RESEARCH PROPOSAL

Unpacking the Impacts of a Randomized CCT program in sub-Saharan Africa

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1. Summary

The forthcoming World Bank Policy Research Report (PRR, forthcoming), titled "Conditional Cash Transfers for Attacking Present and Future Poverty" states that 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." 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, designing a new CCT program remains a complex task. Many difficult decisions need to be made regarding the selection of beneficiaries, the nature (and enforcement) of conditions, and the level and structure of payments. While numerous evaluations of CCTs have been conducted in Latin America, the evidence base needed by a government to design a new CCT program is either limited or non-existent in several important areas discussed below.

First, the question of whether the observed effects of a CCT program are a result of the "income effect" associated with the transfer or the "price effect" from the condition remains largely unanswered. As the PRR (forthcoming) convincingly argues, this issue is of much more than academic interest, because it has direct implications on program design. The ideal experiment to answer this question – i.e. a randomized controlled trial with one treatment arm receiving *conditional* cash transfers, another receiving *unconditional* transfers, and a control group receiving *no* transfers – has not yet been conducted anywhere. The evidence that can be gleaned so far is either from model-based simulation exercises (e.g. Bourguignon, Ferreira, and Leite, 2003; Todd and Wolpin, 2006) or from interventions with implementation glitches in Mexico and Ecuador (De Brauw and Hoddinot, 2007; Schady and Araujo, 2008).

Second, while "…the key parameter in setting the benefit levels in CCT programs is the size of the elasticity of the relevant outcomes to the benefit levels" (PRR, forthcoming, pp. 135), random variation in transfer size among program participants is rarely, if ever, observed. Nor has

the related issue of to whom the transfer should be made been studied extensively. While there are a few studies examining the effect of making the transfer to the mother or the father, we know of no impact evaluations assessing the impact of splitting the transfer payments between the student and his/her parent/guardian.[1]

Third, 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.

Finally, impact evaluations of CCT programs are non-existent for SSA.[2] The fact that much of what is known about the effectiveness of CCT programs is based mainly on evaluations in Latin America (and a few countries in Asia) is not encouraging for those hoping to implement them in SSA, given that these countries are significantly poorer and may have weaker institutions.

The study for which additional funding is being requested in this proposal is designed to evaluate a two-year randomized intervention in Malawi that provides cash transfers to current schoolgirls (and young women who have recently dropped out of school) to stay in (and return to) school. Through the use of a unique research design, we hope to contribute to the literature by examining the abovementioned questions – the understanding of all of which is critical to inform effective policy interventions.

First, each of the 176 enumeration areas (EA) that are home to the study sample of 3,821 girls were randomly assigned treatment or control status. Each treatment EA was then randomly assigned to receive either *conditional* or *unconditional* transfers. This experimental design allows the study team to isolate the impact of the conditionality on various outcomes of interest.

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[1] Except Ashworth et al. 2002 that examines this question for a program in the UK.
[2] An exception is the "Going to Scale" program in South Africa, whose economy resembles that of a Latin American country rather than a poor sub-Saharan African one.

Second, transfer size was randomly varied within and across treatment EAs to estimate the size of the elasticity of relevant outcomes to the benefit level. 'Pure' income elasticity can be estimated by restricting the analysis to only those receiving *unconditional* transfers. In addition, separate transfers were made to parents and students (the size of each of which were randomized), which will allow experimental identification of the relative impact of these transfers.

Third, the study is designed to evaluate the impact of a CCT program for schooling on various demographic and health outcomes of its target population, such as nutritional health, sexual behavior, fertility, and subsequent HIV risk (both in the short- and long-run). For young women in SSA, among whom the prevalence of HIV is sharply higher than their male counterparts, several recent studies have shown a link between increased school attendance and a decreased likelihood of HIV infection (e.g. Duflo et al., 2006 or Beegle and Özler, 2007). However, in order to convincingly test the presence of a causal link, a thorough impact evaluation study such as this is required. The study involves the collection of rich panel data utilizing both individual and household surveys, as well as biomarker tests for HIV, other STDs, anemia, malaria, and anthropometric measurements.

As discussed in Behrman & King (2008), evaluations that focus only on short-term outcomes can do a disservice to our understanding of how to achieve meaningful, long-term improvements in education and health. Because the intervention we are evaluating creates cohorts that were randomly exposed to additional schooling and income at a young age, we can track this cohort over time to understand both short- and long-run effects, as well as to assess how impacts vary with baseline characteristics, such as the age at first exposure.

The CCT program started at the beginning of the Malawian school year in January, 2008 and will continue for two years until November, 2009. Baseline data collection was conducted in the autumn of 2007 and follow-up data collection to assess the one-year impact of the program started in the autumn of 2008. The major budgetary cost to be financed with the funding requested under this proposal is the second follow-up data collection effort planned to take place in the autumn of 2009. This third round of data collection is critical for the success of our study as

it will take place at the conclusion of the two-year intervention.

The study will produce two sets of products that have overlapping audiences. A large number of working papers, journal articles, policy notes, as well as tailored reports for various departments of the Government of Malawi will be produced by the study team – targeting both the research community and development practitioners in the Bank, donor agencies, NGOs, and other policy-makers in the region. In addition, the data sets will be made publicly available.

2. Motivation and Context

Many developing countries have some type of a CCT program in place, while many others are either piloting or planning one. In addition, many developing countries have various cash transfer programs in place but are not sure whether the transfers should be conditioned on certain actions by the recipients, such as keeping children in school or taking infants to health clinics.

However, while the amount of evidence we have on the impact of CCT programs is impressive, especially when compared with how much we know about other types of programs in developing countries, there are also areas of critical policy importance about which we know very little. These include design features of CCTs, the different settings in which they are implemented, and impacts on various outcomes that have previously been overlooked.

Regarding the decisions needed to be taken while designing a new CCT intervention; a policy maker faces choices along many different dimensions. These include whether to make the transfers conditional or not (if so, what to condition the transfers on and whether to monitor the condition), the amount and frequency of transfers, and the recipient(s) within the household. The ideal experiment to answer some of these questions – i.e. a randomized controlled trial with one treatment arm receiving *conditional* cash transfers (of varying amounts to different recipients), another receiving *unconditional* transfers, and a control group receiving *no* transfers – has not yet been conducted anywhere.

While the next generation of CCT programs will increasingly take place in sub-Saharan Africa, impact evaluations of such programs in SSA are basically non-existent. It is hard to argue that policy-makers in the region can expect similar average impacts that are observed in impact evaluations elsewhere, when an overwhelming majority of the evidence comes from Latin America. Countries in SSA are, on average, significantly poorer, have weaker institutional settings, and may have different priorities in terms of outcomes of interest.

Finally, 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 proposed here, that provides randomly varied amounts of cash transfers to young individuals and their guardians is the perfect setting 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 research proposal presented here aims to provide answers to these questions outlined above. Given that providing assistance with the design of CCT programs in client countries (in addition to financial support in the form of loans and/or grants) is at the core of the work of many sectors of the World Bank (social protection, human development, gender), the results of the study will be of direct interest to our colleagues within the Bank.

2.1. Project Objectives

In the preceding section, we argued that despite the wealth of credible evidence that exists regarding the impact of CCT programs, there are important questions that remain

unanswered. These questions are much more than of just academic interest, as they have direct implications for the design of these interventions. What is required is to 'unpack' the impacts reported in earlier studies by providing the detail and nuance in program design that is usually missing from a typical CCT project. Hence, the objective of the proposed study here is to provide credible evidence on issues about which we still know very little.

Specifically, the main questions the study will try to answer are the following:

1. Are the observed effects of a CCT program a result of the "income effect" associated with the transfer or the "price effect" from the conditionality imposed on the recipient?

2. What is the size elasticity of any outcome of interest to the benefit level set by the program?

3. Do the impacts vary by the amount given to the parents (or the guardian) of the student instead of that transferred directly to the student herself? How does changing this split alter the composition of household consumption?

4. Do CCT programs for schooling have any positive health impacts, including prevention of STDs such as HIV/AIDS among young people?

Among the larger set of questions that are of both academic and policy interest and can be answered using the data from this study are the following:

a. Are there any negative (or positive) spillover effects from CCT programs for schooling?

b. Can CCTs, carefully designed to target adolescent girls, improve access to (and success in) the labor market by keeping them in school and reducing their economic dependency (on their boyfriends and their families), and if so under what conditions and for which sub-groups are they most effective?

2.2. Literature review

To inform the design of this project, the research team has thoroughly examined the design and impact of conditional cash transfer programs throughout the developing world. In addition, the research team has also reviewed the body of evidence on the possible impact of schooling (and income) on the risk of HIV infection, particularly for AIDS-affected countries in Sub-Saharan Africa. In the following subsections we review some of this work and highlight issues that are of particular salience for our study design.[3]

2.2.1. Disentangling the 'price effect' from the 'income effect' in CCT Programs

From a program design standpoint, it is important to know whether the impact of CCT programs are a result of the income effects associated with the transfers, or the price changes implicit in the condition, or both. Conducting randomized pilots to answer this question can be time consuming and expensive, so experimental evidence is not available to shed light on this issue. What we do know on the topic comes mainly from accidental glitches in program implementation or structural models of household behavior.

