collected for each sample respondent, such as who was picking up the money (girl, guardian, or proxy), how far they had to travel, etc. As part of the transfer program, monthly school attendance of all the conditional cash transfer recipients was checked and payment for the following month was 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 was ever kicked out of the program, i.e. cash transfer payments were 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 enrollment 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 4a.

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 enrollment 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 enrollment 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 enrollment 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 enrollment 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 enrollment 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 enrollment 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 Table 4b confirm those presented above using self-reported attendance data. We also 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, however, we see that the share of

[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.

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 in 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 4a – 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 enrollment 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 enrollment 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

[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.

enrollment are higher when the transfer size given directly to the girl herself is increased, but only statistically 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 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 detect statistically significant impacts of the conditionality, we would 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 will 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

[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.

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 .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 on self-reported school enrollment is, in 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 total transfer to the household 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 Followup	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**					**Passed Grade**				
	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	0.103	0.306	0.059	0.066	0.047	0.049	0.162	0.025	0.039	-0.005
	(0.019)***	(0.038)***	(0.022)***	(0.027)**	(0.028)*	(0.030)	(0.028)***	(0.037)	(0.044)	(0.043)
In School at Baseline	0.556					0.428				
	(0.026)***					(0.024)***				
Mean of Outcome in Control:	0.674	0.114	0.791	0.791	0.791	0.497	0.087	0.582	0.582	0.582
Observations	2874	787	2087	1832	1618	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 the student to have 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%, robust 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	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	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

	Schooling		English Literacy	
Dependent Variable: In School	DID	Saturation	DID	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)

