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Schooling, Income, and Sexual Behavior: The Design, Implementation, and Short-Term Impacts of a CCT Program for Schooling in Malawi

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1. Introduction

Conditional Cash Transfers (CCTs) can be an important component of social protection policy, and finds that "…there is considerable evidence that CCTs have improved the lives of poor people."[2] Early CCT programs have been popular and became national programs a few years later. As of 2007, twenty-four developing countries had some type of a CCT program in place, with many others planning or piloting one. It seems that CCT programs are here to stay – at least for the foreseeable future.

However, evidence on the impact of CCT programs on final outcomes is limited, and, when available, mixed at best. While there have been several evaluations of the impact CCT programs have on school attainment and learning, early childhood development, and adult health, no one has studied the possible effect of these programs on the sexual behavior of the beneficiaries and their subsequent HIV risk. Given the high prevalence of HIV infection among young people in sub-Saharan Africa (SSA) and the burden AIDS poses on these economies, this is potentially a very important impact to document.

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[2] "Conditional Cash Transfers for Attacking Present and Future Poverty", World Bank Policy Research Report, 2009.

While education has been suggested as a "social vaccine" to prevent the spread of HIV (Jukes, Simmons, and Bundy, 2008), almost all of the evidence we have on the link between school attendance (or attainment) and the risk of HIV infection comes from cross-sectional studies. Furthermore, the role of income (especially that of women's poverty) has been hypothesized as a significant factor in the spread of HIV in SSA, but again there is no credible evidence showing a causal link between income and HIV risk. A randomized intervention, such as the one that is the subject of this paper, which provides randomly varied amounts of cash transfers to young individuals and their guardians, is as close to an ideal setting as possible to examine the possible existence of such causal relationships. Given the high prevalence of HIV infection among young women in SSA, the policy importance of identifying any potentially large impacts of CCT for schooling interventions on HIV prevention cannot be overstated.

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

2. Can CCT Programs for Schooling protect young people from HIV?

To our knowledge, no CCT program for schooling has been evaluated to assess its possible impact on the sexual behavior of the young people benefitting from the program. CCT programs are likely to become more common in sub-Saharan Africa, where the risk of HIV infection is disproportionately high among young women and school-aged girls. Hence, impact evaluations that document the impact of such programs on the risk of HIV infection among young people can greatly help in improving the design of upcoming CCT programs in SSA.

The forthcoming PRR argues that among the areas that should receive high priority in impact evaluations (and, more generally, research) on CCTs is the role they play in reducing the transmission of HIV. Both schooling and poverty reduction (especially for women) are seen by many as key components in a comprehensive strategy to combat HIV/AIDS. However, causal evidence that links increased schooling or income to reduced risk of contracting HIV is very limited. Most of what we know about the relationship between schooling (attendance or attainment) and HIV risk comes from cross-sectional studies. The same is true of the relationship between poverty and HIV/AIDS.

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

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

improving women's economic circumstances are unlikely to decrease women's vulnerability to HIV infection.

However, many of the same sources asserting the plausibility of the relationship between poverty and HIV are puzzled to report evidence to the contrary. For example, Shelton, Cassell, and Adetunji (2005) report a positive correlation between household possessions and HIV prevalence in Tanzania. Examining the determinants of HIV in five countries with DHS data in sub-Saharan Africa, De Walque (2006) finds that wealth (measured by an asset index) is positively correlated with HIV status in three of the five countries, especially for females.[3] Finally, using prime-age adult mortality as a proxy measure for HIV/AIDS affected households; several studies find that higher income households are more likely to suffer an adult death (Yamano and Jayne, 2004; World Bank, 1999, Chapter 4; World Bank, 2006).

3. Survey Setting

Malawi, the setting for this research project, is an impoverished small country in southern Africa. Its population of almost 14 million in 2007 is overwhelmingly rural, 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).[4] Malawi also has the eighth-highest HIV prevalence in the world with 14 percent of the adult population infected (UNAIDS, 2006).[5] The gender gap in HIV prevalence among young adults, aged 15-24, is startling: prevalence was more than *four* times higher for females than males in 2004.