Evidence on the effect of the conditionality on school enrolment points us in favor of the conditions. Based on the fact that some households in Mexico and Ecuador did not think that the cash transfer program in their respective country was conditional on school attendance, de Brauw and Hoddinott (2007) and Schady and Araujo (2008) both find that school enrolment was significantly lower among those who thought that the cash transfers were unconditional.

Ex-ante program evaluations provide further evidence that the impacts on various schooling related outcomes would have been significantly attenuated without the conditionality. In Brazil, Bourguignon, Ferreira, and Leite (2003) find that unconditional transfers would have

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[3] In this subsection, we draw substantially from the forthcoming World Bank Policy Research Report, titled "Conditional Cash Transfers for Attacking Present and Future Poverty".

no impact on school enrolment; while Todd and Wolpin (2006) report that the impact of unconditional transfers on attainment would be only 20% of that of conditional transfers.

Finally, there is some evidence that the condition that pre-school children receive regular check-ups at health clinics (enforced by a social marketing campaign, but not monitoring the condition) had a significant impact on child cognitive outcomes, physical health, and fine motor control. Two studies in Latin America (Paxson and Schady, 2007; Macours, Schady, and Vakis, 2008) show behavioral changes in parents' behaviors and household spending patterns that are inconsistent with changes in *just* the household income. The studies, however, cannot isolate the impact of the social marketing campaign from that of the transfers being made to women.

The evidence presented here points to the notion that the conditions under which cash transfers are made to households are important and that unconditional transfers are likely to be less effective in obtaining the desired behavioral change – at least for the outcomes examined in the literature. To our knowledge, there are two other studies that plan to examine the impact of the conditionality in the near future. "Impact Evaluation of a Randomized Conditional Cash Transfer Program in Rural Education in Morocco" has three treatment arms: unconditional, conditional with minimal monitoring, and conditional with heavy monitoring (using finger printing machines at schools). A similar pilot in Burkina Faso has comparative treatment arms for conditional and unconditional transfers. Accumulation of reliable evidence on the effect of the conditionality on various outcomes of interest from the study proposed here and these other studies promises to be of significant use to policy-makers designing cash transfer programs in the near future.

2.2.2. Elasticity of relevant outcomes to the benefit levels

As the forthcoming PRR convincingly argues, the key parameter in setting the benefit levels in CCT programs is the size of the elasticity of the relevant outcomes to the benefit levels. Several programs, such as PROGRESA in Mexico or PRAF in Honduras, set their transfer sizes

to cover the opportunity costs of attending school and, in the case of the latter, direct costs of schooling.

To our knowledge, there are no CCT programs under which the transfers are randomly varied across beneficiary households to estimate how school enrolment, attendance, or attainment may improve as the transfer amount is increased. Again, with one exception (discussed below), the only evidence we have comes from structural models that simulate the expected impacts of different transfer amounts on various outcomes. Bourguignon, Ferreira, and Leite (2003) find that doubling the transfer amount under Brazil's Bolsa Escola would have halved the percentage of children in poor households not attending school; while Todd and Wolpin (2006) estimate that incremental increases in transfer size in Mexico would have diminishing effects on school attainment. It is worth noting that these estimates are not pure elasticities as they incorporate the impact of the conditionality of the amount transferred. Pure elasticities can be estimated by varying unconditional transfer amounts.

One study that addresses the issue of the impact of transfer size on enrolment is from Cambodia (Filmer and Schady, 2008). The program offered varying amounts of cash to students based on their poverty status at baseline. Using a regression discontinuity design, the authors find that while the difference between the impact of a $45 scholarship and no scholarship was large, the difference between the impact of a $60 scholarship and the $45 scholarship was quite small. Their findings are consistent with those from structural models reported above.

2.2.3. Does it matter to whom the cash transfers are made?

Almost all CCT programs make their payments to women (mothers or other female guardians) in the household. While there are a few studies that point to improved outcomes as a result of the transfer being made to women in the beneficiary households, there is virtually no evidence from developing countries on whether making some of the payment to the young person in question can improve outcomes.

Lundberg, Pollak, and Wales (1997) provide evidence that when transfers were made to women in a British transfer program, a larger fraction of household expenditures were made to purchase children's clothing. The evaluation of another British pilot program (Education Maintenance Allowance) found that impact on enrolment doubled when the payment was made to the young person (Ashworth et. al. 2002). Two programs, in Bangladesh and Colombia, make transfers to a Bank account in the student's name, which can be accessed by the student later, but no evaluation of this aspect of these programs is available. It seems plausible that paying at least a portion of the transfers to young people – either directly or into a savings account – may be worth considering.

Pilot programs in Burkina Faso, Morocco, and Yemen all have randomized treatment arms for making transfers to women/mothers vs. men/fathers. To our knowledge, no other study than the one proposed here explicitly evaluates the effect of making some of the payment to the young person (student) vs. the parents/guardians.

2.2.4. 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

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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.[4] 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).

2.3. Bank-Wide Consultations

The specific focus of this proposal has been informed by discussions with a diverse set of Bank staff – both from the research department and from various sectors. Specifically, the proposal benefited from discussions with the authors of the forthcoming PRR on CCTs, as well as sector experts in education, social protection, gender, and HIV/AIDS. In addition, the project was presented to the Malawi country team in Lilongwe, who welcomed the study with enthusiasm. The fact that the project design is suitable to draw useful policy implications for the future design of similar projects in Malawi is in large part a result of these discussions. The Malawi country team is assisting the Government of Malawi on the formulation of their SP Policy and Program. Given that the Malawi Social Action Funds (MASAF) has recently approved a project with a CCT component, the country team regards the study as highly relevant to their work.

The details of the proposed study have also been shared with government officials in Malawi – both at the national and at the local level. Presentations were made in Lilongwe, which were attended by representatives of the Ministry of Education, MASAF, Office of the President, and DfID. The project has also received the support of officials in the study district of Zomba and continues to enjoy assistance from the District Education Manager, District Health Office, and the Director of Planning and Development.

Comments from this diverse set of colleagues from inside and outside the Bank have been incorporated into the design of the intervention and its proposed impact evaluation.

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

Additional discussions with collaborators at the Department of Economics at the Chancellor College (University of Malawi) located in Zomba, and comments received from a group of evaluation experts at two separate workshops organized by the Global Development Network further helped bring the proposal to its current form. The CCT intervention and the first two rounds of data collection were made possible by financial support received from the Global Development Network (GDN), as well as trust funds inside the Bank, including the Knowledge for Change Program, WDR 2007, Gender Action Plan, and Spanish Impact Evaluation Funds.

Letters of support for this study have been from the authors of the forthcoming PRR as well as from the WB Malawi country team and will be sent separately to Jean-Jacques Dethier (Research Manager, DECRS).

2.4. Relevance

As mentioned in the preceding sections, as of 2007, twenty-four developing countries had some type of a CCT program in place, with many others planning or piloting one. An increasing share of countries that are planning to implement cash transfer programs are to be found in sub-Saharan Africa. Despite many evaluations on the impact of CCT programs, there are areas of critical importance about which we still know very little. The proposed study aims to begin filling some of the remaining gaps in our knowledge, especially for low-income countries (in sub-Saharan Africa and elsewhere).

First, by randomizing whether the transfers are conditional, the size of transfer, and the recipient of the transfer within the household, the project will be able to produce new evidence that is highly relevant for policy-makers in the developing world. While the study team is well aware that the external validity of the results for other countries in SSA or elsewhere may be limited for certain findings, many other findings are likely to apply in a variety of different countries and/or settings. The elasticity of school attendance to the size of the transfer is likely to be different in Tanzania than in Malawi, but a more general finding of diminishing marginal

returns may apply, especially if combined with similar findings in other studies for other countries. Evidence on whether unconditional transfers are less effective than conditional ones or whether giving part of the transfer to a secondary school student is effective in improving outcomes are likely to be highly relevant for practitioners in a wide-range of countries and settings while they set out to design their own programs.

Second, while we do have a good deal of evidence on the impact of CCTs in middle-income countries in Latin America, we know much less on how they might work in lower-income settings. For this reason, the proposed study in Malawi is likely to be relevant for not only other countries in the region, but for low-income settings in general.

Finally, even in lower income settings, our knowledge on the impact of CCTs is limited to education outcomes. Existing studies do not provide evidence on the possible impacts of schooling CCTs on demographic and health changes among the young beneficiaries, which include sexual behavior, HIV risk, age at first marriage, and fertility. Given that HIV prevention is a priority for many countries in SSA, where the risk of HIV infection is high among school-aged girls and young women, the results from the study are likely to be highly relevant for policy-makers.

To effectively serve the audiences in developing countries, the study team will produce policy notes, in addition to publishing papers in peer-reviewed journals. These notes are intended to make study findings more accessible to policy audiences in a timely manner. For example, our study will provide unique experimental evidence on how CCT programs can be best designed to pull school-aged girls back into the educational system within the African context. Given that we randomize the conditionality and vary the size of the transfer to the girl and to the household, our study will provide an experimentally estimated 'demand curve' for CCTs (and CTs), thus allowing us to estimate the relationship between transfer size and schooling attendance. This kind of evidence is potentially crucial for African governments considering creating schooling-based CCT programs in the near future.