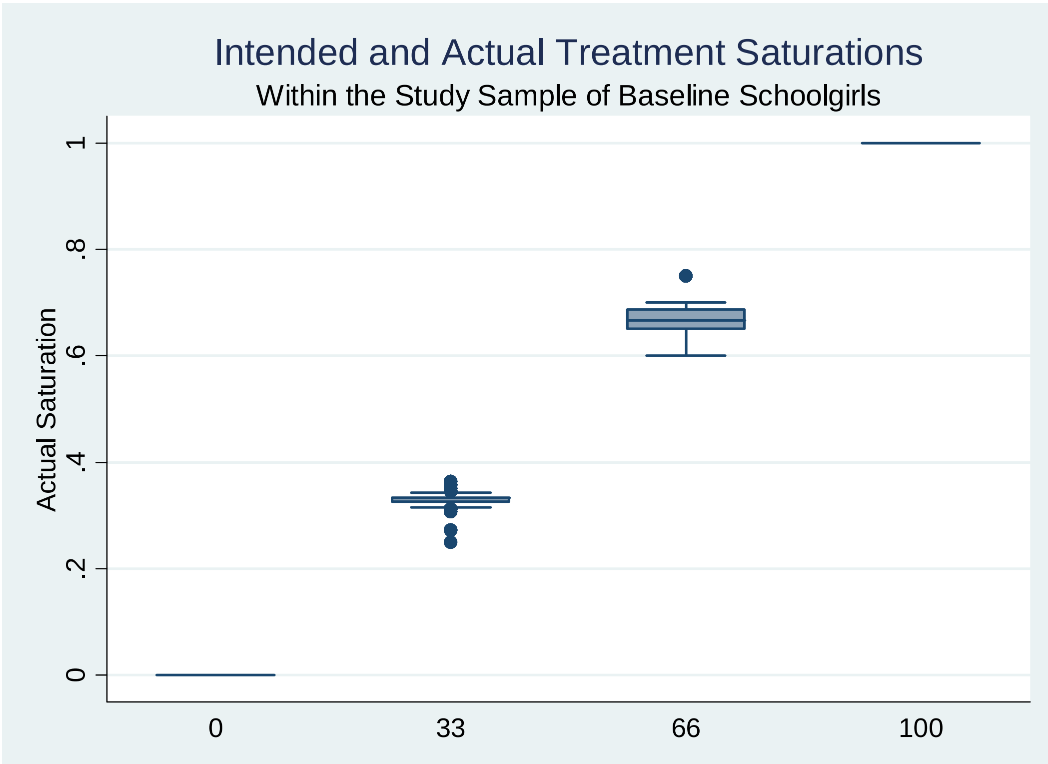


Figure 2

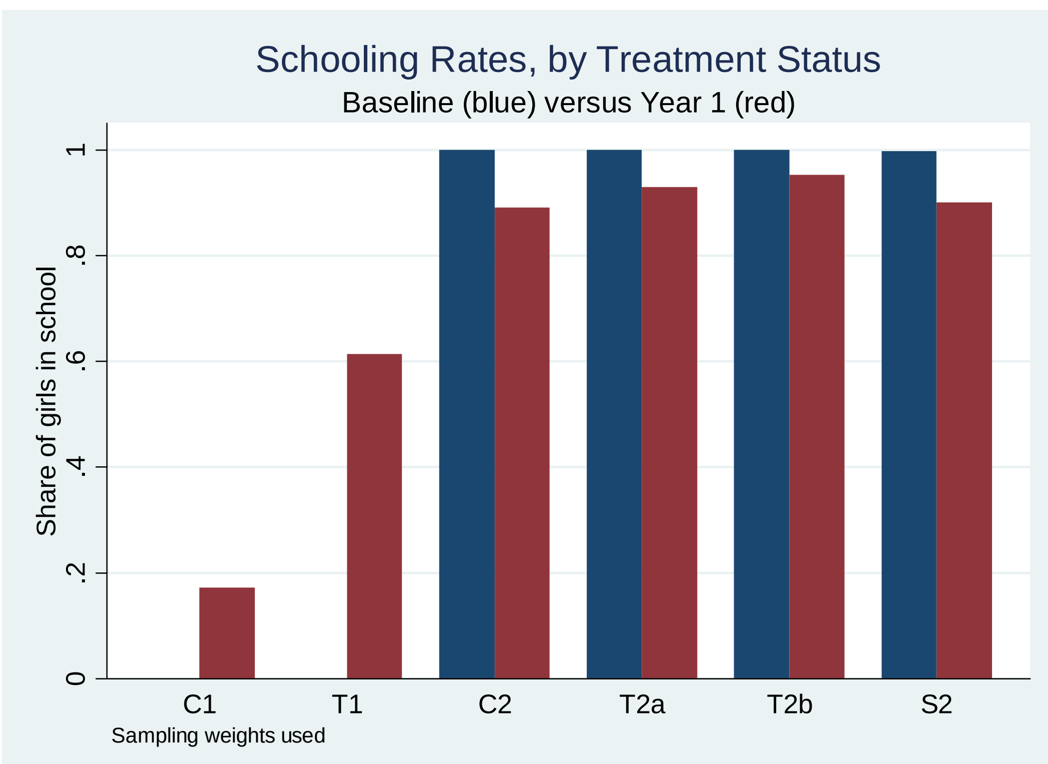