[3] De Walque and Corno (2007) report a similar positive conditional correlation in Lesotho.
[4] 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.
[5] The UNAIDS HIV estimate of 14.1 percent is close to the Demographic and Health Survey (2004) estimate of 12.7 percent (National Statistical Office and ORC Macro, 2005).

The CCT intervention that is the subject of this paper takes place in one district of Malawi, which both reduces project costs (lower fixed costs of office infrastructure and transport) and increases data quality through more careful supervision. Zomba district in the Southern region of Malawi was chosen as the site for this study for several reasons. First, it has a large enough population within a small enough geographic area rendering 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 has a high rate of school dropouts and low educational attainment. According to IHS-2 (2004), the biggest reason for dropout from school is financial. Finally, HIV/AIDS rates of women aged 15-49 in Zomba are the highest in the country at 24.6% (MDHS, 2004).

Because of Zomba district's particular characteristics with respect to its relatively high poverty and HIV prevalence, one might worry that the findings from the study may not be relevant for other parts of Malawi or for neighboring countries. While there is an element of truth in this for any impact evaluation in a particular setting, we feel that concerns for lack of external validity are minimal for our study. First, while Zomba district may be different than the rest of the country, it certainly is quite representative of the Southern Region (one of the three major regions of Malawi), which is home to two of the country's three biggest cities (Blantyre and Zomba). As such, we have no concern that regarding the relevance of the study for the region that Zomba lies in. As the Southern Region is the poorest one in the country with low educational outcomes and high HIV rates, it would be a natural place for the government to implement a similar program were it to consider geographic targeting.

Second, unlike many other districts, Zomba has the advantage of having a true urban center as well as rural areas. 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.

4. Research Design and Intervention

This paper is evaluating the impact of a randomized conditional/unconditional cash transfer intervention targeting young women in Malawi that provides incentives (in the form of school fees and cash transfers) to current schoolgirls and young women who have recently dropped out of school to stay in or return to school. Between October 2007 and January 2008, baseline surveys were conducted with 3,821 girls in 176 Enumeration Areas (EAs) in Zomba district of Malawi. These girls were selected based on information collected during a listing exercise, which involved going door to door to *all* households in these 176 EAs. This listing exercise identified all never-married, 13-22 year old females living in the area. We sampled all dropouts and 75-100% of current school girls, where the percentage sampled depended on the age of the girl. This sampling procedure led to an average sample size of 5.1 dropouts and 16.6 current school girls in each EA.[6]

Out of the 3,821 girls sampled in 176 EAs, 1,230 girls in 88 treatment EAs were sampled to receive cash transfers.[7] From December 2007 through January 2008 offers were made to all these girls and, except for a few girls who turned out to be ineligible, close to 100% accepted.[8] The offer consisted of a household transfer and a transfer directly to the girl, as well as full

[6] 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.

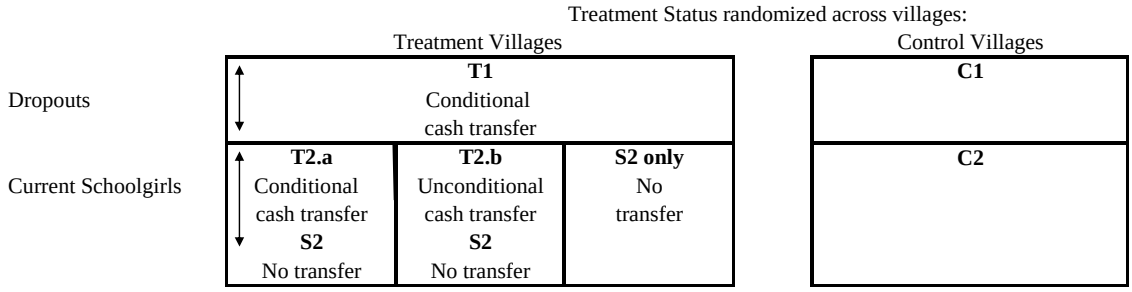
[7] Due to uncertainties regarding funding, the initial offers were only made for the 2008 school year (conditional on adequate school attendance for the girls receiving the conditional transfers). However, upon receipt of more funds for the intervention in April 2008, all the girls in the program were 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).