3. Analytic Design

3.1. *Evaluation design, intervention details, and specific research questions*

3.1.1. Evaluation design (summary)

The study 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.[5]

Out of the 3,821 girls sampled in 176 EAs, 1,230 girls in 88 treatment EAs were sampled to receive cash transfers.[6] 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.[7] The offer consisted of a household transfer and a transfer directly to the girl, as well as full payment of school fees for girls in secondary school.[8] The household amount was randomly varied across EAs from $4/month to $10/month, with all recipients in a given EA receiving the

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[5] 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.

[6] 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).
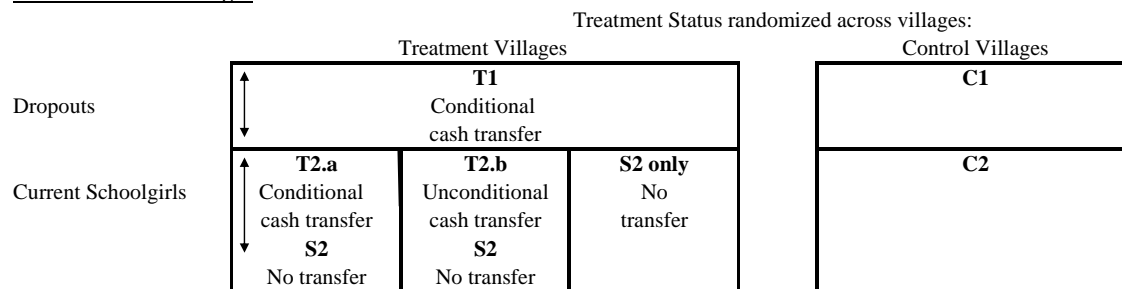
[7] Note that about 10% of girls did not attend these offer meetings, most of whom then received their offer letters at the first cash transfer point in February and entered the program thereafter.

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

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:

Treatment Status randomized across villages:

	Treatment Villages				Control Villages
Dropouts	**T1** Conditional cash transfer				**C1**
Current Schoolgirls	**T2.a** Conditional cash transfer **S2** No transfer	**T2.b** Unconditional cash transfer **S2** No transfer	**S2 only** No transfer		**C2**

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.[9] 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 school girls receives no transfers.[10] 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

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[9] 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.
[10] We randomly vary the percentage of school girls receiving transfers between 0%, 33%, 66% and 100% across treatment EAs.

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.

3.1.2. Intervention design

In Malawi, while the gender gap in enrolment is closing, especially at the primary level, there are still significant gaps in enrolment and attainment between boys and girls. While dropouts for boys and girls are roughly even up until Standard 6, they start diverging towards the end of primary school.[11] In 2004, only 43% of students in Standard 8 were female and among those who passed the PSLC, the ratio was even lower at 39% (Malawi Public Expenditure Review, 2006). The median age at first marriage in Malawi is 18.0 for women aged 20-39 (Malawi Integrated Household Survey 2004/05), and more than 20% of dropouts cite pregnancy as the reason for dropping out of school.[12] Hence, like programs in other countries aiming to reduce inefficient under-investment in girls' education (such as those in Bangladesh, Cambodia, Pakistan, and Yemen); the cash transfer intervention being evaluated here is targeted to females.

The intervention targets two groups of girls: current schoolgirls and those who have recently dropped out of school.[13] We chose to target these two groups separately to ensure that we had as many recent female school dropouts in our sample as possible. This sampling

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[11] In Malawi, primary school takes eight years (Standard 1-8), and is followed by four years of secondary school (Form 1-4). Students take the Primary School Leaving Certificate examination at the end of Standard 8 (PSLC), and sit two examinations in secondary school: Junior Certificate Examination (JCE) at From 2 and Malawi School Certification Examination (MSCE) at Form 4.

[12] Authors' own calculation using baseline data for the study sample in Zomba. In terms of reasons for dropping out of school, "pregnancy" is only second to the "lack of funds for school fees and uniforms".

[13] The determination of "current schoolgirl" vs. "dropout" is based on the schooling status of the sampled girl or young woman at baseline. Anyone who was still in school at the end of the 2007 school year was considered a "current schoolgirl".

procedure is important because treating current schoolgirls involves giving transfers to many girls who would have stayed in school even without the transfer. Hence, sampling current schoolgirls alone would be a very expensive way of trying to identify the effect of CCTs on schooling and other outcomes. Treating dropouts, on the other hand, allows us to focus on a subpopulation whose schooling rates we expect to be much more sensitive to transfers as the likelihood of returning to school is likely to be very low among this group in the absence of the program.

Because one of the research questions the study aims to answer is the possible impact of CCT programs on HIV risk among young people, we further restricted our sample to focus on an age group where the majority of girls are likely to be sexually active. We therefore restricted the eligibility for the CCT program to never married girls, aged 13-22. Our eligibility requirement differed slightly between the school girls and dropouts – school girls had to be eligible to attend grades Standard 7 through Form 4 in 2008, while dropouts simply had to be eligible to go back to any grade in primary or secondary school.[14] Despite this difference in the eligibility criteria, the majority of the dropouts were eligible to return to the same grades as the schoolgirls.

Within the target population, there are important differences between those that are eligible to attend primary school and those that are eligible to attend secondary school. In most other areas of the world where CCTs have been implemented (Mexico, Brazil, etc.), school enrolment is free. Given that attending primary schools is free in Malawi, the administration of our CCT program at the primary school level will look more like a standard CCT program, except that we have an additional randomized transfer directly to the schoolgirl herself (see below).

In Malawian secondary schools, on the other hand, fees impose a major financial burden on even middle-class families, and access to secondary schools is strictly regulated by national entrance exams. In secondary schools therefore, we implemented the conditional transfer by directly paying the school fees of treated subjects (in addition to making separate cash transfers to

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[14] The main reason for choosing eligibility based on these grades is the fact that a two-year CCT intervention will allow each beneficiary the chance to complete the PSLC, JCE, or the MSCE. All recent never-married dropouts were sampled for the study, regardless of which grade they could return to, as never married dropouts are a much smaller group and we wanted to sample as many of them as possible.

the household and the schoolgirl as explained below). This process greatly simplified the payment of the transfers for our team, and has the added benefit of making the conditionality as transparent as possible: only by paying fees directly to the schools can we guarantee that the transfers are not diverted into other forms of consumption by parents. This approach also most closely mimics a real government program under which school fees for some or all secondary school girls would be abolished. In order to protect the integrity of the attendance data against the conflict of interest when schools are asked to report on attendance of students for whom they receive transfers, we have instituted random spot checks of attendance by program staff through the course of the intervention. In this way, we achieve the removal of financial barriers towards school attendance with a simple and easily administered mechanism.

The intervention is designed to be able to examine important questions about the "design" features of CCT programs. We introduced random variation in (a) whether the cash transfer is conditional on school attendance or not, (b) the size of the transfer, and (c) the portion of the payment that is transferred directly to the girl instead of her parents/guardians. This unique combination of design elements allows us to unpack the main research questions of interest with experimental identification.

As mentioned in the previous section, in a randomly selected subset of treatment EAs, the cash transfer offers to schoolgirls were made *unconditionally* with no schooling (or any other) requirement.[15] Since these cash transfer offers are entirely unconditional, they will allow us to examine the 'pure' elasticity of various outcomes to the transfer amount – free of the explicit conditionality attached to the transfer made to the CCT recipients. The comparison between the conditional (CCT) and unconditional (CT) groups gives the pure impact of the conditionality, because all other aspects of the program design (including the average transfer size) are held identical.

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[15] However, as mentioned in the previous subsection, dropouts in all treatment villages were offered only *conditional* transfers.

As we alluded to before, the elasticity of relevant outcomes to the transfer amount is a key parameter in designing efficient CCT programs. Typically, such programs have a standardized design that is not subject to variation, meaning that researchers gain little information over how school enrolment (or any other outcome of interest) varies with transfer size. To estimate the 'optimal' benefit levels, transfer amounts to the parents/guardians were randomized at the village level to vary between $4 and $10 per month. The study team determined this as a reasonable range after examining the size of the transfer as a share of pre-transfer monthly household expenditures for CCT programs in other countries, calculating the direct costs of schooling in Malawi (uniforms, books, secondary school fees, transportation, etc.), and conducting focus group interviews with eligible girls and their parents. This random variation in cash transfer amounts will allow us to estimate the yield curve of schooling achieved by transfer size over this range, and hence identify the 'optimal' transfer size in this context.

A third question relating to the payment of transfers is whether there is heterogeneity in the impact of a CCT program if the transfers are made to different people in the household. While previous research suggests that making the transfers to mothers (or female guardians in general) are more effective (at least for children's outcomes) than giving them to the father (or a male guardian), there is no evidence to date on whether outcomes improve if some of the transfer is made directly to the student.[16] In order to answer this question, we divide transfers into three clearly distinguished types. The first part of the transfer is made directly to secondary schools to cover school fees. The second part is a monthly household level cash transfer that is given to the parents (or the guardian) of the girl.[17] The third is a cash transfer given directly to the girls themselves once a month at a pre-determined transfer location. These last transfers are randomly varied at the individual level between US$1 and US$5 per month, and given directly to the girls.

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[16] Except Ashworth et al. 2002 that examines this question for a program in the UK.

[17] The young beneficiary's family is free to designate one person to receive the household portion of the transfer. In addition, the household designates one proxy. All three individuals (student, parent/guardian, and the proxy) are identified at the offer stage and photographed. From that point onwards, no other person can pick up any part of the transfer for that household. In practice, most families designated the mother (or a female guardian) to pick up the household portion of the transfer even though this was neither required nor encouraged.