Figure 3a

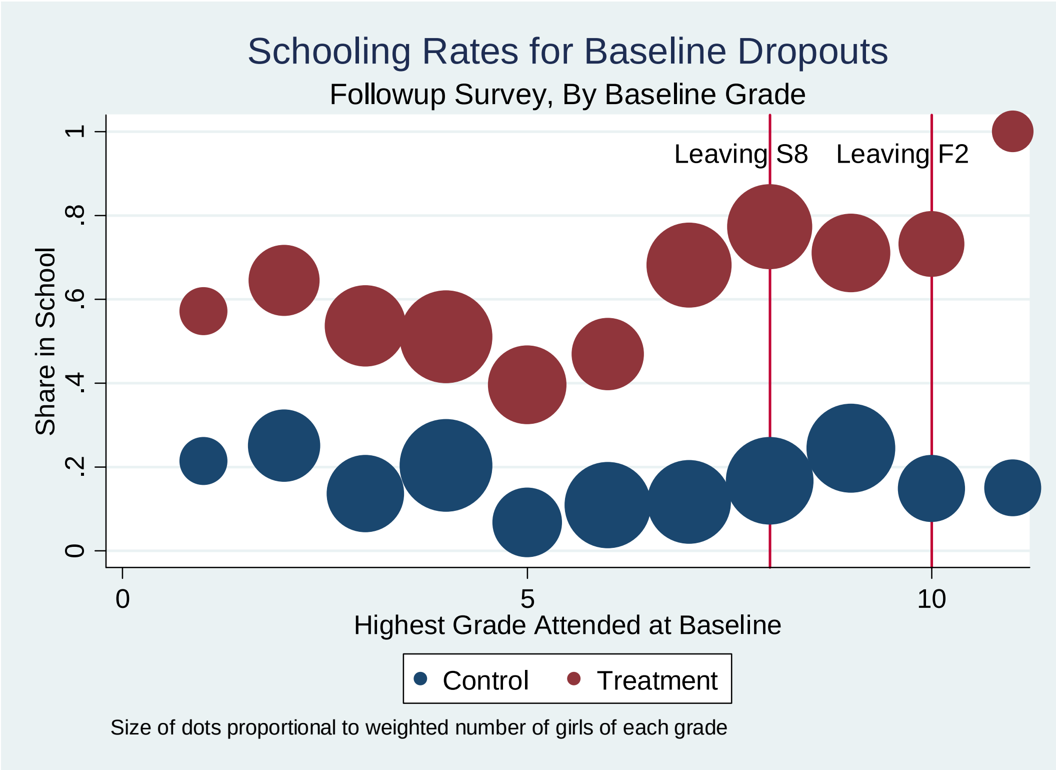


Figure 3b