[8] Note that about 10% of girls did not attend these offer meetings, most of whom then received their offer letters at the first cash transfer point in February and entered the program thereafter.

payment of school fees for girls in secondary school.[9] The household amount was randomly varied across EAs from $4/month to $10/month, with all recipients in a given EA receiving the same amount. To determine 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.

We randomly assigned half of the 176 EAs to receive the intervention (treatment), and the rest serve as the control group. The following schematic best captures the remaining features of this intervention:

Malawi Research Design:



Within each *treatment* community, **all** never-married 13-22 year-old recent *dropouts* who are eligible to return to primary or secondary school are identified and **always** treated (with *conditional* cash transfers). We denote this core treatment group as T1. The same universe of would-be-eligible girls was identified in control communities, denoted by C1. Our second group of eligible girls are never married 13-22 year old *school girls* who are eligible to return to Standard 7-Form 4.[10] We randomly assigned treatment communities into three categories: those where *school girls* receive transfers *conditional* on school attendance (T2.a), those where *school girls* receive *unconditional* transfers (T2.b), and finally those where *no school girls* receive any cash transfers (S2). In addition, within T2.a and T2.b communities, a randomly selected subset of

[9] Students have to pay school fees at the secondary level in Malawi, but not at the primary level.
[10] 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.

school girls receives no transfers.[11] The sample of untreated *school girls* in treatment villages, i.e. in T2.a, T2.b, and S2 only, will allow us to identify any spillover effects of the program. This same universe of would-be-eligible *school girls* are also identified in the control communities, denoted by C2. Within treatment communities, we provide monthly cash transfers separately to the school girl and her parents/guardians as described above, and randomly vary the amount transferred to the parents/guardians *across* EAs, and the amount transferred to the girls *within* each EA. In the next subsection, we describe the design of the intervention in significantly greater detail.

4.1. Implementation of transfers/survey

The CCT program entailed sampling 3,821 young women from 176 EAs in Zomba district of Malawi. We started following these girls in the fall of 2007 and will continue following these individuals for at least two years. Enumerations areas (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). The remaining 470 are rural (population areas, or PAs). Our 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-step listing form. The first form, Form A, asked, for each household, 'do you have any never married girls in this household who are between the ages of 13 and 22?' This form allowed us to quickly identify households that had members that fit into our sampling frame, thus

[11] We randomly vary the percentage of school girls receiving transfers between 0%, 33%, 66% and 100% across treatment EAs.

significantly reducing the costs of listing. If we received a yes on Form A, then we moved to Form B which listed members of the household. For individuals in these households we asked the following additional questions:

1. Name
2. Age
3. Marital status
4. Current schooling status
5. If currently in school, level attending in 2007 for current school girls
6. If currently not in school, highest grade completed
7. If currently not in school, the last year during which they were in school

This information collected in Form B gave us a census of all girls within the target age range, and allowed us to categorize them into two groups:

A. Eligible dropouts, the majority of whom have been out of school for 2 years or fewer
B. Eligible schoolgirls, those in our age group and grade range that our still in school

These two groups comprised the basis of our sample frame. In each EA, we sampled an average of 5.1 dropouts and 16.7 schoolgirls. We sampled all eligible dropouts and 75%-100% of all eligible school girls, where the percentage depended on the age of the school-girl.[12]

Out of these 3,821 young women, 931 girls in 88 EAs were sampled to be part of the CCT program.[13] A core LSMS-type household questionnaire was administered to our entire core sample, both treatment and control, at baseline and will be repeated annually. This survey, described in more detail below, includes information on household characteristics, sexual behavior, and social networks.

[12] These percentages were lower for urban areas since the populations are much higher.
[13] An additional 299 girls were selected for unconditional offers.