This individual transfer not only provides substantially more statistical power for the identification of treatment effects (because it was randomized at the individual level as opposed to the village level randomization for the parents), but also allows us to ask whether a dollar transferred is more or less efficacious if given to the girl rather than her parents/guardians.[18]

A final component of the design that will allow us to examine a rich set of interesting questions is the fact that the saturation of treatment among schoolgirls was randomized at the village level. Using this design, we can compare untreated girls in treatment communities with those in control EAs in order to estimate spillover effects experimentally. Given the paucity of data on general- versus partial-equilibrium effects of CCT programs, as well as the lack of variation in most CCT programs in the treatment intensity of the beneficiary groups, this feature of the design promises to be of real use in understanding how CCT programs affect the outcomes of nearby non-beneficiaries.[19] The several forms of spillover effects we can pursue are described in more detail in the next subsection.

Our intervention and evaluation design is well-suited to assess short- as well as long-term impacts. Several of the factors limiting long-term evaluations, for example those discussed in King and Behrman (2008) are not found in our study. For example, there are no other CCT programs in the study district at present (nor are they likely to be present in the near future), so we are unlikely to see 'displacement' effects (in the sense of funds being channeled to the controls over time). Our design is structured to allow us to capture spillover and peer effects, hence we do not anticipate that these will undermine our ability to estimate long-term treatment effects. Furthermore, we have access to excellent program data as the program is entirely under our direction and control. Finally, while intermediate outcomes such as schooling and attainment

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[18] To give an example, for a household receiving a total of $9/month (excluding school fees), the amount transferred to the parents could be as little as $4/month (with the transfer to the girl at $5/month) and as high as $8/month (with the transfer to the girl at only $1/month). By comparing outcomes among families for whom the total transfer is the same but the share transferred to the parents is different, we can get experimental identification on the effectiveness of the additional dollar that is transferred to the parents (or the girl).

[19] Angelucci and De Georgi (forthcoming) discuss how cash transfers to eligible households under PROGRESA indirectly affect the consumption of *ineligible* households living in the same villages. In our case, we will be able to document spillover effects on *eligible* non-beneficiaries in the same villages.

can be observed in the short-term, the real interest of this research project is the longer-term impacts of education on welfare, labor market outcomes, marriage, and HIV risk (and even early childhood outcomes for the children of program beneficiaries). This cohort of girls and young women, exhibiting random variation in teenage income and schooling, will continue to be a natural laboratory as they age. The proposed two-year surveys would provide us with a rich set of outcomes as of the end of the intervention.

There is an important "pioneering" effect inherent to beginning a new cash transfer program: cohorts that had recently dropped out of school are induced to re-enroll by the program. Moving forward, dropouts will be choosing to leave the program (as opposed to dropping out of school in the absence of the program). We stratified the research design across baseline schooling status in order to be able to estimate clean impacts in both groups. Given the share of dropouts who re-enroll in the control group, we can simulate the steady-state average treatment effect (ATE) of conditional transfers.

3.1.3. Specific Research Questions

Given the study design described above, we can estimate a wide range of treatment effects, many of which are unique to this study. We now proceed to describe the identification of some of these possible impacts in more detail.

(1). Impact of *Conditional* Cash Transfers:

Direct comparison of the experimental groups T1 and C1 gives the impact of the average transfer for dropouts. Comparison of T2.a and C2 gives the impact for girls who were in school at baseline. In this case, the impact of the average transfer is defined as the impact of the average total cash payment to the household, which includes the transfer to the parents/guardians and the student (plus the school fees for secondary school students). Since we do not expect 100% compliance with the conditionality and we cannot drop non-compliers without introducing bias,

we will estimate the Intention to Treat Effect (ITE) of the program, i.e. the impact of *offering* the CCT, as opposed to taking it up and complying with it. This estimate will be a direct mechanical function of the take-up and compliance rates.

(2). Impact of *Unconditional* Transfers:

Direct comparison of the experimental groups T2.b and C2 gives the impact of the average unconditional transfer on schoolgirls. Because there is no 'compliance' in this treatment, this comparison gives the Treatment Effect on the Treated (TET) of the average total amount transferred to the household, which is the sum of the 'parental' and 'individual' transfers.

(3). Impact of the *Conditionality*:

We isolate the impact of the conditionality itself by making the same average household and personal transfers to schoolgirls in the two treatment arms T2.a and T2.b[20]. Thus, the only difference between groups T2.a and T2.b is the fact that in one case the payments are conditional on school attendance and in the other they are not. The impact of the conditionality is therefore given by the simple difference T2.a - T2.b.

(4). Effect of *Conditional* Transfer Size on Schooling Outcomes:

The combined transfer to the household (girl-level transfer plus household-level transfer) varies randomly between $5 and $15 per month.[21] This allows us to use very simple, non-parametric techniques to estimate the curve which describes how school enrolment, attendance, and attainment improve with transfer size. For example, we can plot local averages of enrolment and attendance across the distribution of transfer sizes for girls in T1 and/or T2.a to illustrate the extent to which increased transfers generate more schooling. Similarly, we can run treatment

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[20] The average school fee payment made to the schools in the CCT group is given to the households in the CT group on a monthly basis, so that the means are truly identical.

[21] This amount is per program participant. Some households have more than one girl in the program, as the randomization into the program was conducted at the girl level.

regressions using linear and quadratic terms for transfer size instead of a binary treatment term for the Conditional girls versus the controls. If it is found that schooling improves very rapidly over some transfer interval and then ceases to increase thereafter, this would provide invaluable information to the Malawian Ministry of Education as to the optimal size of any potential future transfer.

(5). Elasticity of Outcomes with Respect to Transfer Size:

A similar exercise to (4) can be conducted in the *unconditional* group T2.b. For this group, the only difference from the controls (C2) is the presence of a random income shock. This permits experimental variation of a wide variety of otherwise endogenous behavioral outcome's, such as consumption patterns, borrowing and savings, schooling, as well as marriage, pregnancy, and sexual behavior for the girl. The continuous and random variation in the size of the transfer gives us a very rich way of understanding these important behavioral questions. Specifically, we can use non-parametric methods to construct an average relationship between changes in an outcome ΔS_{iv} where 'i' is the core respondent and 'v' is the village/EA that the core respondent resides in and the size of the transfer τ_{iv} in the unconditional transfer group. A parametric regression specification for estimating the average elasticity in the sample would be,

$$(i)\log(\Delta S_{iv}) = \alpha_0 + \beta X_{iv} + \varepsilon_2 \log(\tau_{iv}) + \upsilon_{iv} \quad \forall v, i \in T2.b$$

where X_{iv} are a series of individual and EA level characteristics and ε_2 provides the elasticity among the group receiving unconditional transfers (T2.b).

Because we will be conducting a widespread voluntary counseling and testing campaign in concert with the follow-up waves of the survey, we will have an unusual ability to observe the impact of a CCT program for schooling on the risk of HIV infection (as well as other STIs). This is of direct policy interest particularly in sub-Saharan Africa, but the randomized transfer amounts also present a unique ability to understand the ways in which income and sexual behavior are related to each other in this context. A large (and influential) literature implicates women's

poverty and 'transactional sex' as one of the causes of the spread of HIV/AIDS in sub-Saharan Africa. If it is true that money and gifts from often older boyfriends with higher HIV risk play a role in the frequency of unsafe sex, then increasing the girls' economic self-sufficiency with the cash transfers may lead to a profile of lower risk behaviors (apart from the potential effect of increased schooling on sexual behavior).

A similar analysis of a host of outcomes as a function of transfer size can also be conducted among T1 and T2.a, i.e. the groups receiving *conditional* transfers. For these groups, variation in payment is attached to continued attendance of school, and so the relationship between ΔS_{iv} and τ_{iv} in this group should be driven both by the elasticity estimated above and by the higher-powered incentives as a result of the fact that the transfers are *conditional* on satisfactory school attendance. We therefore hypothesize that the slope of the elasticity ε_1 or

$\left(\dfrac{dS_i}{d\tau_i} * \dfrac{\tau_i}{S_i} \right)$, calculated through the counterpart to (i) but estimated on T1 and/or T2.a will be

larger than ε_2.

(6). Share of Income Retained by Girls in the Study Group (Intra-household Allocation).

The degree to which schoolgirls are able to keep control over money given directly to them is a variant of the household income pooling/bargaining question, on which a large literature exists. Parents (or guardians) of the girls are aware of the size of the 'personal' transfers made directly to the girls, and hence either altruism on the part of the girls or dictatorial powers on the part of the household head may result in some or all of the transfer being utilized by or for other household members. The standard approach to such questions, as in Thomas (1990), is to try to find 'assigned goods', such as hair braids or girls' clothing items, which are consumed solely by the recipient of the transfer. Usually, the identifying assumption is that certain sources of income are exogenous and unanticipated, and the share of consumption increase for the assigned goods in that for total household consumption allows the researcher to calculate the share of the transfer

which was controlled by a specific individual. The 'shocks' used to identify these exercises are often of questionable exogeneity (McElroy, 1990).

