CASH OR CONDITION? EVIDENCE FROM A CASH TRANSFER EXPERIMENT*

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This article assesses the role of conditionality in cash transfer programs using a unique experiment targeted at adolescent girls in Malawi. The program featured two distinct interventions: unconditional transfers (UCT arm) and transfers conditional on school attendance (CCT arm). Although there was a modest decline in the dropout rate in the UCT arm in comparison with the control group, it was only 43% as large as the impact in the CCT arm at the end of the 2-year program. The CCT arm also outperformed the UCT arm in tests of English reading comprehension. However, teenage pregnancy and marriage rates were substantially lower in the UCT than the CCT arm, entirely due to the impact of UCTs on these outcomes among girls who dropped out of school. *JEL* Codes: C93, I21, I38, J12.

I. INTRODUCTION

Conditional cash transfers (CCTs) are "targeted to the poor and made conditional on certain behaviors of recipient households" (World Bank 2009). A large and empirically well-identified body of evidence has demonstrated the ability of CCTs to improve schooling outcomes in the developing world (Schultz 2004; de Janvry et al. 2006; Filmer and Schady 2011; among many others).[1] Due in large part to the high-quality evaluation of

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1. Many CCT programs require households to comply with only schooling conditions, while some also require compliance with health conditions, such as regular visits to health clinics for children.

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Mexico's PROGRESA, CCT programs have become common in Latin America and began to spread to other parts of the world, with CCT programs now in more than 29 developing countries (World Bank 2009).[2]

There are also rigorous evaluations of unconditional cash transfers (UCTs), which cover a wide range of programs: non-contributory pension schemes, disability benefits, child allowance, and income support. Whether examining a cash transfer program in Ecuador (Bono de Desarollo Humano), the old age pension program in South Africa, or the child support grants also in South Africa, studies find that UCTs reduce child labor, increase schooling, and improve child health and nutrition (Duflo 2003; Case, Hosegood, and Lund 2005; Edmonds 2006; Edmonds and Schady 2009).[3] Hence, UCTs also change the behaviors on which CCTs are typically conditioned.

The debate over the relative merits of these two approaches has intensified as CCTs have become more widely implemented. Proponents of CCT programs argue that market failures may often lead to underinvestment in education or health, which are addressed by the conditions imposed on recipient households. Another advantage of CCT programs is that the conditions make cash transfers politically palatable to middle- and upper-class voters who are not direct beneficiaries of such programs.[4] To critics of conditionality, the theoretical default position should be to favor UCTs, particularly because the marginal contribution of the conditions to cash transfer programs remains largely unknown.[5] Furthermore, the implementation of CCT programs may strain administrative capacity as these programs expand to poorer countries outside of Latin America.

The existing knowledge base concerning the marginal impact of attaching conditions to cash transfer programs remains very limited—especially in sub-Saharan Africa where such evaluations

2. CCTs are also implemented in developed countries. For example, a 3-year pilot CCT program in New York City ended in early 2010. For more on Opportunity NYC, see http://www.nyc.gov/html/ceo/html/programs/opportunity_nyc.shtml.

3. For a recent review of cash transfer programs, see Adato and Bassett (2009), which gives more examples of UCT programs in Sub-Saharan Africa improving education, health, and nutrition among children.

4. For an excellent discussion of the economic rationale for CCTs, see Chapter 2 in World Bank (2009).

5. For a discussion of "The Conditionality Dilemma", see Chapter 8 in Hanlon, Barrientos, and Hulme (2010).

are relatively rare.[6] One strain of relevant literature relies on accidental glitches in program implementation. 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 (2011) and Schady and Araujo (2008) both find that school enrollment was significantly lower among those who thought that the cash transfers were *unconditional*. There is also a literature that takes a structural approach, where a model of household behavior is calibrated using real data, and then the impacts of various policy experiments are simulated. Micro-simulating Brazil's Bolsa Escola program, Bourguignon, Ferreira, and Leite (2003) find that UCTs would have no impact on school enrollment. Todd and Wolpin (2006), examining PROGRESA in Mexico, report that the increase in schooling with unconditional transfers would be only 20% as large as the conditional transfers, whereas the cost per family would be an order of magnitude larger.[7] Overall, the existing nonexperimental evidence suggests that conditionality plays an important role in the overall impact of CCTs.[8]

The ideal experiment to identify the marginal contribution of conditionality in a cash transfer program—that is, 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 previously been conducted anywhere.[9] This article describes the impacts of such an

6. A few experiments to improve the design of CCT programs have recently been conducted, most notably in Colombia (Barrera-Osorio et al. 2011).