Following the baseline survey, which took place in Fall 2007, girls were notified of their treatment status and were made offers to participate in the program. 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, it told secondary school CCT recipients that their school fees would be paid in full. The contract was then signed by both the recipients (guardian and core respondent) and the NGO 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 are 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. Due to security concerns with large amounts of cash, the location and the date for the cash payment point is changed each month and the recipients are informed about the next 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. After counting the amount and making sure it is correct, she signs acknowledging the receipt of the money. In between payment dates, the NGO collects attendance (and progress) records for all the students in the program to make sure that they are complying with the program requirements and attending school.

The cash transfers take place monthly and at each meeting some basic information is collected for each sample respondent, such as who is picking up the money (girl, guardian, or

proxy), how far they had to travel, etc. As part of the transfer program we also monitor the attendance of all the conditional cash transfer recipients and they only get the transfer if they have attended school at a satisfactory level during the previous month.

4.2. Survey Instrument

The annual SIHR Household Survey consists of a multi-topic questionnaire to be administered to the households in which the selected sample respondents reside. Although it is described as a household questionnaire, the primary goal of the SIHR Household Survey is to collect detailed information from the individual respondents selected for the survey. The survey consists of two parts: 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, consumption (food and non-food), household access to safety nets, and shocks (economic, health, and otherwise) experienced by the household. The core respondent provides further information about her family background, her education and labor market participation, her health, her dating patterns, sexual behavior, marital expectations, knowledge of HIV/AIDS, her social networks, as well as her own consumption of girl-specific goods (such as soaps, mobile phone airtime, clothing, braids, sodas and alcoholic drinks, etc.). Community characteristics are also collected in a separate short community questionnaire. A school questionnaire was administered at the end of the 2008 school year in all schools (treatment and control) attended by core respondents.

This paper utilizes baseline and follow-up data to analyze the one-year impact of the program on the marital status, childbearing, and the detailed sexual behavior for the program participants. All data utilized here are self-reported and the study team is in the process of collecting Biomarker data to complement these data.

5. Program Impacts

Before examining the short term impacts of the CCT program on sexual behavior, it is important to first confirm that our randomization, with respect to key outcomes and controls, was successful. Although we can control for baseline differences in these variables in our analysis, it makes the analysis of the data simpler if there were no significant differences at baseline. Table 1 shows the results of the randomization. As per our research design, we always compare treatment and control groups for dropouts and schoolgirls at baseline separately, and hence the equality of means at baseline is also examined within each of these two important sub-groups. Across the 10 variables that are most pertinent for this paper, there are no significant differences at baseline between the treatment and control groups for those who were dropouts at baseline. The fact that these variables look very similar across treatment and control is strong evidence that our randomization procedure was implemented successfully.

Table 1: Equality of Means at Baseline

	Dropouts (N=889)		School Girl (N=2285)	
	Control Mean	Treatment Difference	Control Mean	Treatment Difference
Age	17.434	-0.305	15.253	-0.097
Father Alive	0.643	-0.002	0.69	0.035
Mother Alive	0.784	-0.037	0.834	-0.027
Read English	0.469	-0.065	0.832	0.002
No Qualification	0.667	0.011	0.656	-0.016
Ever pregnant	0.436	-0.02	0.02	0.008
Never had sex	0.309	-0.017	0.795	0.006
Number of partners	1.135	0.031	0.268	-0.014

Notes: The entire sample was never married at baseline, so the control and treatment means were both zero. Dropout and school girl refer to schooling status at baseline. The sample was split into dropouts (girls not in school) and school girls at baseline, so the control and treatment means of schooling status were identical at baseline (dropouts were 100% not in school while school girls were 100% in school).

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

Table 2 shows that the panel data was successful in tracking more than 93% of the study sample in the one-year follow-up and that the panel data are balanced across treatment and control groups:

Table 2: Determinants of Survey Attrition

	ALL	**No S2**	**School Girls**	**School Girls**	**Dropouts**
=1 if Treatment Girl	0.005	0.006	0.011		
	(0.561)	(0.512)	(0.268)		
=1 if Conditional Schoolgirl				0.016	
				(0.158)	
=1 if Unconditional Schoolgirl				0.003	
				(0.863)	
=1 if Treatment Dropout					0.012
					(0.554)
Tracking Success	0.932***	0.931***	0.941***	0.941***	0.899***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	3,802	3,173	2,284	2,284	889

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

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

We follow the typical practice of estimating impacts using both Difference in Differences and individual-level fixed effects regressions in order to absorb as much residual variation as possible. Standard errors are clustered at the EA (village) level because this is the unit at which the treatment is administered (see Bruhn & McKenzie, 2008).

