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The Short-Term Impacts of a CCT Program for Schooling on the Sexual Behavior of Young Women [1]

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Abstract

In the past decade evidence has emerged from many parts of the world that Conditional Cash Transfer Programs (CCTs) for schooling are an effective way to boost enrolment. However, there is good reason to believe that such programs can also affect other outcomes, such as the sexual behavior of their young beneficiaries. The Zomba Cash Transfer Program (ZCTP) is a randomized CCT intervention targeting young women in Malawi that provides incentives (in the form of school fees and cash transfers) to current schoolgirls and recent dropouts to stay in or return to school. An average offer of US$10/month conditional on satisfactory school attendance – plus direct payment of secondary school fees – led to significant declines in early marriage, teenage pregnancy, and sexual activity among program beneficiaries after just one year of program implementation. The effects were particularly strong among those who had already dropped out of school at baseline: the program reduced their likelihood to be married by approximately 40% and to start childbearing by more than 30%. Onset of sexual activity was also delayed among all program beneficiaries. Overall, these results suggest that CCT programs not only serve as useful tools for improving school attendance, but may also reduce sexual activity, age at marriage and teen pregnancy.

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

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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 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, such programs have been largely evaluated on a small set of outcomes, which have more to do with the behavior that the program is being "conditioned" on (such as school enrolment), rather than, say, learning or labor market outcomes. Naturally, there is now an increased focus, from policy-makers and researchers alike, on examining a broader set of outcomes that might be plausibly affected by these programs and that are pertinent for policy design.

When available, evidence on the impact of CCT programs on "final" outcomes, such as learning, 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 "final" outcomes in general, and more specifically on the sexual behavior of the beneficiaries (including age of marriage and fertility decisions) and their subsequent HIV risk.[3] Given the high prevalence of HIV infection among young people in SSA and

[3] 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 this study is significantly lower than that of the program being evaluated here.

the burden AIDS 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 and their subsequent risk of HIV infection. Education has been suggested as a "social vaccine" to 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 and HIV risk. Given the high prevalence of HIV infection among young women in SSA, the policy importance of identifying any potentially large impacts of CCT programs for schooling on HIV prevention 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 sexual behavior of the young, female beneficiaries of the program. It does so by examining the one-year impacts of a two-year randomized intervention in Malawi that provides cash transfers to young women to stay in (or return to) school. As such, this paper provides the first experimental evidence on the impact of a CCT program 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 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, 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).[4] 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 their children using health facilities on a regular basis and attending school between the third year of primary school and the third year of secondary school (de Janvry and Sadoulet, 2004).

World Bank (2009) finds that CCTs led to large increases in school enrolment, particularly among those with low enrolment rates to begin with. However, evidence on the impact of CCTs on 'final outcomes', such as learning, is not as encouraging, with a

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

number of studies finding no effects even after controlling for school selection. Filmer and Schady (2009) argue that the lack of any discernible effect of such programs on learning (despite large impacts on school enrolment) may be due to the fact that they draw lower ability students back to school.

To our knowledge, no CCT program for schooling has been evaluated to assess its possible impact on the sexual behavior of the young people benefitting 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. 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. 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 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. 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.

Credible causal evidence regarding the effect of increased income on subsequent risk of HIV infection among young people is also practically 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, 2005a; 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.[5] Finally, using prime-age adult mortality as a proxy measure for HIV/AIDS affected households; several studies find that higher income

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

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, 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).[6] Malawi also has the eighth-highest HIV prevalence in the world with 14 percent of the adult population infected (UNAIDS, 2007).[7] The gender gap

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

[7] 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).

in HIV prevalence among young adults, aged 15-24, is startling: prevalence was more than *four* times higher for females than males in 2004.

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. 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 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 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.[8] 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 within Malawi are relative.