7. There is also some evidence that the condition that preschool 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 (2010) and Macours, Schady, and Vakis (2008)—show behavioral changes in the spending patterns of parents and households that they argue to be inconsistent with changes in *just* the household income. These studies, however, cannot isolate the impact of the social marketing campaign from that of the transfers being made to women.

8. However, using an experiment that provided in-kind food transfers in one arm and equal-valued unrestricted cash transfers in another arm in Mexico, Cunha (2010) finds that households receiving the latter consumed equally nutritious foods as the former and that there were no differences in anthropometric or health outcomes of children between the two treatment arms. The study concludes that there is little evidence to justify the paternalistic motivation when it comes to this in-kind food transfer program.

9. 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

experiment in Malawi that provided cash transfers to households with school-age girls. In the experiment, 176 enumeration areas (EAs) were randomly assigned treatment or control status.[10] A subgroup of the 88 treatment EAs was then randomly assigned to receive offers for monthly cash transfers conditional on attending school regularly (CCT arm) while another group of EAs received offers for unconditional cash transfers (UCT arm). Offers included separate transfers to the girls and their parents/guardians. The transfer amounts offered to the parents were randomized at the village level, and those offered to the girls were randomized at the individual level.

In this article, we exploit this experiment to examine the impact of each treatment arm not only on behaviors on which the CCT intervention was conditioned (school enrollment and attendance) but also on outcomes that are of central importance to the long-term prospects of school-age girls: human capital formation (measured by tests of English reading comprehension, mathematics, and cognitive skills), marriage, and childbearing. At the micro-level, improved test scores are associated with increased wages later in life (Blau and Kahn 2005), whereas delayed fertility is associated with improved maternal and child health outcomes.[11] Increased age at first marriage can improve the quality of marriage matches and reduce the likelihood of divorce, increase women's decision-making power in the household, reduce their chances of experiencing domestic violence, and improve health care practices among pregnant women (Goldin and Katz 2002; Jensen and Thornton 2003; Field and Ambrus 2008). At the macro-level, improved cognition may lead to more

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 fingerprinting machines at schools). A similar pilot in Burkina Faso has comparative treatment arms for conditional and unconditional transfers.

10. An EA consists of approximately 250 households spanning several villages.

11. Evidence on the causal effects of childbearing as a teen is inconclusive in both the biomedical and the economics literature. While some argue that gynecological immaturity increases the likelihood of preterm births and competition for nutrients between the mother and baby can cause low birth weight, the evidence is mixed (Fraser, Brockert, and Ward 1995; Akinbami, Schoendorf, and Kiely 2000; Smith and Pell 2001; Horgan and Kenny 2007). In economics, while Hotz, McElroy, and Sander (2005) argue that "much of the 'concern' that has been registered regarding teenage childbearing is misplaced," the debate is ongoing (Ashcraft and Lang 2006; Fletcher and Wolfe 2009; Dahl 2010).

growth (Hanushek and Woessmann 2009), and lower fertility rates may also contribute to economic growth through increased female labor supply (Bloom et al. 2009) and by allowing greater investments in the health and education of children.

Starting with schooling outcomes, we find that although dropout rates declined in both treatment arms, the effect in the UCT arm is 43% as large as that in the CCT arm.[12] Evidence from school ledgers for students enrolled in school also suggests that the fraction of days attended in the CCT arm is higher than the UCT arm. Using independently administered tests of cognitive ability, mathematics, and English reading comprehension, we find that although achievement is significantly improved in all three tests in the CCT arm compared with the control group, no such gains are detectable in the UCT arm. The difference in program impacts between the two treatment arms is significant at the 90% confidence level for English reading comprehension. In summary, the CCT arm had a significant edge in terms of schooling outcomes over the UCT arm: a large gain in enrollment and a modest yet significant advantage in learning.

When we turn to examine marriage and pregnancy rates, however, unconditional transfers dominate. The likelihood of being ever pregnant and ever married were 27% and 44% lower in the UCT arm than in the control group at the end of the 2-year intervention, respectively, whereas program impacts on these two outcomes were small and statistically insignificant in the CCT arm. These substantial delays in marriage and fertility in the UCT arm are found entirely among adolescent girls who dropped out of school after the start of the 2-year intervention; rates of marriage and fertility among girls still enrolled in school at follow-up were negligible regardless of treatment status. Hence, the success of the conditionality in promoting the formation of human capital among the compliers appears to be achieved at the cost of denying transfers to noncompliers who are shown to be particularly at risk for early marriage and teenage pregnancy.

