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1. Introduction

Conditional Cash Transfers (CCTs) can be an important component of social protection policy and there is "…considerable evidence that CCTs have improved the lives of poor people" (World Bank, 2009). Early CCT pilots such as Mexico's *Progresa,* Brazil's *Bolsa Escola*, and Nicaragua's *Red de Protección* have been popular and became national programs a few years later. As of 2007, "…29 developing countries had some type of CCT program in place (in some cases, more than one) and many other countries were planning one." (World Bank, 2009) It seems that CCT programs are here to stay – at least for the foreseeable future.

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

When available, evidence as to the impact of CCT programs on "final" educational outcomes such as test scores, shows only modest effects (World Bank, 2009). More importantly for Sub-Saharan Africa (SSA), very little is known about the possible effect of these programs on the sexual behavior of the target beneficiaries, including age

of marriage and fertility decisions.[4] Given the close link between sexual behavior and HIV infection, and given the burden HIV poses on these economies, this is potentially a very important impact to document.

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

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

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

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

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

2. Literature Review

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

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

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

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

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

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

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

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

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

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

3. Study Setting

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

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

with most people living from subsistence farming supplemented by small-scale income-generating opportunities that are typically more available to men than they are to women. The country is poor even by African standards: the GNI per capita (PPP, current international $) is $750 in 2007, compared to an average of $1,870 for Sub-Saharan Africa (World Bank, 2008).[7] Malawi also has the eighth-highest HIV prevalence in the world with 14 percent of the adult population infected (UNAIDS, 2007).[8] The gender gap in HIV prevalence among young adults, aged 15-24, is startling: prevalence was more than *four* times higher for females than males in 2004.

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

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

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

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

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

4. Research Design and Intervention

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

4.1. Implementation of transfers

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

4.2. Survey Instrument

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

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

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

sampled girl from our target population. Part I collects information on the household roster, dwelling characteristics, household assets and durables, consumption (food and non-food), household access to safety nets, and shocks (economic, health, and otherwise) experienced by the household. In Part II, the core respondent provides further information about her family background, her education and labor market participation, her health, her dating patterns, sexual behavior, marital expectations, knowledge of HIV/AIDS, her social networks, as well as her own consumption of girl-specific goods (such as soaps, mobile phone airtime, clothing, braids, sodas and alcoholic drinks, etc.). This paper utilizes baseline and follow-up data to analyze the one-year impact of the program on the marital status, childbearing, and the detailed sexual behavior for the program participants. The first round of the survey was administered from October 2007 to February 2008, and the second wave from October 2008 to February 2009.

5. Program Impacts

5.1. Balance and Attrition

Before examining the short-term impacts of the CCT program on sexual behavior, it is important to first confirm that our randomization, with respect to key outcomes and controls, was successful. Table 1a provides some basic summary statistics from baseline data. Table 1b shows the success of the randomization. As per our research design, we always compare treatment and control groups for dropouts and schoolgirls at baseline separately, and hence the equality of means at baseline is also examined within each of these two important sub-groups. Across the thirteen variables that are most pertinent for this paper, there are no significant differences at baseline between the treatment and

control groups for those who were dropouts at baseline. Among baseline school girls, the only variable that is significantly different between treatment and control is whether the girl resides in a female headed household, where those in treatment are significantly less likely to reside in a female headed household. The fact that these variables look very similar across treatment and control is strong evidence that the randomization procedure was implemented successfully.[21]

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

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

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

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

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

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

5.2. School Enrollment

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

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

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

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

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

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

5.3. Marriage and Fertility

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

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

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

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

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

5.4. Sexual Activity and Risk Behaviors:

Finally, we present impacts on self-reported sexual activity and risky behaviors. Table 6a examines onset of sexual activity and the number of sexual partners in the past 12 months. At baseline, 29.6% of initial dropouts and 79.4% of initial schoolgirls reported having **never** had sex. Columns 2-3 of Table 6a indicate that the reduction in the onset of sexual activity is 5.5 percentage points among initial dropouts (significant at the 1% level) and 2.5 percentage points among initial schoolgirls (with a p-value of 0.112), which represent reductions in the onset of sexual activity of 46.6% and 31.3%, respectively. Columns 5-6 complement this finding and show that the change in the number of self-reported number of lifetime partners from baseline to follow-up is smaller for the program beneficiaries. The decrease in the number of lifetime partners is approximately 25% lower for both initial dropouts and schoolgirls, although the

difference is only statistically significant among baseline dropouts. These results suggest that program beneficiaries reduce their (self-reported) sexual activity by both delaying sexual activity and reducing the number of sexual partners. Table 6b extends the analysis by examining the sub-sample of young women who haven't yet gotten married by Round 2 and finds similar impacts – suggesting that the reduction in self-reported sexual activity is not due solely to delayed marriage in this population.

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

In columns 1-3 of Table 7, we examine self-reported condom use and find no discernible impact of the program. In columns 4-6, we present the likelihood of having sexual intercourse at least once a week. We find that treatment baseline schoolgirls are significantly less likely to have sexual intercourse on a weekly basis, but we find no

significant impact for baseline dropouts. Similarly, the likelihood of having an older sexual partner is lowered significantly for baseline schoolgirls in treatment (columns 7-9). If we believe that the treatment girls who stopped having sex had a lower propensity to engage in risky sexual behaviors, then the protective effects of the program found here are likely to be stronger, and vice versa.[26]

6. Conclusions

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

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

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

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

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

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

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Table 1a: Summary Statistics (N=2893)

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

Table 1b: Equality of Means at Baseline

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

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

Table 2: Determinants of Survey Attrition

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

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

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

Table 3: Dependent Variable is Enrolled in School

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

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

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

Table 4: Dependent Variable is Never Married

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

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

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

Table 5: Dependent Variable is Ever Pregnant

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

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

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

Table 6a: Sexual Activity

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

Table 6b: Sexual Activity for Unmarried Subsample

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

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

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

Table 7: Risky Sexual Activity

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

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

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