4. Research Design and Intervention

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. 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 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 3,821 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. For the study, we sampled **all** dropouts and 75-100% of current school girls, where the exact

[8] 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.

percentage sampled depended on the age of the school-girl.[9] In addition, the majority of these girls were eligible to return to Standard 7-Form 4.[10] This sampling procedure led to an average sample size of 5.1 dropouts and 16.6 current school girls at baseline in each EA.[11]

Out of these 3,821 young women, 1,230 girls in 88 randomly selected EAs were sampled to be part of the CCT program.[12] A household questionnaire (not unlike a Living Standards Measurement Survey, or LSMS) was administered to our entire core sample – both treatment and control – at baseline and follow-up, which were conducted 12 months apart. This survey, described in more detail below, includes information on household characteristics, school enrolment, sexual behavior, and social networks.

From December 2007 through January 2008 offers to participate in the CCT program were made to the selected girls in treatment villages, and, except for a few girls who turned out to be ineligible, close to 100% accepted.[13]. As part of the offer, a detailed informational sheet was given to each household that detailed the conditions of the contract. In addition, it told secondary school CCT recipients that their school fees would

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

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

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

[12] 299 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.

[13] 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 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).

be paid directly to their school in full.[14] 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.[15] $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.[16] In addition to the transfers to the household, secondary school fees were paid directly to the schools upon confirmation of enrolment.[17]

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.[18] At each meeting some basic information is collected for each sample respondent, such as who is

[14] 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.

[15] The intervention being evaluated here is part of a larger experiment, with a complex set of treatment arms, where transfer size was randomly varied across treatment units and the transfers 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 existence of a causal relationship between a "typical" CCT program and a change in the sexual behavior of its young beneficiaries, as well as the "average" size of this potential impact.

[16] 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.

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

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

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.[19] Each household receives the transfer only if the young woman attended school for at least 80% 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 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

[19] 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.

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.

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 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 ten 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 age, where those in treatment are younger by less than a third of a year. The fact that these variables look very similar across treatment and control is strong evidence that the randomization procedure was implemented successfully.[20]

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. That the panel data are balanced

[20] Please also notice 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.

across treatment and control groups indicates that this small sample attrition will not introduce any bias into the estimation of treatment impacts.

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

$$Y_{it} = a_i + d_t + b(T_i * d_t) + e_{it},$$

where a_i represents a set of individual-level fixed effects, d_t is a dummy variable for the second round, and the interaction term $(T_i * d_t)$ 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 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 $\hat{b}$ therefore gives the intention-to-treat effect of the program on the average girl in our study EAs.[21]

5.2. School Enrolment

We start by showing the impact of the program on 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 impacts on school attendance and attainment. The simple act of attending school may be enough to

21 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%).

cause sexual behavior change among the study beneficiaries – for example by raising the opportunity cost of pregnancy (Jukes, Bundy, and Simmons, 2008).[22]

Table 3 shows that the program led to large increases in school enrolment, especially among those who were not in school at baseline. Column 2 of Table 3 shows that the percentage of initial dropouts who returned to school (and were in school at the end of the 2008 school year) was 17.2% among the control group compared with 61.4% 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.[23]

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, 89.2% 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 10.8% among controls to 7% among treatments.

5.3. Marriage and Fertility

[22] 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.

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

We now turn to early marriage and teen pregnancy as indicators of sexual activity.[24] 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 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 – 4.6% 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 6 examines onset of sexual activity and the number of lifetime partners. At baseline, 29.6% of initial dropouts and 79.5% of initial schoolgirls had **never** had sex.

[24] 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.

Columns 2-3 of Table 6 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, although the difference is only statistically significant among baseline dropouts. The results suggest that program beneficiaries reduce their (self-reported) sexual activity by both delaying sexual activity and reducing the number of sexual partners.

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 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 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.[25]

6. Conclusions

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 young beneficiaries and their subsequent 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

[25] We have also tried to 'bound' our estimates using "Lee bounds" (Lee, 2005) but as the effect of the treatment on the extensive margin is substantial, the bounds are too wide to be useful.

very limited. While most of what we know about these relationships comes from cross-sectional studies, the existing evidence is suggestive of the possibility that CCT programs for schooling may also have a significant impact on the sexual behavior and the subsequent HIV risk 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 school enrolment, as well as declines in early marriage, teenage 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 also become 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 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 the African continent.