By exploiting an experiment featuring a CCT and a UCT arm and by broadening the impact assessment beyond

12. Our preferred measure of enrollment uses enrollment data for each term in 2008 and 2009, which are confirmed by the teachers during the school surveys. Self-reported measures of enrollment produce divergent results in terms of the relative effectiveness of the CCT and UCT arms. We discuss measurement issues in detail in Section II and present enrollment impacts using self-reported data as well as data collected from the schools in Section III.

schooling, our study exposes a trade-off that is inherent in CCT programs. The existing literature is focused primarily on assessing the desired behavior change in CCT programs, and may have overlooked the effects of denying benefits to those who fail to satisfy the conditions. Our findings show that UCTs can improve important outcomes among such households even though they might be much less effective than CCTs in achieving the desired behavior change. The trade-off between improved schooling outcomes and delayed marriage and childbearing among school-age girls illuminates the importance of carefully considering what exactly transfer programs are trying to achieve in the target population.

In the next section, we describe the study setting and sample selection, the research design and the intervention, as well as the multiple sources of data collection undertaken for this study. As the CCT and UCT interventions took place simultaneously in different communities within the same district, we include a discussion of the circumstances under which this experiment was conducted and provide evidence on the program beneficiaries' understanding of program rules. Issues concerning the measurement of various schooling outcomes are also discussed in this section. Section III describes the estimation strategy and presents the main program impacts on schooling, fertility, and marriage by treatment arm, as well as the heterogeneity of these impacts by age group and transfer size. Section IV concludes.

II. Background, Study Design, and Data

II.A. Study Setting

Malawi, the setting for this research project, is a small and poor country in southern Africa. Eighty-one percent of its population of 15.3 million lived in rural areas in 2009, with most people relying on subsistence farming. The country is poor even by African standards: Malawi's 2008 GNI per capita figure of $760 (PPP, current international $) is less than 40% of the Sub-Saharan African average of $1,973 (World Development Indicators 2010). According to the same data source, net secondary school enrollment is very low at 24%.

II.B. Sampling

Zomba District in the southern region was chosen as the site for this study. Zomba District is divided into 550 enumeration

areas (EAs), which are defined by the National Statistical Office of Malawi and contain an average of 250 households spanning several villages. Fifty of these EAs lie in Zomba city, and the rest are in seven traditional authorities. Prior to the start of the experiment, 176 EAs were selected from three different strata: Zomba city (urban, 29 EAs), near rural (within a 16 km radius of Zomba city, 119 EAs), and far rural (28 EAs). The choice of a 16 km radius around Zomba city was arbitrary and based mainly on a consideration of transport costs.

In these 176 EAs, each dwelling was visited to obtain a full listing of never-married females, aged 13–22.[13] The target population was then divided into two main groups: those who were out of school at baseline (*baseline dropouts*) and those who were in school at baseline (*baseline schoolgirls*). *Baseline schoolgirls*, who form 87% of the target population within our study EAs and among whom the conditionality experiment was carried out, are the subject of this article.[14] In each EA, a percentage of baseline schoolgirls were randomly selected for the study. These sampling percentages differed by strata and age group and varied between 14% and 45% in urban areas and 70% to 100% in rural areas. This procedure led to a total sample size of 2,907 schoolgirls in 176 EAs, or an average of 16.5 per EA.

13. The target population of 13–22-year-never-married females was selected for a variety of reasons. As the study was designed with an eye to examine the possible effect of schooling cash transfer programs on the risk of HIV infection, we focused on females because the HIV rate among boys and young men of schooling age is negligible. The age range was selected so that the study population was school-age and had a reasonable chance of being or becoming sexually active during the study period. Finally, a decision was made to not make any offers to girls who were (or had previously been) married, because marriage and schooling are practically mutually exclusive in Malawi—at least for females in our study district.

14. Many cash transfer programs are school-based, meaning that they do not cover those who have already dropped out of school (see, for example, the discussion of Cambodia's CESSP Scholarship Program in Filmer and Schady 2009). Other programs, such as PROGRESA in Mexico, covered baseline dropouts, but studies usually exclude this group from the evaluation due to the one-time effect at the onset of the program for this group (de Janvry and Sadoulet 2006). While outcomes for *baseline dropouts* were also evaluated under the broader study, they are not the subject of this article because the conditionality experiment was not conducted among this group. As the sample size for this group is quite small (889 girls in 176 EAs at baseline, i.e., approximately 5 girls per EA), dividing the treatment group into a CCT and a UCT group would yield an experiment with low statistical power. Hence, in treatment EAs, this group received CCT offers only.