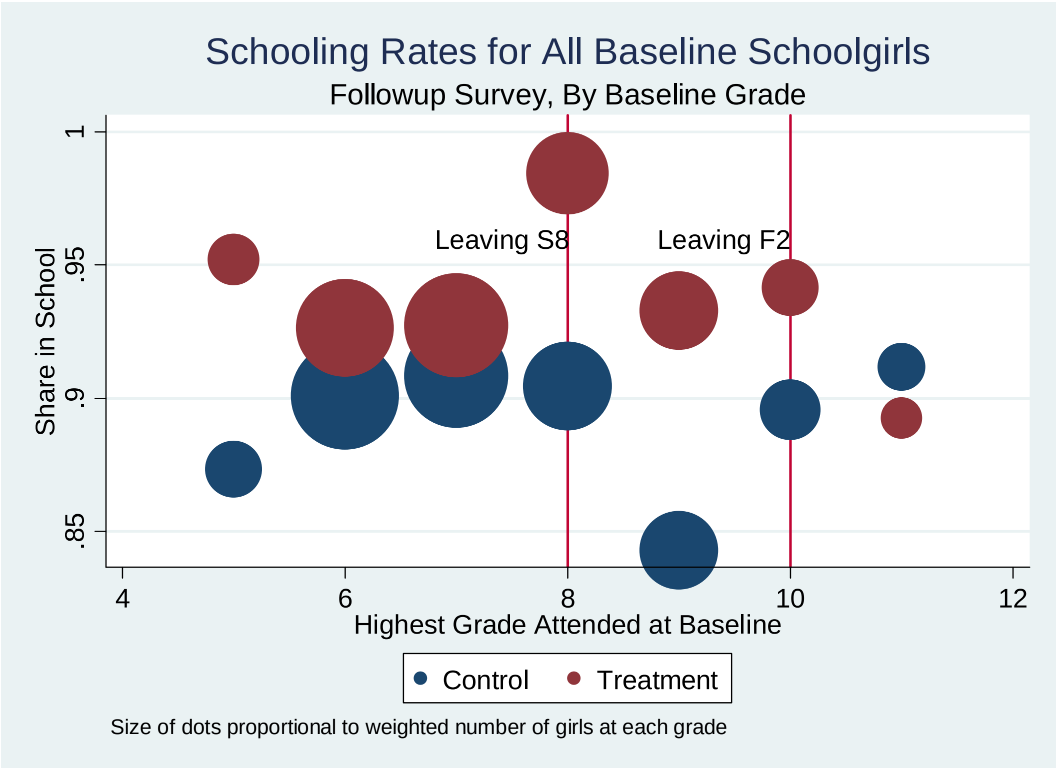
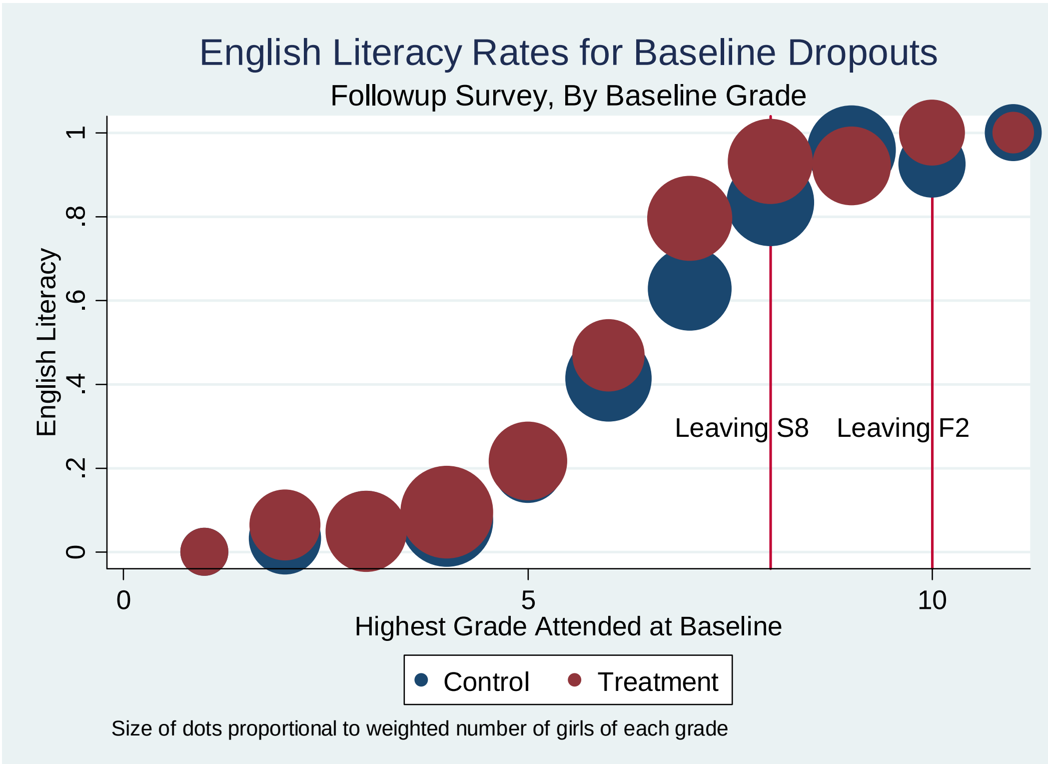