Because the initial income shock in this intervention is randomly assigned and unanticipated, we have the ideal empirical structure for testing this question using a consumption module specifically designed to capture the individual consumption of the program participant. The null hypothesis of perfect intra-household income pooling implies that the composition of consumption will be invariant to the split of the transfer between parents and children. If we reject pooling, by studying the increase in the consumption of assigned goods relative to overall household consumption, we can infer the share of the transfer that has remained in the girls' hands. While this assigned share is endogenous, we can instrument for the amount retained by the girl using the total (randomized) transfer to the household, and thereby gain clean identification on how both the 'parental' and 'individual' transfers alter behavior.

(7). Relationship between Transfers in Girls' Pockets and Impacts:

We can use the 'assigned goods' methodology above to estimate the value of the transfer a girl directly retains herself. Through a matching exercise we can calculate individual-level impacts of the treatment. While both of these quantities are potentially endogenous to household characteristics, we are interested in understanding how these two quantities are related. We will not be able to determine, for example, that a girl experienced higher impacts *because* she retained a higher share of the transfer, but we are still interested in knowing whether this correlation exists.

(8). Spillovers within the village or classroom:

Along with estimation of the treatment effects detailed in (1)-(7), the research design allows for the estimation of a variety of spillover effects. Direct comparison of S2 to C2 gives us the average joint spillover effect on untreated schoolgirls (at the time of baseline) when dropouts and other schoolgirls in their communities are given transfers. These spillovers may include

adverse impacts on educational quality or positive indirect economic effects stemming from the transfers into their communities.

The potential negative educational spillovers of CCT programs deserve some attention. These effects are via the entrance of a wave of new classmates on girls who would have attended school anyway. References to the deterioration of the quality of primary education in Africa as a result of universal primary education are legion (e.g. Appleton, 2001 or Al-Samarrai, 2003), and this experiment gives us a controlled setting in which to quantify any such effects. Presumably, this question will be important to African policymakers as they consider how best to expand access to secondary schooling across the continent.

The unit at which we expect education spillovers to be most concentrated is at the classroom level. The impact of having additional students placed into your school is likely to be muted unless they are placed into your classroom and compete directly for resources and teacher attention. In secondary schools, we can calculate this classroom-level spillover effect in a very concrete way. Taking the classroom as the unit of analysis, we can take advantage of the fact that our experiment creates random variation in class size. Our outcome of interest would be a schooling outcome, such as secondary school JCE scores (exam taken upon completion of Form 2) among untreated students, and the treatment variable would be the percentage of treatment girls in each classroom.[22]

(9). Spillovers within Friend Networks:

As part of the baseline survey, we conducted a detailed network analysis, asking standard social network questions about whom each individual spends free time with, to whom they go to for advice, etc. This information allows us to identify the social network of each individual in S2 at the beginning of the experiment. The experiment then generates random variation in the share of individuals in each girl's social network who are beneficiaries of the program, and therefore

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[22] If non-compliance rates are found to be high, or if in the end many of our 'controls' in C1 end up attending secondary school despite the fact that we predicted they would not, we can run the educational spillovers regression using the treatment as an instrument for attendance.

gives us a measure of the intensity of the spillover effect that we expect to observe. The intensity of the spillover comes both from the share of a girl's social network that newly attends school (a'_{iv}), as well as the average transfer made to girls within her social network (τ'_{iv}). The network-based counterpart to (1) that allows us to estimate spillovers at the individual level, then, is

$$(ii)\, \log(\Delta S_{iv}) = \alpha_0 + \beta X_{iv} + \varepsilon_a \log(a'_{iv}) + \varepsilon_s \log(\tau'_{iv}) + \upsilon_{iv} \quad \forall v, i \in S2$$

This regression allows us to estimate an important and fascinating set of relationships: how do the behaviors and outcomes of young women respond to exogenous changes in school attendance and income of others in their peer group? Much of the interest in this regression lies in the complexity of otherwise estimating peer effects; the randomness in the fraction of individuals in a girl's peer group who are treated allows us to overcome Manski's reflection problem (Manski, 1993).

(10). Net benefit comparison for conditional and unconditional transfers:

The intervention, which is being administered under the guidance and control of the study team, is too small to benefit from economies of scale. In addition, as the program is operating outside of the formal government educational bureaucracy, it fails to capitalize on some efficiencies that the government could achieve in actions like monitoring attendance. We will therefore use estimates of the cost of making transfers and monitoring compliance based on estimates from true government-run programs in similar contexts rather than using our own costs, so as to achieve as realistic as possible a cost-benefit scenario for actual implementation. We can add that our own costs can be used as an upper bound on the additional cost of monitoring and enforcing the conditionality.

First, let us introduce some basic notation: let c^m be the marginal cost of monitoring one additional girl's attendance, and c^p be the marginal cost of administering the program to each girl. The schooling treatment effect is a function of the transfer size, which we write as s_c^t for

conditional girls and s_u^t for unconditional girls. If b is the total social + private benefit from an additional year of schooling, we can write the aggregate cost-benefit question as $c^m + c^p + t >< s_c^t b$ for each transfer size for conditional girls, and $c^p + t >< s_u^t b$ for unconditional girls. We can identify the most cost-effective way of increasing schooling rates through several forms of comparison:

a. *Optimal Transfer Size.*

Thinking of the response to the transfers as a 'schooling production function', we would imagine that $\dfrac{ds^t}{dt} > 0$ and $\dfrac{d^2 s^t}{dt^2} < 0$, meaning that the response is increasing and concave. We have five randomized individual transfer amounts and four randomized household transfer amounts; these can either be considered separately, or combined into a single household transfer amount where we observe 20 points on this production function. The marginal total benefit from increasing conditional (unconditional) transfers can be written as $s_c' b$ ($s_u' b$), and the marginal cost of doing so is 1. Assuming that $\left. \dfrac{ds^t}{dt} \right|_{t=0} > \dfrac{1}{b}$, i.e. that increasing transfers from zero is worthwhile, then the optimal conditional (unconditional) transfer is implicitly defined by

$$\frac{ds_c^t}{dt_c^*} = \frac{1}{b} \text{ (or } \frac{ds_u^t}{dt_u^*} = \frac{1}{b} \text{)}.$$

b. *Transfer size versus conditionality:*

Assume that both increasing transfer size and the imposition of conditionality are effective in increasing attendance. The cost of monitoring attendance is invariant to transfer size, and so a natural question then becomes: is it more effective to drive attendance through transfers or through monitoring? Let us take the optimal conditional transfer from above, t_c^*, which induces treatment effect $s_c^{t^*}$ at cost $c^m + c^p + t_c^*$. We can now use our impact estimates to see

whether there exists an unconditional transfer $\tilde{t}_u$ such that $\tilde{s}_u^t > s_c^{t*}$ and $\tilde{t}_u < t_c^* + c^m$ (that is to say, money which would otherwise be spent monitoring is more effective when provided directly to households). If such a transfer exists, then it is more cost-effective to drive schooling through unmonitored, unconditional transfers than it is through the optimal conditional cash transfer scheme. Because we do not have a treatment arm which is 'monitoring only', this analysis compares the joint effect of monitoring & transfers to the effect of transfers alone.

c. *Efficient targeting.*

First, because distribution of transfers to the girl and her household head are equally costly, on policy grounds we'd prefer whichever recipient generates the best outcomes. Further, with interaction effects or partitioned samples we can estimate treatment effects for subsets of the study sample (for example dropouts who have been out of school more than one year or schoolgirls above some baseline income level). We can see the quantity $\dfrac{c^m + c^p + t}{b}$ as a threshold impact which must be achieved in order to make administration of a CCT program cost-effective. Subgroups (based on easily observed characteristics) whose treatment effect exceeds this threshold should be targeted by future government CCT programs.

3.2. *Study Setting and external validity*

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).[23]

Malawi also has the eighth-highest HIV prevalence in the world with 14 percent of the adult

population infected (UNAIDS, 2006).[24] 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.

Since this project entails several in-depth data collection components that require well-

managed field staff, in addition to the complex logistics of running a CCT program, the research

team opted to focus the project within one district. This focus both reduces project costs (lower

fixed costs of office infrastructure and transport) and increases data quality through more careful

supervision. Within Malawi, Zomba district in the Southern region was chosen as the site for this

study for several reasons. First, it has a large enough population within a small enough

geographic area rendering field work logistics easier and keeping transport costs lower. Zomba is

a highly populated district, but distances from the district capital (Zomba Town) are relatively

small. Second, characteristic of Southern Malawi, Zomba 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

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[23] 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.

[24] 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).

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. We have access to both nationally representative household survey data, as well as data from another study of our own (with close to identical survey instruments) in Salima (Central Malawi). These data can be used to compare Zomba to other parts of Malawi for characteristics that are pertinent to the design of a CCT program to inform policy, should anyone contemplate expanding this program or to implement a similar one nationally.

Two of the principal investigators in this study (especially Chirwa, but also Özler) have previous field research experience in Malawi and the study benefits both from their expertise, as well as the survey instruments and field resources developed under another ongoing project in Malawi, thus reducing costs and facilitating field work for this study.

3.3. *Sampling and Power Calculations*

The CCT program entailed sampling 3,821 young women from 176 EAs in Zomba district in Malawi. Baseline data were collected for this sample in the fall of 2007 and the first round of follow-up data collection started on October 6, 2008. We propose to continue following

these individuals in 2009, and hopefully in 2011, two years after the end of the intervention, to assess long-term impacts.