II.C. Study Design and Intervention

Treatment status was assigned at the EA level and the sample of 176 EAs was randomly divided into two groups of equal size: treatment and control. The sample of 88 treatment EAs was further divided into two arms based on the treatment status of *baseline schoolgirls*: (i) CCT arm (46 EAs), and (ii) UCT arm (27 EAs). In the remaining 15 treatment EAs, no baseline schoolgirls were made offers to receive cash transfers.[15] Excluding the 623 girls who lived in intervention EAs but did not receive an offer, we are left with a sample of 2,284 *baseline schoolgirls* in 161 EAs (1,495 in 88 control EAs, 506 in 46 CCT EAs, and the remaining 283 in 27 UCT EAs). See Figure I for an illustration of the sample. No EA in the sample had a similar cash transfer program before or during the study.

CCT Arm. After the random selection of EAs and individuals into the treatment group, the local nongovernmental organization (NGO) retained to implement the cash transfers held meetings in each treatment EA between December 2007 and early January 2008 to invite the selected individuals to participate in the program. At these meetings, the program beneficiary and her parents/guardians were made an offer that specified the monthly transfer amounts offered to the beneficiary and her parents, the condition to regularly attend school, and the duration of the program.[16] An example of the CCT offer letters can be seen in Online Appendix A. It was possible for more than one eligible girl from a household to participate in the program. Transfer amounts to the parents were varied randomly across EAs between \$4, \$6, \$8, and \$10 per month, so that each parent within an EA received the same offer. Within each EA, a lottery was held to determine the transfer amount to the young female program beneficiaries,

15. To measure potential spillover effects of the program, a randomly selected percentage (33%, 66%, or 100%) of baseline schoolgirls in each treatment EA were randomly selected to participate in the cash transfer program. In the 15 treatment EAs, where no baseline schoolgirls were offered cash transfers, this percentage was equal to 0. In these 15 EAs, the only spillovers on *baseline schoolgirls* would be from the *baseline dropouts* who were offered CCTs. We do not use this random variation in treatment intensity in this article.

16. Due to uncertainties regarding funding, the initial offers were only made for the 2008 school year. However, on 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 continue until the end of 2009.

FIGURE I
Study Sample and Attrition

which was equal to \$1, \$2, \$3, \$4, or \$5 per month.[17] The fact that the lottery was held publicly ensured that the process was transparent and helped the beneficiaries view the offers they received as fair. In addition, the offer sheet for CCT recipients eligible to attend secondary school stated that their school fees would be paid in full directly to the school.[18]

Monthly school attendance for all girls in the CCT arm was checked and payment for the following month was withheld for any student whose attendance was below 80% of the number of days school was in session for the previous month.[19] However, participants were never administratively removed from the program for failing to meet the monthly 80% attendance rate, meaning that if they subsequently had satisfactory attendance, their payments would resume. Offers to everyone, identical to the previous one they received and regardless of their schooling status during the first year of the program in 2008, were renewed between December 2008 and January 2009 for the second and final year of the intervention, which ended at the end of 2009.

UCT Arm. In the UCT EAs, the offers were identical with one crucial difference: there was no requirement to attend school to receive the monthly cash transfers. An example of the UCT offer letters can also be seen in Online Appendix A. Other design

17. The average total transfer to the household of \$10/month for 10 months a year is nearly 10% of the average household consumption expenditure of \$965 in Malawi (calculated using final consumption expenditure for 2009, World Development Indicators 2010). This falls in the range of cash transfers as a share of household consumption (or income) in other countries with similar CCT programs. Furthermore, Malawi itself has a Social Cash Transfer Scheme, which is now under consideration for scale-up at the national level that transferred \$12/month plus bonuses for school-age children during its pilot phase (Miller and Tsoka 2007).

18. Primary schools are free in Malawi, but students have to pay nonnegligible school fees at the secondary level. The program paid these school fees for students in the conditional treatment arm upon confirmation of enrollment for each term. Private secondary school fees were also paid up to a maximum equal to the average school fee for public secondary schools in the study sample.