Figure 4a



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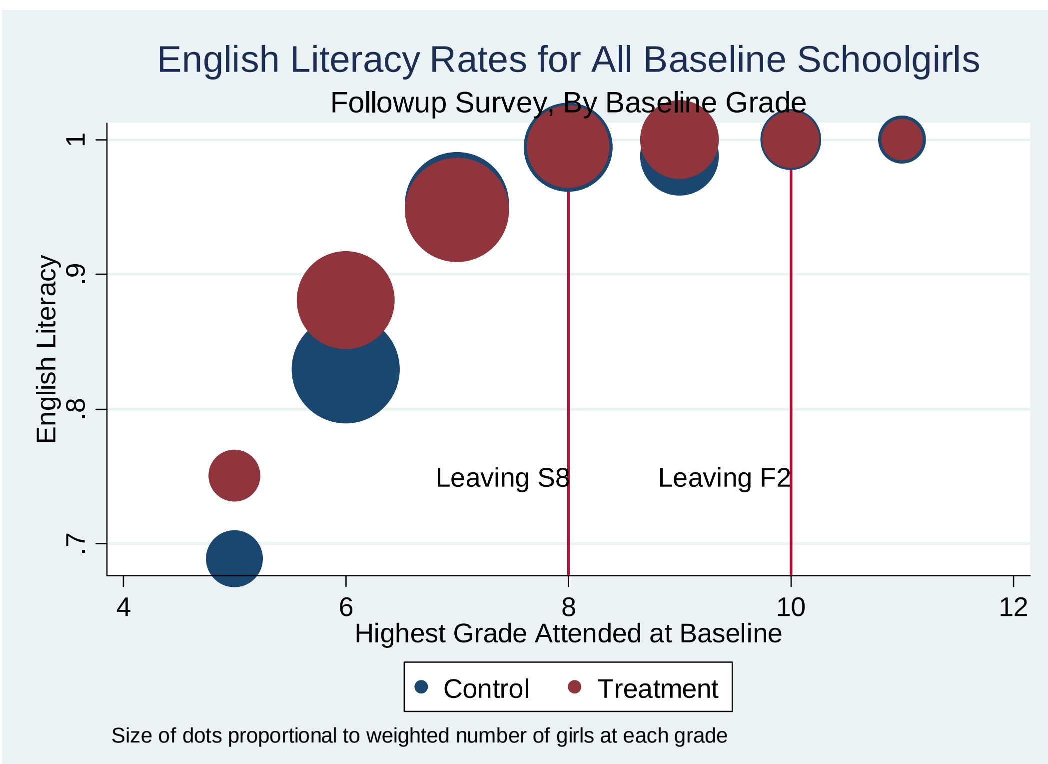

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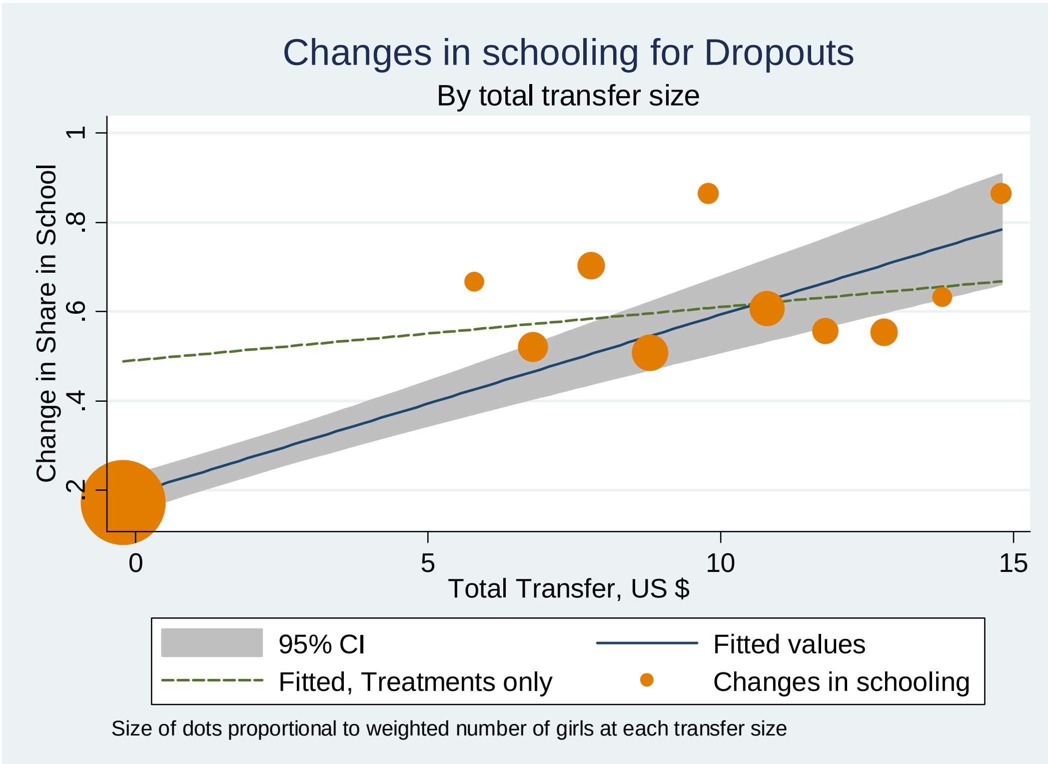