School Enrolment and Literacy:

We start by showing the impact of the program on schooling outcomes. It is fairly obvious that we would be much less likely to expect an impact on the sexual behavior, early marriage, fertility, and HIV risk of the young beneficiaries of the program in the absence of impacts on school enrolment, attendance, and attainment. Table 3 shows that the program led to large increases in school enrolment, especially among those who were not in school at baseline.

Column 2 of Table 3 shows that the percentage of initial dropouts who returned to school (and were in school at the completion of the 2008 school year) was 17.2% among the control group and 61.4% among treatment. Thus, program beneficiaries were 3-4 times more likely to be in school at the end of the 2008 school year than the control group.[14]

Table 3: Dependent Variable Enrolled in School

	All	Dropouts	All School Girls	Conditional	Unconditional	All School Girls
Post-Treatment Indicator	0.121***	0.442***	0.046***	0.038**	0.061***	0.061***
	(0.000)	(0.000)	(0.005)	(0.039)	(0.001)	(0.001)
Round 2 Indicator	-0.061***	0.172***	-0.108***	-0.108***	-0.108***	-0.108***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
=1 if Conditional Girl						-0.023
						(0.241)
Baseline Mean of Outcome in	0.826***	-0.000	1.000***	1.000***	1.000***	1.000***
Control	(0.000)	(1.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	5,922	1,608	4,314	3,782	3,354	4,314
Number of individuals	2,961	804	2,157	1,891	1,677	2,157

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

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

For those who were in school at baseline (initial schoolgirls), while the absolute numbers are smaller (due to continued high rates of schooling for this group), the relative impact is still impressive (column 3). 89.2% of initial schoolgirls were still enrolled in school at the end of the 2008 school year among the control group, compared with 93.8% in the treatment group. Thinking of these as dropout rates, the dropout rate of 6.2% among treatment is more than 40% lower than the 10.8% among controls. Column 6 of the same table shows that we find no statistically significant differences in the impact of the conditional vs. the unconditional treatments on school enrolment.

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

While the simple fact of attending school may be enough to alter behavior change regarding sexual activity among the study beneficiaries, it is also possible that what is learned at school is also a factor in causing behavioral change. For this reason, Table 4 examines outcomes in literacy in English, defined as the self-reported "ability to read a one-page letter" in that language. Initial dropouts in the program are significantly more likely to be literate in English than the controls after one year in the program. We find no differences for baseline schoolgirls, who have much higher levels of literacy than baseline dropouts.

Table 4: Dependent Variable is English Literacy

	All	Dropouts	All School Girls	Conditional	Unconditional	All School Girls
Post-Treatment Indicator	0.028	0.072**	0.019	0.030	-0.001	-0.001
	(0.199)	(0.012)	(0.452)	(0.336)	(0.983)	(0.983)
Round 2 Indicator	0.076***	0.025	0.086***	0.086***	0.086***	0.086***
	(0.000)	(0.202)	(0.000)	(0.000)	(0.000)	(0.000)
=1 if Conditional Girl						0.031
						(0.363)
Baseline Mean of Outcome in Control	0.760***	0.436***	0.829***	0.818***	0.841***	0.829***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	5,917	1,607	4,310	3,778	3,350	4,310
Number of individuals	2,959	804	2,155	1,889	1,675	2,155

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

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

Marriage and Fertility:

We now turn to early marriage and teen pregnancy as indicators of sexual behavior.[15] Table 5 presents the impact of the program on **never** having been married. As described earlier, the study sample was selected to be never-married at baseline, so levels of marriage are equal to the incidence during 2008. We see that 27.7% of initial dropouts in the control group have gotten married during the past year, compared with only 16.4% of the same group in treatment (columns 4-6). This is a reduction in the marriage rate of more than 40% among those who were not in

[15] The reader may object to marriages in this study being described as 'early' and pregnancies as 'teenage'. While it is true that the study sample does include some over the age of 19, this is a small percentage (approximately 7% at baseline and less than 13% at the end of Year 1) of the sample.

school at baseline. We also note that while the program had no effect on the baseline schoolgirls receiving conditional transfers (columns 7-9), it did significantly reduce the chances that those receiving the cash unconditionally were married at follow-up.