19. We were initially concerned that teachers may falsify attendance records for program beneficiaries—either out of benevolence for the student or perhaps to extract bribes. To ensure this did not happen, a series of spot checks were conducted about halfway through the first year of the program in 2008. This meant that the program administrators went to a randomly selected subsample of schools attended by girls in the CCT arm and conducted roll calls for the whole class after attendance for that day had been taken. In all schools but one, the ledger perfectly matched the observed class attendance for that day. As these spot checks were expensive to conduct, they were discontinued after the study team was convinced that the school ledgers gave an accurate reflection of real attendance.

aspects of the intervention were kept identical to be able to isolate the effect of imposing a schooling conditionality on the primary outcomes of interest. For households with girls eligible to attend secondary schools at baseline, the total transfer amount was adjusted upward by an amount equal to the average annual secondary school fees paid in the conditional treatment arm.[20] This additional amount ensured that the average transfer amounts offered in the CCT and UCT arms were identical and the only difference between the two groups was the conditionality of the transfers on satisfactory school attendance. Attendance was never checked for recipients in the UCT arm and they received their payments by simply presenting at the transfer locations each month.

The UCT experiment was conducted alongside the CCT experiment in the same district.[21] Even though the offer letters were differentiated carefully and treatment status for each individual was reinforced during the monthly cash transfer meetings by the implementing NGO, it is natural to question whether the beneficiaries in the UCT arm understood the program rules correctly. To interpret the differential impacts between the two treatment arms, it is important to know what was understood by those in the UCT arm as to the nature of their transfers and to understand the context in which the cash transfer experiment was conducted.

As summarized in Section I and presented in detail in Section III, we find statistically significant differences between programs impacts in the CCT and UCT for all the main outcome indicators examined in this article: enrollment, test scores, marriage,

20. Because the average school fees paid in the conditional treatment arm could not be calculated until the first term fees were paid, the adjustment in the unconditional treatment arm was made starting with the second of 10 monthly payments for the 2008 school year. The average school fees paid for secondary school girls in the conditional treatment group for Term 1 (3,000 Malawian kwacha, or approximately $20) was multiplied by 3 (to calculate an estimate of the mean annual school fees), divided by 9 (the number of remaining payments in 2008) and added to the transfers received by households with girls eligible to attend secondary school in the UCT arm. The NGO implementing the program was instructed to make no mention of school fees but only explain to these households that they were randomly selected to receive a bonus.

21. The reader will note that there are no ideal conditions under which to conduct this particular experiment. For example, we could have run the CCT and UCT experiments in separate districts with little interdistrict communication, but then we would not be able to rule out the possibility of unobserved heterogeneity driving the results.

and pregnancy. These differences offer prima facie evidence that the two interventions were perceived to be different than each other.[22]

To understand the perceptions of study participants more fully, we conducted qualitative interviews with a random subsample in the autumn of 2010—approximately 9 months after the 2-year intervention ended in December 2009. Of the 15 girls randomly selected from the UCT arm, only 1 of them reported a fear of losing payments if she was not attending school.[23] The interviews with those in the UCT arm lead to two clear conclusions. First, the rules of the program were well understood by the girls in the UCT arm; the interviews make clear that UCT girls knew that nothing was required of them to participate in the program and they were given no rules or regulations tied to the receipt of the transfers other than showing up at the predetermined cash transfer locations. Second, girls in the UCT arm were very much aware of the CCT intervention. Interviews suggest that the girls in the UCT arm not only knew about the CCT program but many actually had friends or acquaintances in the CCT arm. Through these contacts they knew that school attendance was strictly monitored in the CCT arm, and that noncompliers were penalized.[24]

The evidence from the in-depth interviews makes it clear that the UCT experiment did not happen in a vacuum. Instead, it took place under a rubric of education that naturally led the beneficiaries to believe that the program aimed to support girls to

22. In Online Appendix E, we provide further evidence on this issue by showing that the exogenous variation in the number of CCT beneficiaries at the monthly cash transfer meetings had no effect on outcomes in the UCT arm.

23. Similarly, we can find no evidence supporting the notion that girls in the UCT arm thought their transfer payments were conditional on not getting married.