Figure 6

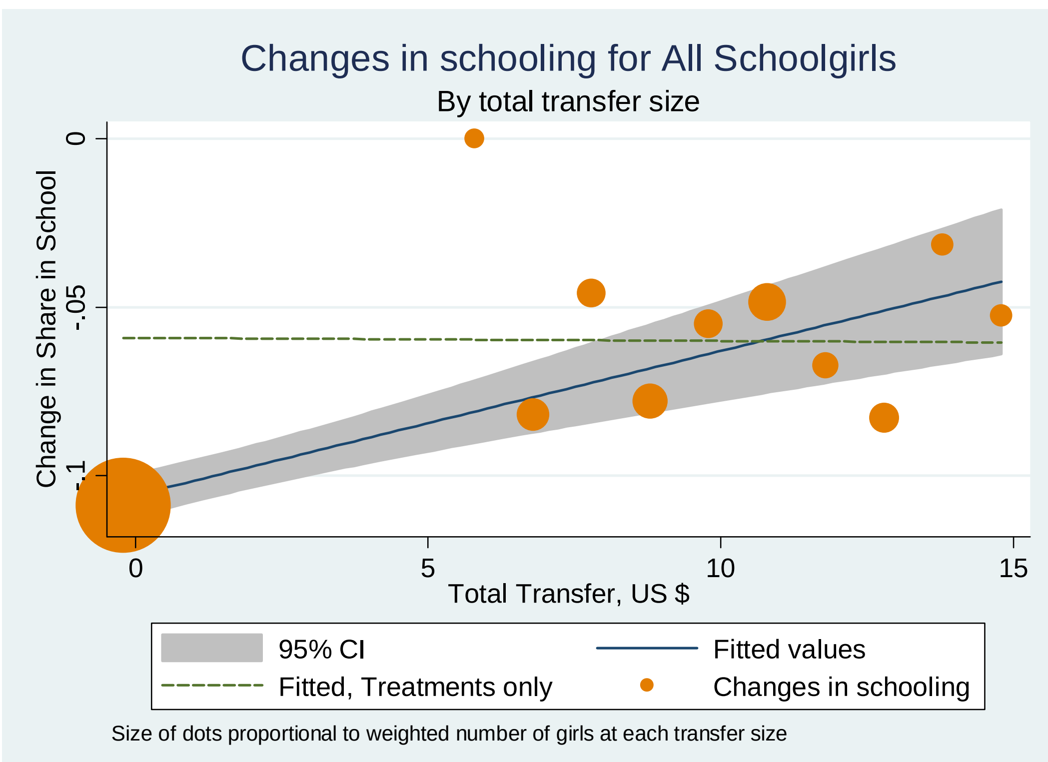

Figure 7