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), while the remaining 470 are rural (population areas, or PAs). Our stratified random sample of 176 EAs consists of 29 EAs in Zomba town, 8 trading centers in Zomba rural, 111 population areas within 16 kilometers of Zomba town, and 28 EAs more than 16 kilometers from Zomba town.

After selecting sample EAs, all households were listed in the 176 sample EAs using a short two-stage listing procedure. The first form, Form A, asked each household the following question: 'Do you have any never-married girls in this household who are between the ages of 13 and 22?' This form allowed the field teams to quickly identify households that had members that fit into our sampling frame, thus significantly reducing the costs of listing. If the answer received on Form A was a 'yes', then Form B was filled to list members of the household. For individuals in these households the following information was collected:

1. Name

2. Age

3. Marital status

4. Current schooling status

5. If currently in school, level attended 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* in our age range, the majority of whom have been out of school for three years or fewer, and

b.   Eligible *schoolgirls* in our age range, who reported being in

school at the time of listing.

These two groups comprised the basis of our sample frame. In each EA, we sampled all eligible dropouts and 75%-100% of all eligible school girls, where the percentage depended on the age of the school-girl.[25] In each EA, we sampled an average of 5.1 dropouts and 16.7 schoolgirls.

Out of these 3,821 young women, 1,230 girls in 88 EAs were sampled to receive the cash transfer intervention, receiving either *conditional* or *unconditional* cash transfers. In each of the 88 treatment EAs, sampled dropouts were always treated. The sample of treatment EAs was randomly divided into three groups based on how the sample of schoolgirls was treated: in 43 EAs (a randomly determined share of) schoolgirls received *conditional* transfers; in 30 EAs schoolgirls received *unconditional* transfers; and in the remaining 15 EAs they received *no* transfers.[26] A table describing sample is presented below:

	Treatment/Control (T/C)	Number of clusters	Number of core respondents
Baseline dropouts	C1	88	454
	T1 (conditional T)	88	435
Baseline schoolgirls	C2	88	1,500
	T2a (conditional T)	43	504
	T2b (unconditional T)	30	282
	S2* (untreated in T)	88	630
ALL	Control	88	1954
	Treatment	88	1221
	Total	176	3805

* There are S2 girls (for identifying potential spillover effects) in all treatment EAs with randomly varied percentages sampled. Please refer to the research design schematic on page 18 for the definitions of the groups and details on the research design.

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

[26] These randomly determined shares of schoolgirls that were treated were 33%, 66%, or 100%. The 15 EAs, where no schoolgirls received transfers could be considered a special case where the share was set to zero. In those EAs, the only individuals treated were dropouts.

The random, clustered sample of girls and young women in Zomba was chosen to enable the research team to identify treatment effects on the outcome variables of interest with reasonable confidence. As we demonstrate below, across the variables of interest, power calculations indicate that our sample size of 3,821 individuals (in 176 enumeration areas) will allow us to detect moderate treatment effects being significantly different than zero with confidence (90 percent) and considerable power (80 percent).[27] We present power calculations on three different variables, school enrolment, marital status, and HSV-2 (herpes simplex virus) prevalence. We start with the calculations for school enrolment.

As this is a cash transfer program conditional on schooling for one of the treatment arms, it is important that the study be powered to detect not only overall treatment effects on schooling, but also for each of the two treatment arms and for various sub-groups (baseline dropouts and schoolgirls). Table 1.a presents power calculations for detecting one-year treatment impacts of the program across the treatment arms. The figures for the "*observed* probability of enrolment in control", "range of mean enrolment in control EAs", and "*observed* probability of enrolment in treatment" come directly from our analysis of the one-year impact of the program using 50% of the follow-up data currently available to us.[28] "Minimum probability of detectable success in treatment" (in column 4) is the minimum enrolment rate that our power calculations tell us we can detect to be significantly different than control at follow-up. This means that, if a treatment effect can be detected, it has to be outside the range given by columns (2) and (4). Table 1.b then makes informed *projections* on two-year impacts (to be evaluated using data from the round of data collection requested to be funded under this proposal). As mentioned above, in all the calculations, we use alpha = 0.1 and power = 80%. We utilize the "Optimal Design (OD)"

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[27] This level of power is generally recognized by the research community to be sufficient (Raudenbush et al., 2004).

[28] It should be noted that the *observed* impacts may change when the analysis is conducted using the whole follow-up data instead of the 50% available currently. The change is likely to make the impact sizes larger as one treatment effect is lower mobility. As there are many more tracking cases among the control group than the treatment, we are more likely to find larger differences in schooling, marriage, and fertility when these data points are included, as moving away is highly correlated with dropping out of school, marriage, etc.

software, which allows us to take into account the fact that our intervention has a randomized, clustered design that is evaluating impacts for continuous or binary outcome variables.[29]

Table 1.a: Power calculations for "school enrolment" (*observed* one-year impact)

(1)	(2)	(3)	(4)	(5)
T/C	*Observed* probability of success in C	Plausible range in C	Minimum detectable probability of success in T	*Observed* probability of success in T
All	70.8	50-90	76.4	80.6
T1/C1	18.6	10-50	26.5	63.2
T2/C2	87.0	75-95	90.9	90.2
T2a/C2	87.0	75-95	91.4	90.3
T2b/C2	87.0	75-95	91.6	90.0
T2a/T2b	90.0*	80-95	95.1[+]	90.3[+]

Note: All figures are in percentages. Alpha = 0.1, power = 0.8. Figures in columns (2), (3), and (5) are based on the impact analysis conducted using 50% of follow-up data entered and cleaned so far.
* Probability of success in T2b (i.e. for *unconditional* transfer recipients)
+ Probability of success in T2a (i.e. for *conditional* transfer recipients)

Table 1.b: Power calculations for "school enrolment" (*projected* two-year impact)

(1)	(2)	(3)	(4)	(5)
T/C	*Projected* probability of success in C	Plausible range in C	Minimum detectable probability of success in T	*Projected* probability of success in T
All	55.0	35-75	60.7	72-79
T1/C1	10.0	5-35	16.5	50-60
T2/C2	70.0	55-85	75.7	80-85
T2a/C2	70.0	55-85	76.4	85-90
T2b/C2	70.0	55-85	76.7	80-90
T2a/T2b	80.0*	70-90	87.2[+]	85-90[+]

Note: All figures are in percentages. Alpha = 0.1, power = 0.8. Figures in columns (2), (3), and (5) are projections of two-year impacts based on the one-year impact analysis conducted using 50% of follow-up data entered and cleaned so far.
* Probability of success in T2b (i.e. for *unconditional* transfer recipients)
+ Probability of success in T2a (i.e. for *conditional* transfer recipients)

Table 1.a. shows that our study is powered to detect meaningful changes in enrolment even after just one year of the program. Relative to the impacts we are observing with the sub-sample of the data currently available to us, the power of the study is quite high for the combined effect of the two treatment arms and for baseline dropouts, and is sufficient to detect the impact from each of the two treatment arms separately for schoolgirls. Table 1.b shows that, as beneficiaries and controls continue to diverge in terms of their schooling status and attainment, the study is likely to detect even larger and statistically significant impacts after two years.

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[29] "Optimal Design for Longitudinal and Multilevel Research", Spybrook et al (2007).

Table 2 shows that our study has enough power to detect quite small changes in marriage rates (and much larger changes among baseline dropouts) after one year. The impact sizes are again expected to be higher (bottom panel in Table 2) after two years, well above the minimum detectable probability of success according to our power calculations.

Table 2: Power calculations for "never-married"

(1)	(2)	(3)	(4)	(5)
Observed one-year impact based using 50% of follow-up data available				
T/C	*Observed* probability of success in C	Plausible range in C	Minimum detectable probability of success in T	*Observed* probability of success in T
All	89.0	80-95	91.9	90.7
T1/C1	71.5	50-85	79.4	83.3
T2/C2	94.4	87.5-97.5	96.8	94.5
Projected two-year impact				
T/C	*Projected* probability of success in C	Plausible range in C	Minimum detectable probability of success in T	*Projected* probability of success in T
All	70	55-85	74.7	80-86
T1/C1	40	30-55	48.9	65-75
T2/C2	80	70-90	84.8	85-90

Note: All figures are in percentages. The entire sample was never-married at baseline. Alpha = 0.1, power = 0.8. Figures in columns (2), (3), and (5) for the **top panel** are based on the impact analysis conducted using 50% of follow-up data entered and cleaned so far. Figures in the same columns in the **bottom panel** are projections of two-year impacts based on the one-year impact analysis reported in the top panel.

Finally, Table 3 shows the estimated power of the study to detect differences in the prevalence of HSV-2 between the treatment and control groups after two years. As we do not have data of our own to base these calculations on, we have scoured the existing literature to identify reasonable estimates of HSV-2 prevalence (for a demographic group similar to our study sample) in Malawi and elsewhere in Eastern and Southern Africa. We have examined estimates for women aged 15-19 and 20-24 in Northern Malawi (Glynn et al , 2008), from the 4-cities study (Weiss et al, 2001), from Mwanza, Tanzania (Obasi et al, 1998; Ross et al, 2007), and a recent pilot by the "Poverty Action Lab" in Western Kenya on a similarly aged population of schoolgirls. From these studies, we have come up with an estimated prevalence of HSV-2 among the control group in our study population after two years from baseline data collection. We were conservative in our estimates and chose to use the lowest levels reported in the literature for

samples similar to ours (for example, Ross et al, 2007 – re: Mema Kwa Vijana in Tanzania – reports a 21% prevalence of HSV-2 among a sample of adolescents aged 17-20) so as not to overstate the power of our study.