Table 6 describes the impact of the program on currently being pregnant. The analysis sample excludes the small number of girls who were pregnant at baseline, but the results do not change if they are included. Column 2 shows that 11.1% of initial dropouts among the control group were pregnant during follow-up data collection, compared with 8.0% of the program beneficiaries, a 25% reduction that is not statistically significant (p-value=0.15). Again, like marriage, the program had no impact on the fertility of baseline schoolgirls receiving CCTs, but significantly decreased the chances of pregnancy among program beneficiaries receiving the transfers unconditionally (columns 3-6).

Table 5: Dependent Variable is Never Married

	All	**Dropouts**	**All School Girls**	**Conditional**	**Unconditional**	**All School Girls**
Post-Treatment Indicator	0.026**	0.113***	0.010	-0.000	0.031***	0.031***
	(0.033)	(0.000)	(0.330)	(0.973)	(0.010)	(0.009)
Round 2 Indicator	-0.085***	-0.277***	-0.047***	-0.047***	-0.047***	-0.047***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
=1 if Conditional Girl						-0.031**
						(0.026)
Baseline Mean of Outcome in Control	1.000***	1.000***	1.000***	1.000***	1.000***	1.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	5,922	1,608	4,314	3,782	3,354	4,314
Number of individuals	2,961	804	2,157	1,891	1,677	2,157

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

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

Table 6: Dependent Variable is Currently Pregnant

	All	Dropouts	All School Girls	Conditional	Unconditional	All School Girls
Post-Treatment Indicator	-0.016**	-0.031	-0.014*	-0.005	-0.031***	-0.031***
	(0.045)	(0.151)	(0.067)	(0.590)	(0.000)	(0.000)
Round 2 Indicator	0.049***	0.111***	0.037***	0.037***	0.037***	0.037***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
=1 if Conditional Girl						0.026***
						(0.005)
Baseline Mean of Outcome in Control	-0.000 (1.000)	-0.000 (1.000)	-0.000 (1.000)	-0.000 (1.000)	-0.000 (1.000)	-0.000 (1.000)
Number of observations	5,822	1,530	4,292	3,762	3,338	4,292
Number of individuals	2,911	765	2,146	1,881	1,669	2,146

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

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

Sexual Activity, Risk Behaviors, and HIV knowledge/testing:

Finally, we present impacts on self-reported sexual activity, risky behaviors, knowledge about HIV/AIDS, and own serostatus. Table 7 examines onset of sexual activity and the number of lifetime partners. At baseline, 29.6% of initial dropouts and 79.3% of initial schoolgirls had never had sex. Columns 2-3 of Table 7 indicate that the reduction in the onset of sexual activity is 5.5 percentage points among initial dropouts and 2.6 percentage points among initial schoolgirls, which represent reductions in the onset of sexual activity of 46.6% and 32.5%, respectively. Columns 5-6 complement this finding and show that the self-reported number of lifetime partners is smaller for the program beneficiaries. The increase in the number of lifetime partners is approximately 25% lower for both initial dropouts and schoolgirls. The results suggest that program beneficiaries reduce their sexual activity by both delaying sex and reducing the number of partners they have sex with.