24. The following excerpt from an interview (with respondent ID 1461204) is a good example:

Interviewer: *Earlier you talked of conditional and unconditional. What did you say about the rules for conditional girls?*

Respondent: *They had to attend class all the time... not missing more than 3 days of classes in a [month]—like I already explained.*

Interviewer: *How did you say the program managers knew about the missed school days?*

Respondent: *They would go to the schools... For example, I have a friend, [name], who was learning at [school name]. They would go each month to the school to monitor her attendance, and if she was absent for more than three days she would not get her monthly money.*

further their education. The differential impacts of the UCT and CCT interventions should be interpreted in this context.[25]

II.D. Data Sources and Outcomes

Data sources. The data used in this article were collected in three rounds. Baseline data, or Round 1, were collected between October 2007 and January 2008, before the offers to participate in the program took place. First follow-up data collection, or Round 2, was conducted approximately 12 months later—between October 2008 and February 2009. The second follow-up (Round 3) data collection was conducted between February and June 2010—after the completion of the 2-year intervention at the end of 2009 to examine the final impacts of the program. To assess program impacts on school enrollment, attendance, and achievement, as well as on fertility and marriage, we use multiple data sources: household surveys (all rounds), school surveys (Rounds 2 and 3), school ledgers, independently developed achievement tests, and qualitative interviews (Round 3 only).

The annual household survey consisted of a multitopic questionnaire administered to the households in which the sampled respondents resided. It consisted of two parts: one that was administered to the head of the household and the other administered to the core respondent, that is, the sampled girl from our target population. The former collected information on the household roster, dwelling characteristics, household assets and durables, shocks, and consumption. The survey administered to the core respondent provides detailed information about her family background, schooling status, health, dating patterns, sexual behavior, fertility, and marriage.

During Round 2, we also conducted a school survey that involved visiting every school attended by any of the core respondents (according to self-reported data from the household survey) in our study sample in 2008. This procedure was repeated in Round 3 for a randomly selected subsample of core respondents who reported being enrolled in school in 2009.[26] Using Round 2

25. It is also possible that the existence of a CCT program could have reduced the motivation of the girls in the UCT arm.

26. The reason the school survey in Round 3 was conducted for a randomly selected subsample instead of every student who reported being enrolled in school in 2009 is that school ledgers were also sought to check the attendance of core respondents. As locating these ledgers, examining them, and recording attendance for each core respondent is time-consuming and costly, the study team decided to reduce the sample size.

(Round 3) household survey data, we collected the name of the school, the grade, and the teacher's name for the core respondent if she reported being enrolled in school at any point during the 2008 (2009) school year. These teachers were then located at the named schools and interviewed about each respondent's schooling status (term by term) during the past school year. Furthermore, during Round 3, school ledgers were sought to check the attendance of respondents for each school term in 2009 and the first term of 2010.

To measure program impacts on student achievement, mathematics and English reading comprehension tests were developed and administered to all study participants at their homes. The tests were developed by a team of experts at the Human Sciences Research Council according to the Malawian curricula for these subjects for Standards 5–8 and Forms 1–2.[27] In addition, to measure cognitive skills, we utilized a version of Raven's Colored Progressive Matrices that was used in the Indonesia Family Life Survey (IFLS-2).[28] The mathematics and English tests were piloted for a small, randomly selected subsample of the study participants in the control group before being finalized for administration during Round 3 data collection. These tests were administered by trained proctors at the residences of the study participants and were always administered after the household survey, but never on the same day. The order of the math and English tests were randomized at the individual level and the Raven's test was always administered last.

Finally, structured in-depth interviews were conducted with a small sample of study participants, their parents or guardians, community leaders, program managers, and schools. The sample was selected randomly using block stratification based on treatment status at baseline, as well as schooling and marital status at Round 3. The total number of baseline schoolgirls sampled was 48, of whom 46 were successfully interviewed.[29] The main aim of these structured interviews was to gauge the "understanding of the cash transfer intervention" by study participants. In addition, topics of discussion included schooling decisions, dating, fertility, and marriage, as well as empowerment and future aspirations.

27. In Malawi, there are eight grades in primary school (Standard 1–8) and four in secondary school (Form 1–4).

28. These three tests are available from the authors upon request.

29. We have 14 in-depth interviews in the control group, 17 in the CCT arm, and 15 in the UCT arm.

The interviews usually lasted 60–90 minutes and were conducted by trained enumerators, many of whom had previous experience in qualitative field work. The conversations in Chichewa were taped and transcribed in English immediately after the interviews.