Table 3: Power calculations for HSV-2

(1)	(2)	(3)	(4)	(5)
T/C	*Projected* probability of success in C	Plausible range in C	Minimum detectable probability of success in T	*Projected* probability of success in T
Projected two-year impact				
All	20	15-30	16.8	12-18
T1/C1	32	20-45	24.2	19-24
T2/C2	16	10-25	12.2	12-16

Note: All figures are in percentages. Alpha = 0.1, power = 0.8. As we do not yet have data on HSV-2 in our own study, the estimates are based on other studies of HSV-2 prevalence for young women in Eastern and Southern Africa. To make the minimum detectable probability estimates conservative, we have chosen to use HSV prevalence figures that are lower than the average reported elsewhere. The power of our study will be higher if the prevalence of HSV-2 among our study population is higher.

As can be seen in Table 3, assuming an overall prevalence rate of 20% among the control group two years after baseline data collection, our study has enough power to detect 16%-25% (3.2-7.8 percentage points) differences in HSV-2 prevalence between treatment and control groups. Given that all of the studies we reviewed show large and significant increases in HSV-2 prevalence for married women and by the number of lifetime partners (for both of which we are already seeing evidence of large declines among program beneficiaries after only one year, especially among baseline dropouts), these differences seem well within the expected range of impacts for the program.

Power calculations with respect to the transfer sizes are complicated by two factors. The first is that the explanatory variable takes multiple integer values, and so cannot be simply captured in a binary treatment/control power framework. Secondly, the portion of the transfer which goes to individuals was randomized at the individual level through a lottery, meaning that the treatment/control comparison contains variation at the individual (rather than the cluster or EA) level.

In order to address these issues in as straightforward a manner as possible, we ran preliminary impact regressions again using 50% of our follow-up data that we have in hand now.[30] Using school enrolment as the dependent variable, we find that the "minimum detectable effect" of a US$1.00 increase in individual (household) transfer size on the probability of attending school is approximately 2.4% (1.5%) when the entire treatment group is compared with the control. These figures vary from 1.6% - 4% (1%-2.8%) across treatment groups for individual (household) transfer size. As these estimates are based only on half of our study sample, we expect the "minimum detectable impact" to become smaller as the precision of the estimates are likely to improve when the entire sample is used for the impact regressions performed here.

3.4. *Annual Survey Instruments*

The annual Household Survey consists of an LSMS-like, multi-topic questionnaire that is administered to the households in which the selected core sample respondents reside. Although it is described as a household questionnaire, the primary goal of this instrument is to collect detailed information from the individual respondents in the target population of the study.

The survey consists of two parts: Part I is administered to the head of the household and collects information on the household roster, dwelling characteristics, assets and durables, consumption (food and non-food), access to safety nets, and shocks (economic, health, and otherwise) – all at the household level. Part II is administered to the core respondent, i.e. the sampled girl from our target population. 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.).

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[30] We use alpha = 0.1, so figures for MDE are calculated as 1.65 times the standard error of the regression coefficient on transfer size. Regression includes individual fixed effects, and dummies for follow-up round and treatment status, with standard errors clustered at the EA level (observations on 1,784 individuals in 114 clusters).

This instrument was administered to all sampled households at baseline and a revised version of this questionnaire to assess changes is currently being administered to collect the first round of follow-up data. In addition to the household survey instrument, we administered a separate community questionnaire and a market questionnaire (to collect food prices) at baseline. The current follow-up data collection includes (in addition to the community and the market surveys), a school and a health facility questionnaire. Copies of the questionnaires are separately attached to this proposal.

3.5. *Biomarker data collection for HIV and other sexually transmitted diseases*

One of the research questions this study is trying to shed some light on is whether schooling CCTs can reduce HIV infection among young people. So as not to solely rely on self-reported data on sexual behavior and fertility, we have decided to administer home-based voluntary counseling and testing (HCT) to *50%* of the sample during the first follow-up interviews and the *entire* sample during the second follow-up data collection at the end of 2009.[31]

In addition to HIV, the HCT will collect biomarker data for HSV-2 (herpes simplex virus) and syphilis.[32] HSV-2 and syphilis serology can be used as proxies for risky sexual behavior among persons aged 25 and younger (see, for example, Obasi et al, 1999). As such, collection of these biomarkers will add great value to the study by enhancing its statistical power.[33]

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[31] The reason why the entire sample is not tested for HIV and other STDs at the end of year 1 is that this would eliminate the possibility of experimentally assessing the pure impact of the second year of CCT intervention. Since being tested for HIV and learning the results is like an intervention itself, the sub-sample that remains untested until the end of the intervention will help us identify the two-year impact of the CCT program.

[32] The team is also planning to collect biomarkers for anemia and malaria during the second follow-up data collection on the entire sample at the end of 2009.

[33] As explained earlier, given that the prevalence and incidence of HSV-2 is expected to be much higher than that of HIV among the study population of school-aged women, the study is more likely to be able to detect differences between treatment and control groups in HSV-2 prevalence than HIV after just one year. In the longer run, we hope to be able to more clearly detect any differences in HIV prevalence/incidence.

All three tests will be conducted using rapid test kits, needing only a finger prick. The consenting participants will be given their results, receive counseling to explain the results, and referred to public clinics as necessary.[34] The HCT will be conducted by Malawian professional nurses and/or HCT counselors certified in conducting rapid HIV tests and voluntary testing and counseling through the Ministry of Health HIV Unit HCT Counselor Certification Program. HCT will be conducted only upon receiving *ethics and human subject* approval from the relevant Malawian authority – i.e. the National Health Science Research Committee (NHSRC). The study team's application for approval is under review with the NHSRC.

3.6. *Tracking protocols to tackle sample attrition*

One particular problem attributable to studies that collect longitudinal data is non-random attrition. The problem can be significant in biasing results even in observational studies (see, for example, Beegle, De Weerdt, and Dercon, 2008), but it is of utmost importance when the longitudinal data are being collected to evaluate the impact of an intervention, one of the main impacts of which may be selective migration. This is highly likely to be the case in the context of a CCT intervention: beneficiaries are more likely to stay in the area they were found at baseline to attend school (or they may move to attend a secondary school, the location of which is known to the researcher), while individuals in the control group are more likely to move away as they drop out of school, get married, and possibly enter the labor market. Analyzing only the 'stayers' and ignoring the 'leavers' could significantly underestimate the impact of the program on school enrolment, attendance, and attainment, as well as introducing bias to many other outcomes of interest.

For this reason, the study has incorporated a full tracking protocol into the follow-up data collection effort. The tracking protocol used in this study is an improved version of what was

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[34] Treatment for AIDS and syphilis are available and free in public clinics in Zomba. HSV-2 has no known treatment.

implemented in the summer of 2008 under a separate study (also in Malawi, with which Chirwa and Özler are affiliated).

Anytime a household (or just the core respondent) is found to have moved (temporarily or otherwise), a detailed tracking form is filled out by the enumerators, which are then followed up by a special tracking coordinator. The coordinator decides whether the household needs to be revisited at a certain date (for temporary movers), visited by a current team in the field (for those who moved within the study district of Zomba) or by a tracking team (for those whom moved outside the district). The tracking is conducted simultaneously with the field work, not only using teams in a certain area to 'track' those who happened to move to that area, but also using a special tracking team to travel to certain areas outside the district once enough tracking cases are accumulated. In addition, once all of the 176 sampled EAs have been visited, there will also be a second wave tracking effort to locate those who have still not been interviewed.

3.7. *Cash Transfer Program Logistics*

Following the baseline survey, which took place in the fall of 2007; program beneficiaries were notified of the cash transfer program and given offers to participate. 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 (whether the offer is conditional or not; the attendance requirements for the conditional offers; duration of the program, etc.). In addition, the information sheet described for secondary school CCT recipients that their school fees would be paid in full. The contract was then signed by both recipients – guardian and the core respondent. The first transfers were made in February 2008 and the final transfer for the first year of the program is scheduled for November, 2008. Then, the program will continue in 2009 in the same manner as it was implemented in 2008.

The cash transfer program is being implemented by a local NGO based in Zomba. The NGO has local staff members, who are very knowledgeable about the study area and have a good

rapport with the program beneficiaries. It conducts both the attendance verification as well as the cash transfers with close support from the study team.

The monthly cash transfers take place at a number of meeting points across treatment EAs. The meeting points are located in such a way so that no respondent has to travel more than five kilometers to pick up the transfer. At each meeting some basic information is collected for each sample respondent, such as who is picking up each of the two envelopes for the 'parental' and 'individual' transfers (girl, guardian, or proxy), how far they had to travel, etc.

As part of the program, the attendance of all the conditional cash transfer recipients is monitored and they only get the transfer if they have attended school at a satisfactory level (80% attendance during days school was in session for the month in question) during the previous month. The attendance is confirmed using a combination of physical checks (by visiting the school and checking the attendance ledgers) and phone calls to the school principal. These are further combined with unannounced, random spot checks, during which the attendance information from program beneficiaries are contrasted with physical confirmation of the student's presence in the school that day by the program's administrative staff. Both the double checking of information from phone calls with physical checks and the spot checks revealed that the attendance data collected from the ledgers is reliable and accurate.