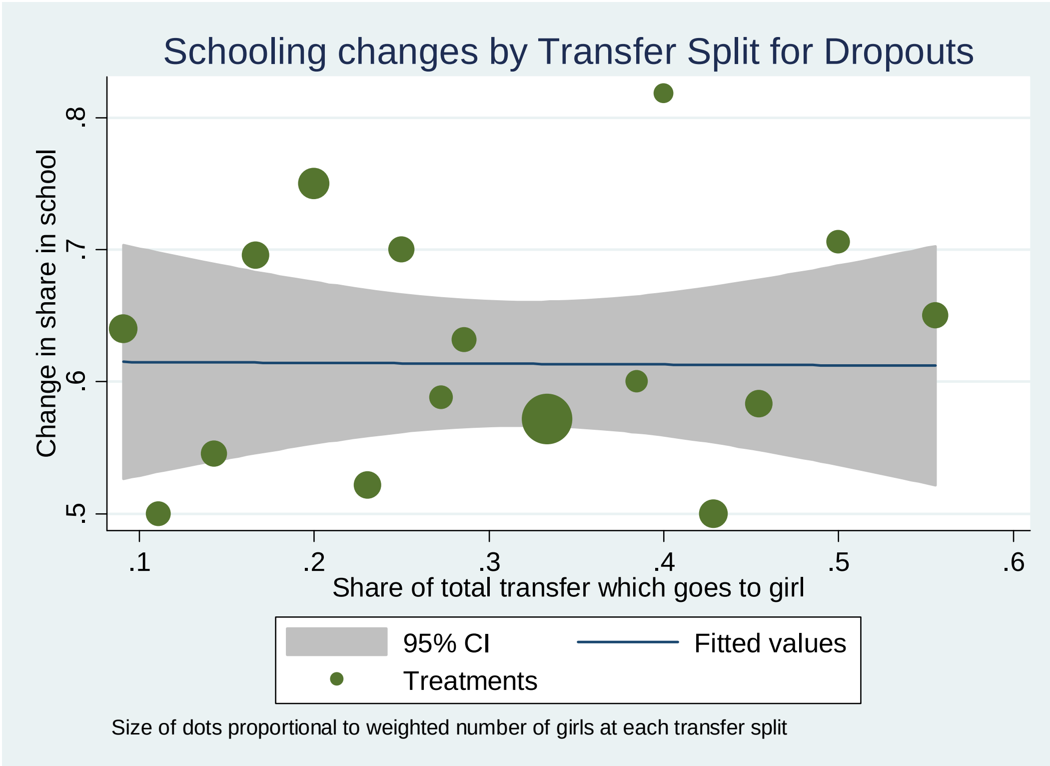

Figure 8

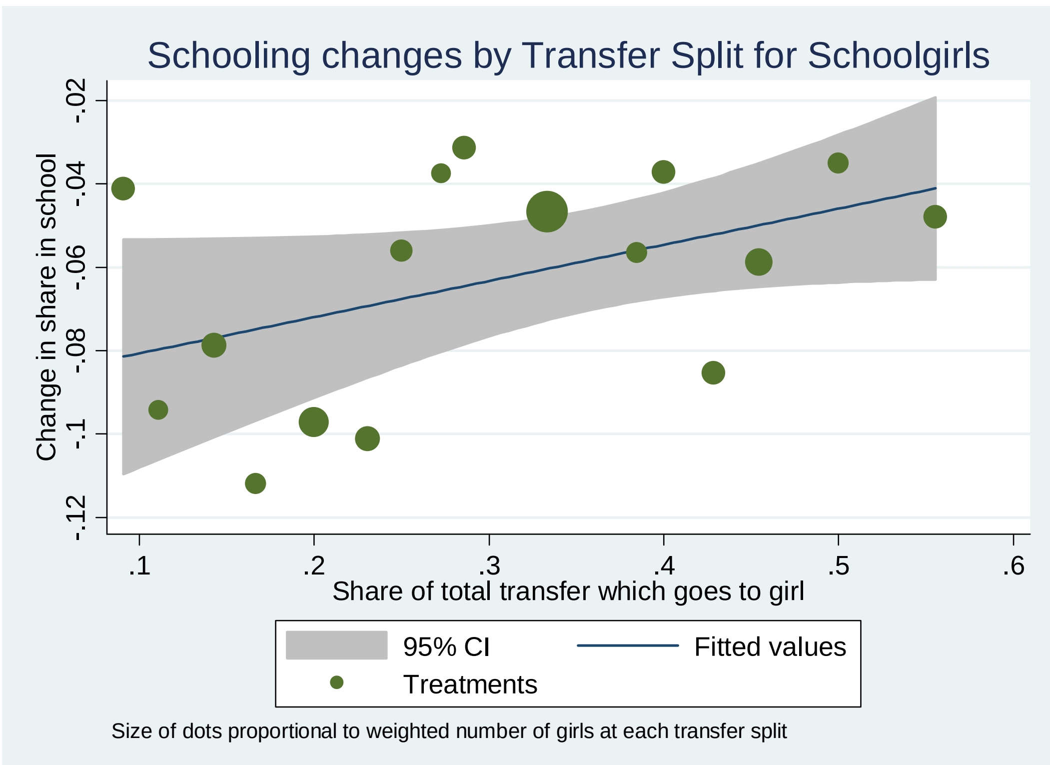