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Table 1: Equality of Means at Baseline

	Dropouts (N=889)		School Girl (N=2002)	
	Control Mean	Treatment Difference	Control Mean	Treatment Difference
Age	17.434	-0.305	15.255	-0.298**
Father Alive	0.643	-0.002	0.698	0.017
Mother Alive	0.784	-0.037	0.837	-0.032
Read English	0.469	-0.065	0.829	-0.028
No Qualification	0.667	0.011	0.657	-0.022
Ever pregnant	0.436	-0.02	0.022	0.007
Never had sex	0.309	-0.017	0.793	0.006
Number of partners	1.135	0.031	0.266	-0.008

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

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

Table 2: Determinants of Survey Attrition

	ALL	**School Girls**	**Dropouts**
=1 if Treatment Girl	0.004	0.016	
	(0.687)	(0.158)	
=1 if Treatment Dropout			0.012
			(0.554)
Tracking Success	0.931***	0.941***	0.899***
	(0.000)	(0.000)	(0.000)
Number of observations	2,891	2,002	889

Note: Each column represents an OLS regression with robust standard errors. P-values in parentheses.

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

Table 3: Dependent Variable Enrolled in School

	All	Dropouts	School Girls
Post-Treatment Indicator	0.163***	0.442***	0.038**
	(0.000)	(0.000)	(0.042)
Round 2 Indicator	-0.061***	0.172***	-0.108***
	(0.000)	(0.000)	(0.000)
Baseline Mean of Outcome in Control	0.800***	-0.000	1.000***
	(0.000)	(1.000)	(0.000)
Number of observations	5,390	1,610	3,782
Number of individuals	2,695	805	1,891

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. P-values in parentheses.

*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	0.008	0.113***	-0.001
	(0.561)	(0.000)	(0.949)
Round 2 Indicator	-0.085***	-0.277***	-0.046***
	(0.000)	(0.000)	(0.000)
Baseline Mean of Outcome in Control	1.000***	1.000***	1.000***
	(0.000)	(0.000)	(0.000)
Number of observations	5,390	1,608	3,782
Number of individuals	2,695	804	1,891

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. P-values in parentheses.

*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	-0.005	-0.051**	-0.001
	(0.716)	(0.033)	(0.966)
Round 2 Indicator	0.085***	0.162***	0.069***
	(0.000)	(0.000)	(0.000)
Baseline Mean of Outcome in Control	0.096***	0.434***	0.025***
	(0.000)	(0.000)	(0.000)
Number of observations	5,296	1,608	3,780
Number of individuals	2,648	804	1,890

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. P-values in parentheses.

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

Table 6: Sexual Activity

Dependent Variable:	=1 if Never Had Sex			Number of partners ever		
	All	**Dropouts**	**School Girls**	**All**	**Dropouts**	**All School Girls**
Post-Treatment Indicator	0.029**	0.055***	0.025*	-0.035	-0.112**	-0.038
	(0.023)	(0.007)	(0.099)	(0.221)	(0.020)	(0.177)
Round 2 Indicator	-0.086***	-0.118***	-0.080***	0.212***	0.428***	0.169***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Baseline Mean of Outcome in Control	0.695***	0.296***	0.795***	0.440***	1.141***	0.265***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	5,388	1,606	3,782	5,388	1,606	3,782
Number of individuals	2,694	803	1,891	2,694	803	1,891

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. P-values in parentheses.

*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	-0.064	-0.254	0.046	-0.121	-0.048	-0.205*	-0.067	0.054	-0.159*
	(0.816)	(0.340)	(0.920)	(0.112)	(0.584)	(0.054)	(0.266)	(0.492)	(0.092)
Round 2 Indicator	0.153	0.356**	0.031	0.125***	0.178***	0.093	0.018	-0.046	0.057
	(0.329)	(0.041)	(0.878)	(0.005)	(0.005)	(0.111)	(0.606)	(0.345)	(0.280)
Baseline Mean of Outcome in Control	2.849***	2.556***	3.061***	0.181***	0.210***	0.159***	0.808***	0.802***	0.813***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
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. P-values in parentheses.

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