Outcomes: Schooling. We measure enrollment and attendance using three different data sources. The first indicator is constructed using self-reported data from the household survey on whether the core respondent was enrolled in school. These questions are asked for each of the seven school terms between Term 1, 2008, and Term 1, 2010. As self-reported data may overstate enrollment, we cross-validated these data by visiting the schools the study participants reported attending and asking the same questions of the teachers of the core respondents. The enrollment indicators from the school survey are coded '0' if the core respondent reported not being enrolled in school for that term or if the teacher reported her as not enrolled and '1' if her teacher(s) confirmed that she was attending school during the relevant term. Finally, as enrollment may be a poor proxy for actual school attendance, we utilize the attendance ledgers for the 2009 school year and the first term of 2010 collected during the school surveys in Round 3 to construct an indicator for the percentage of days the core respondent enrolled in school was recorded present during days the school was in session.

We did not independently monitor the school attendance of study participants through random spot checks. Although studies such as Miguel and Kremer (2004), Kremer, Miguel, and Thornton (2009), and Barrera-Osorio et al. (2011) have measured attendance directly, we deliberately chose to forgo this method of data collection to protect the validity of the UCT experiment. Despite having data on enrollment from the teachers and attendance from school ledgers, direct observation would clearly have produced superior evidence to the alternative measures of school participation used in this article. However, as reported above in Section II.C, girls in the UCT arm were fully aware that the attendance of CCT recipients were being regularly monitored, which led them to believe that program managers cared about the education of the girls in the CCT arm. We were concerned that performing random spot checks of attendance for girls in the UCT arm could have given them the impression that they were also supposed to attend

school regularly to receive their payments.[30] As a consequence, we chose to avoid direct monitoring of attendance to retain as sharp a differential test as possible of the relative merits of conditional and unconditional transfers.

As important as school attendance may be for adolescent girls, perhaps as important is learning achievement and cognitive skills.[31] To measure these, we conducted independently developed tests of mathematics, English reading comprehension, and cognitive ability. Total number of correct answers in each of these tests is standardized to have a mean equal to '0' and standard deviation equal to '1' in the control group and program impacts are presented as changes in standard deviations (SD).

Outcomes: Marriage and Fertility. Teenage pregnancy in Malawi is common with the adolescent fertility rate at 133 per 1,000 women aged 15–19 (World Development Indicators 2010).[32] Many girls cite pregnancy as the main reason for dropping out of school and getting married at an early age. Each of the core respondents was asked the following questions in each round: "Have you ever been pregnant or are you currently pregnant?" and "What is your marital status?" We use the answers to these questions to calculate the prevalence of marriage and pregnancy in Rounds 2 and 3.

III. Estimation Strategy and Results

III.A. Sample Attrition and Balance

Figure I summarizes the study sample and attrition. We began with a sample of 2,284 respondents who were in school at baseline and formed the experimental sample for our study of conditionality. Of this sample, 2,186 were tracked successfully

30. This is nicely illustrated during an in-depth interview with one of the core respondents (respondent ID 1332203): After describing the CCT girls being followed to their schools to monitor their attendance, which she explained showed her that the program was interested in attracting girls to go to school, she was asked by the interviewer in what way the program managers cared (about schooling). She answered by saying: "They cared for the conditional group only but on the other group they didn't care."

31. Other schooling outcomes, such as repetition and reentry rates, are also important and can lead to different inferences regarding program impacts on schooling attainment. See, for example, Behrman, Sengupta, and Todd (2005).

32. For comparison, the same figure is 35 in the United States and 64 in Mexico (World Development Indicators 2010).

for the Round 3 household survey and 2,089 were successfully interviewed in all three rounds, a tracking rate of over 90%. Of the 983 subjects randomly sampled for the school survey in Round 3, enrollment data reported by teachers are available for 922 of them. We were less successful in locating attendance ledgers; of the 821 girls who were selected for the Round 3 school survey and reported being enrolled in school in 2009, legible ledgers for at least one term are only available for 384.

Table I examines attrition across the two treatment arms and the control group separately by each of our data sources: household surveys, achievement tests, and school surveys. The regression analysis indicates that there is no significant differential attrition between the two treatment arms in any of the six subsamples used for analysis in this study. Study participants in both treatment arms, however, were equally more likely to take the achievement tests than the control group. Similarly, legible ledgers are more likely to be found for treatment girls. Thus, the analysis of the available samples should give us unbiased estimates of *differential* program impacts on schooling outcomes, marriage, and childbearing.[33]

In Table II, we test the balance of the experiment using baseline data for the sample used in the analysis, that is, those successfully interviewed during all three rounds. Panel A shows balance on household attributes and Panel B on individual characteristics. Overall, the experiment appears well balanced between the treatment and control groups over a broad range of outcomes; column (4) shows that the two treatment arms only differ in age and highest grade attended at baseline—two variables that are highly correlated with each other. Although the share of female-headed households is balanced between the two treatment arms, it is significantly higher in the control group.[34]

III.B. Estimation Strategy

The experimental study design gives us a reliable source of identification. To estimate intention-to-treat effects of the

33. To assess the robustness of our findings with respect to attrition from the sample that took the achievement tests, we perform a bounding exercise (Lee 2009), described in detail in note 39.