Table 7: Sexual Activity

Dependent Variable:	=1 if Never Had Sex			Number of partners ever		
	All	**Dropouts**	**All School Girls**	**All**	**Dropouts**	**All School Girls**
Post-Treatment Indicator	0.031**	0.055***	0.026**	-0.056**	-0.112**	-0.047**
	(0.010)	(0.007)	(0.048)	(0.024)	(0.020)	(0.050)
Round 2 Indicator	-0.086***	-0.118***	-0.080***	0.213***	0.428***	0.170***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Baseline Mean of Outcome in Control	0.707***	0.296***	0.793***	0.419***	1.141***	0.267***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	5,920	1,606	4,314	5,920	1,606	4,314
Number of individuals	2,960	803	2,157	2,960	803	2,157

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

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

Table 8 reports the impact of the program on the sexual behavior of those who are sexually active at both baseline and follow-up: condom use, frequency of sexual activity, and having sex with older partners. As the program has both an effect on the extensive margin, i.e. on whether to be sexually active in follow-up, and on the intensive margin, i.e. the safety of the sexual activity conditional on being sexually active, we face an identification problem for the latter. Hence, we ask the following question: “For the population of young women who would be active regardless of the existence of the program, what would be the effect of the program on their sexual behavior?” However, the young women we observe to be sexually active in both rounds include both this group, and the group who would have stopped being sexually active had they received the intervention, which introduces a selection bias that prevents us from interpreting the simple difference-in-differences estimates as the marginal effect of treatment on the population in question.

Table 8: Risky Sexual Activity

Dependent Variable:	Average Condom Use (1=Never, 5=Always)			`=1 if sexually active at least once a week			share of partners who are at least one year older		
	All	Dropouts	All School Girls	All	Dropouts	All School Girls	All	Dropouts	All School Girls
Post-Treatment Indicator	-0.086	-0.254	0.017	-0.135*	-0.048	-0.180**	-0.074	0.054	-0.148*
	(0.721)	(0.340)	(0.960)	(0.052)	(0.584)	(0.048)	(0.181)	(0.492)	(0.064)
Round 2 Indicator	0.153	0.356**	0.031	0.125***	0.178***	0.093	0.018	-0.046	0.057
	(0.328)	(0.041)	(0.878)	(0.005)	(0.005)	(0.110)	(0.606)	(0.345)	(0.279)
Baseline Mean of Outcome in Control	2.867***	2.556***	3.045***	0.176***	0.210***	0.156***	0.811***	0.802***	0.816***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	721	351	370	718	351	367	722	352	370
Number of individuals	361	176	185	359	176	184	361	176	185

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

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

In columns 1-3 of Table 8, we examine condom use and find no discernible impact of the program. In columns 4-6, we present the likelihood of having sexual intercourse at least once a week. We find that treatment girls are significantly less likely to have sexual intercourse on a weekly basis, a result mainly driven by the effect of the program on baseline schoolgirls. Similarly, the likelihood of having an older sexual partner decreased significantly for baseline schoolgirls in treatment (columns 7-9). If we believe that the treatment girls who stopped having sex had a lower propensity to engage in risky sexual behaviors, then the protective effects of the program found here are likely to be stronger. However, if they had a higher propensity to engage in risky behaviors when sexually active, then these estimates may be overstating the impact of the program.[16]

We conclude our analysis by examining whether the program had any effect on the likelihood of beneficiaries to get tested for HIV or their basic knowledge about the disease (Table 9). The first three columns of the table indicate that while there were large increases in being tested for HIV from baseline to follow-up, the rates in this increase were the same across

[16] We are currently analyzing the data to sign this selection bias, by examining the baseline risky sexual behaviors of those who stopped being sexually active. We have also tried to 'bound' our estimates using "Lee bounds", but as the effect of the treatment on the extensive margin is substantial, the bounds are too wide to be useful.

treatment and control.[17] So, it does not appear that being in school (and having a higher income) is associated with a higher probability of getting tested for HIV, at least no in this context. Columns 4-6 examine the differences in knowledge about the disease, defined as the number of correct answers (true or false) to four fairly simple statements on HIV/AIDS.[18] The sample mean for the number of correct answers to these four questions is high at 3.65 and the impact of the program is negative and almost significant at the 10% level among baseline dropouts (column 5: p-value=0.108). This, along with the significant and positive time trend for baseline dropouts, may indicate that the knowledge of HIV/AIDS (especially on mother to child transmission) is more likely to be acquired during pre-natal check-ups at health clinics rather than at school.