Figure 9

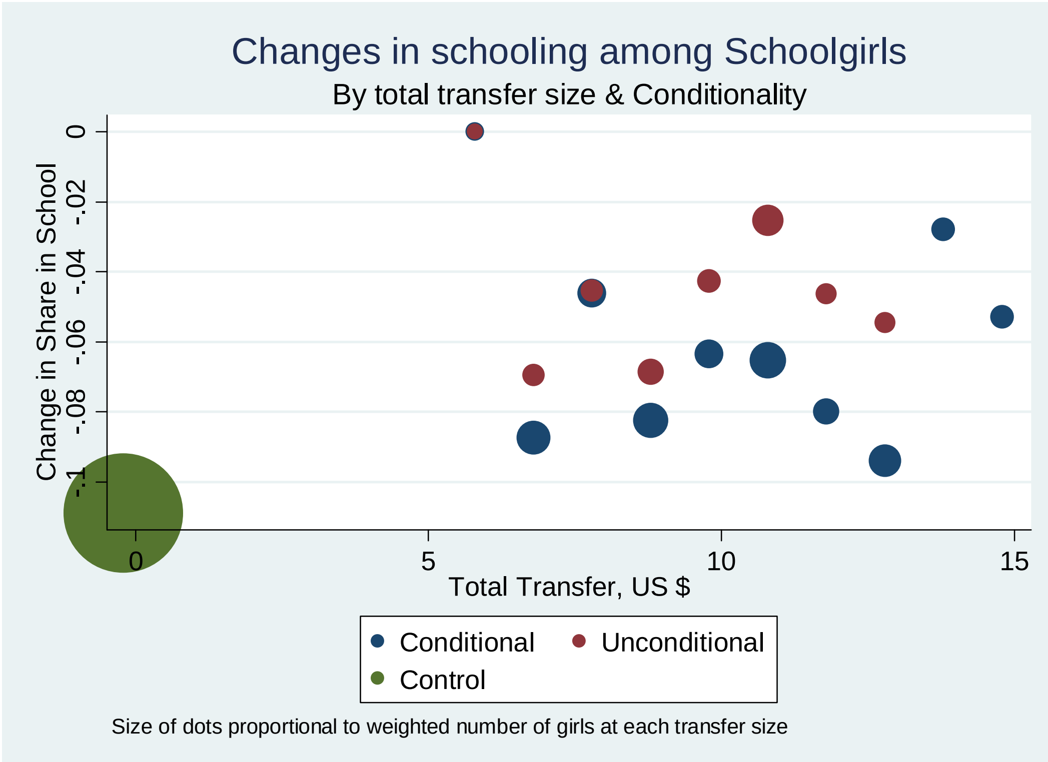

Figure 10