4. Organization

4.1. *Work Program*

Baseline data were collected between October, 2007 and February, 2008. A progress report (along with PowerPoint presentations) was written to describe the study design, field work details, and descriptive statistics about the study sample using baseline data. First round of follow-up data collection began on October 6, 2008 and will continue until the end of January, 2009. Several working papers describing the one-year impacts of the intervention will become available during the first half of 2009.

The study team plans to return to the field again in October, 2009 to conduct the second round of follow-up data collection. This round of data collection will coincide with the end of the CCT intervention, and the study team hopes that these data will provide conclusive evidence on the full impact of the entire two-year program. The funds requested under this proposal are to finance this round of data collection.

Finally, the study team hopes to return the field to interview the entire sample again in 2011 to assess longer-term impacts of the intervention. The team feels that this additional round of data collection is necessary to establish whether the short-to-medium term impacts of the program, if any, were fleeting or sustained. In addition, these data will allow the team to examine impacts that can only be assessed in the longer run, such as labor market outcomes, or investments in the human capital of the infants and children born to young women in the sample.

The study team is a coherent unit with skills that complement each other perfectly. While there is no explicit division of responsibilities to oversee the intervention, the data collection, and the production of papers and reports, the analytical design greatly benefited from the expertise of Craig McIntosh, who has extensive experience conducting randomized and quasi-experimental impact evaluations. The considerable experience Ephraim Chirwa brings in collecting household survey data in Malawi, his knowledge of both the Malawian economy and the study district, and his access to competent field workers made him a perfect fit to oversee the day-to-day management of data collection efforts. Sarah Baird brings in experience with organizing and conducting field work in East Africa, as well as managing large data sets, and is in charge of quality control for the data coming from the field (for both administrative data from the intervention and household survey data from the study sample). Berk Özler has used his experience as a member of DECRG's poverty team (which houses the LSMS surveys) in helping to design the rich and detailed survey instruments for this study. The main research questions in this study directly follow from his research interest in the role of CCTs in development. As the sole member of the study team from the World Bank, he has also taken the lead in raising funds to make the intervention and its impact evaluation possible. The team plans to co-author the main

impact papers from the study. Members of the team hope to produce many other journal articles and policy notes using the rich, experimental data collected under the study – either jointly, with other collaborators, or solo.

4.2. *Study Team*

The study team for this project has substantial expertise and experience in data collection and analyses related to the proposed study. The study is a collaborative effort between researchers from the World Bank, the University of California at San Diego, and the University of Malawi (Chancellor College). CVs of all principal investigators listed below are separately attached to this proposal.

The principal investigators on the team are Sarah Baird (University of California at San Diego), Ephraim Chirwa (University of Malawi, Chancellor College), Craig McIntosh (University of California at San Diego) and Berk Özler (DECRG, The World Bank). Dr. Baird has a PhD in Agriculture and Resource Economics from the University of California at Berkeley. She is currently a Postdoctoral Fellow at the Graduate School of International Relations and Pacific Studies at the University of California at San Diego. Baird has extensive experience conducting field work in developing countries, particularly in Africa. Of particular relevance to this study, she has worked on a randomized evaluation aimed at youth in Kenya that examines the long run impacts of a schooling based health intervention. Baird has also spent time at the World Bank working on analysis of large survey based datasets.

Dr. Chirwa has a PhD in Economics from the University of East Anglia and is currently an Associate Professor of Economics at the Department of Economics of Chancellor College, University of Malawi. He is the former head of his department and is also the managing consultant of Wadonda Consult, a private consulting firm based in Zomba with a long and reputable experience in consultancy services including carrying out needs assessment studies, household surveys, impact assessment studies, and evaluations of development projects in

Malawi. His work has been published in various academic journals – most recently in *Development Southern Africa, Journal of Industrial Economics, Applied Financial Economics,* and *Development Policy Review.*

Dr. McIntosh is an Assistant Professor of Economics at the Graduate School of International Relations and Pacific Studies at University of California at San Diego. His PhD is from the Agriculture and Resource Economics department at the University of California at Berkeley, and his work focuses on the design of institutions which promote economic mobility among the poor in East Africa and Latin America. He has extensive experience conducting randomized and quasi-experimental impact evaluations, including work in Uganda, Rwanda, Malawi, Guatemala, and Mexico. His work has been published in the *Journal of Development Economics, Review of Economics and Statistics, Economic Journal,* and *Economic Development and Cultural Change.*

Dr. Özler is a senior economist at the Development Research Group of the World Bank and a Visiting Scholar at the Graduate School of International Relations and Pacific Studies at the University of California at San Diego. He holds a PhD in Economics from Cornell University. He has extensive experience working on household surveys, including data collection in Malawi through his work on another project (MTM). He has also worked on various aspects of poverty and inequality measurement in the past and investigated the relationship between income inequality and various outcomes, such as crime, pro-poor targeting, elite capture, and most recently health. He has also written about CCTs and was a member of the team that authored the World Development Report 2006: Equity and Development. His work has been published in various academic journals and volumes -- most recently in the *Journal of Development Economics, Economic Development and Cultural Change, Journal of Economic Inequality,* and *Journal of Public Economics.*

These principal investigators lead all phases of the project, including overall study design, questionnaire development, implementation of field interviews, voluntary counseling and testing following appropriate protocols (for HIV and other biomarkers), data management and

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analysis (including public release of data from the study), producing working papers, journal articles, and policy notes, and finally dissemination of study results.

As noted before, the team includes researchers with in-depth experience in data collection and analysis in Malawi. This experience offers numerous advantages, not the least of which is the ability to work with high-quality field staff for data collection, which is perhaps most critical for the non-traditional interview components of the study. During baseline data collection, the research team recruited its field staff from the best field supervisors from previous studies, all of whom have been retained to work on the current round of follow-up data collection. The data collection is managed by *Wadonda Consult*, under the supervision of Dr. Chirwa. Data entry is managed by two research assistants who reside in our field office in Zomba and manage a team of five data entry staff.

4.3. *Capacity Building*

The members of the study team agreed that there is much to be gained from long-term research collaboration (between the World Bank Development Economics Research Group and the Department of Economics at Chancellor College of the University of Malawi) that will enhance the capacity of all sides to conduct high quality research. As a result, a memorandum of understanding (MOU) was signed by both sides on June 17, 2008 (attached to this proposal).[35] Under this MOU, it was agreed that:

  a. Interested and qualified Chancellor College staff and students will be integrated in the research activities, including data collection, analysis, and producing articles and reports. Furthermore, data from the study will be made available to them.

  b. The study will financially support two graduate students at the Department of Economics (enabling the Department to waive their tuition and fees). These

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[35] The MOU was signed by Winford Masanjala (Chair of the Economics Department, Chancellor College, University of Malawi) and Berk Özler (Senior Economist, the World Bank).

students, selected by the Head of the Department, will be involved in various field and research activities.

c. The study team will provide training and short courses at Chancellor College. These may include training in STATA; courses on research design, sampling, and questionnaire design; and quantitative data analysis methods. A one week STATA course was held in Zomba in June, 2008.

d. To facilitate research activities at Chancellor College, the project will donate all equipment procured for this study to the Department of Economics upon completion of final round of data collection and entry.

4.4. *Dissemination Strategy*

Early dissemination (mostly about project design and descriptive statistics from baseline data) has been conducted through presentations in workshops, conferences, and seminars. Presentations have been made in two GDN Workshops in Cairo and Brisbane, at the Pacific Conference for Development Economics in San Diego, and at a seminar intended for the World Bank country team, officials from the government of Malawi, and the donor community in Lilongwe, Malawi.

The study aims to produce two sets of products: a longitudinal, multi-dimensional data set and analytical studies. The dissemination strategy encompasses wide dissemination of the data, as well as publication of the findings in peer-reviewed journals and as policy notes, working papers, chapters in edited volumes, etc.

The target audience for the data will be researchers and policy-makers in the region as well as the international research community. Since the data will be longitudinal (multiple observations on core respondents over time) and multi-dimensional (household surveys, community surveys, individual biomarker data), accessing these data will require a fairly technical background in data analysis. Following the protocol of the LSMS, the data will be made

available on-line for public use in two different sites: the website of the Global Development Network and a new website designed specifically for this project (or, alternatively, on the LSMS website). There will be no-charge for downloading the data. Data are expected to be on-line within 18 months of completion of data entry (following DECRG guidelines on public release of data). The data will be disseminated with appropriate documentation, again following LSMS protocol for documentation.

The outcomes of the experiment will inform the Bank's work on CCT programs in general and its work in Malawi and sub-Saharan Africa in particular. This will happen through the dissemination of findings within the Bank, the academic community, the donor community, the country, and the region. The dissemination strategy includes publication of working papers, publications in highly respected academic journals, policy reports and briefs tailored towards particular groups of policy makers, seminars and presentations at international conferences with a target audience of policy-makers, researchers, NGOs, and international donor agencies. The Global Development Network also plans to publicize the results from the study and publish one paper emerging from this study in a special issue of a journal of its choice or in an edited volume.

We plan to provide multiple dissemination notes that describe the findings of this experiment for the policy audience. Consultation with the Malawi country team has shown that it considers this to be an interesting and important study. Through this and similar instruments, the team hopes to influence Bank's policy dialogue and its lending in various sectors, such as education, social protection, gender, and HIV/AIDS.

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