Table 9: HIV Testing and Knowledge

Dependent Variable:	=1 if ever tested for HIV			Knowledge of HIV/AIDS (scale: 0-4)		
	All	**Dropouts**	**All School Girls**	**All**	**Dropouts**	**All School Girls**
Post-Treatment Indicator	-0.022	-0.019	-0.023	-0.064	-0.112	-0.054
	(0.486)	(0.609)	(0.529)	(0.167)	(0.108)	(0.279)
Round 2 Indicator	0.331***	0.312***	0.334***	0.044*	0.092**	0.034
	(0.000)	(0.000)	(0.000)	(0.071)	(0.021)	(0.223)
Baseline Mean of Outcome in Control	0.249***	0.438***	0.209***	3.652***	3.568***	3.669***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	5,907	1,603	4,304	5,899	1,597	4,302
Number of individuals	2,954	802	2,152	2,950	799	2,151

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

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

6. Conclusions + Policy Implications

While there have been several evaluations of the impact CCT programs have on school attainment and learning, early childhood development, and adult health, no one has studied the possible effect of these programs on the sexual behavior of the beneficiaries and their subsequent

[17] The large increase in the likelihood of having been tested for HIV is likely the result of a pilot campaign by the National AIDS Commission (NAC) that conducted a door-to-door testing campaign in Zomba in 2008.

[18] These statements are: “A pregnant woman can transmit the AIDS virus to her unborn child”, “A woman can transmit the AIDS virus to her child through her breast milk”, “You can get AIDS if you have sex with someone who looks perfectly healthy”, and “AIDS has a cure”.

HIV risk. This is potentially a very important impact to document in sub-Saharan Africa, where CCT programs are likely to become more common in the near future and the risk of HIV infection is disproportionately high among young women and school-aged girls.

Education has been suggested as a 'social vaccine' to prevent the spread of HIV, while both schooling and poverty reduction (especially for women) are seen by many as key components in a comprehensive strategy to combat HIV/AIDS. However, causal evidence that links increased schooling or income to reduced risk of contracting HIV is very limited. Most of what we know about the relationship between schooling (attendance or attainment) and HIV risk comes from cross-sectional studies. Commenting on the lack of clear and credible evidence addressing the relationship between education and HIV, Jukes, Simmons, and Bundy (2008) suggest that long-term, follow-up experimental interventions to improve educational access, such as conditional cash transfer programs, offer the potential to examine the causal relationship between educational attainment and risk of HIV infection. This paper presents the initial results of exactly such a randomized conditional cash transfer program implemented in Malawi.

The results are promising. After one year, the program led to large increases in school enrolment (and some improvement in literacy), declines in early marriage, teen pregnancy, sexual activity, and risky sexual behavior. Most of these effects are large and significant, especially for those who had already dropped out of school at baseline. The evidence presented here suggests that as girls and young women returned to (or stayed in) school, they significantly reduced their sexual activity. The program also delayed marriage – which is the main alternative for schooling for young women in Malawi – and increased age at first pregnancy. As the treatment/control differences in schooling among baseline schoolgirls become starker in year 2, impacts on sexual behavior within that group might likely become even stronger.

It remains to be seen whether the longer-term impacts of the program will be as strong as the short-term impacts described in this paper. One should also not assume that the changes in self-reported sexual behavior, even if very accurate, will result in a decline in HIV rates among

this cohort of program beneficiaries. It is possible that the program simply delays the inevitable, rather than improving well-being and reducing HIV incidence in the long-run. Future rounds of household survey and Biomarker data collection will shed light on these questions.

For now, however, CCT programs for schooling in the context of poor sub-Saharan countries with high rates of HIV infection among young people seem like “win-win” programs, as they may not only increase schooling significantly, but also reduce risk of HIV infection at the same time. Furthermore, increases in age at first marriage, first pregnancy, and educational attainment may lead to improved outcomes for the next generation, as the children of this cohort of program beneficiaries are likely to be born into better circumstances and enjoy better care during early childhood. The evidence presented in this paper may likely provide an additional impetus for the expansion of CCT programs (which already cover much of Latin America) to the African continent.

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