34. In Online Appendix Tables B.1 and B.2, we examine balance for the same set of characteristics for the school survey and ledger subsamples analyzed in this article. Both of these tables look similar to Table II, confirming balance in these subsamples.

TABLE I

ANALYSIS OF ATTRITION

	Dependent variable					
	(1)	(2)	(3)	(4)	(5)	(6)
	=1 if surveyed in Round 3	=1 if surveyed in all three Rounds	=1 if took educational tests	=1 if information found in Round 2 survey	=1 if information found in Round 3 school survey	=1 if legible ledger found
Conditional treatment	0.020	0.021	0.029*	0.033	−0.000	0.116*
	(0.015)	(0.030)	(0.016)	(0.024)	(0.027)	(0.064)
Unconditional treatment	0.021	0.030	0.035*	−0.029	0.014	0.061
	(0.019)	(0.024)	(0.020)	(0.053)	(0.028)	(0.077)
Mean in the control group	0.946	0.893	0.929	0.890	0.935	0.378
Number of observations	2,284	2,284	2,284	2,284	983	821
Prob > F(Conditional = Unconditional)	0.965	0.797	0.801	0.246	0.627	0.513

Notes. Regressions are OLS models with robust standard errors clustered at the EA level. All regressions are weighted to make them representative of the target population in the study EAs. Regressions are restricted to the subsample of core respondents who were in school at baseline and sampled to be part of the conditional, unconditional, or control sample. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

TABLE II

BASELINE MEANS AND BALANCE

	Mean (s.d.)			
	(1)	(2)	(3)	(4)
	Control group	Conditional group	Unconditional group	p-value (Conditional–Unconditional)
Panel A: Household-level variables				
Household size	6.432	6.384	6.662	.202
	(2.257)	(2.146)	(2.075)	
Asset index	0.581	0.984	1.221	.623
	(2.562)	(2.740)	(2.447)	
Female-headed household	0.343	0.252**	0.245**	.899
	(0.475)	(0.434)	(0.431)	
Mobile phone access	0.616	0.583	0.605	.799
	(0.487)	(0.494)	(0.490)	
Household transfer amount	N/A	6.991	6.829	.822
		(2.319)	(2.101)	

TABLE II
(CONTINUED)

	Mean (s.d.)			
	(1)	(2)	(3)	(4)
	Control group	Conditional group	Unconditional group	p-value (Conditional–Unconditional)
Panel B: Individual-level variables				
Age	15.252	14.952*	15.424	.007***
	(1.903)	(1.827)	(1.923)	
Highest grade attended	7.478	7.246	7.896**	.004***
	(1.634)	(1.598)	(1.604)	
Mother alive	0.842	0.802	0.836	.360
	(0.365)	(0.399)	(0.371)	
Father alive	0.705	0.714	0.759	.288
	(0.456)	(0.453)	(0.428)	
Never had sex	0.797	0.797	0.775	.582
	(0.402)	(0.403)	(0.419)	
Ever pregnant	0.023	0.030	0.031	.973
	(0.149)	(0.171)	(0.173)	
Individual transfer amount	N/A	3.090	3.033	.606
		(1.431)	(1.451)	
Number of observations	1356	470	261	

Notes. Mean differences statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence. Asterisks on the coefficients in columns (2) and (3) indicate significantly different than the control group, while in column (4) asterisks indicate a significant difference between the conditional and unconditional treatment arms. Standard errors are clustered at the EA level to account for the design effect. Means are weighted to make them representative of the target population in the study EAs. The number of unique observations vary slightly across variables due to missing observations, with the total sample size ranging between 2089 and 2084.

program in each treatment arm on school enrollment, attendance, test scores, marriage, and pregnancy, we employ a simple reduced-form linear probability model of the following type:

$$Y_i = T_i^C \gamma^C + T_i^U \gamma^U + X_i \beta + \varepsilon_i, \quad (1)$$

where Y_i is an outcome variable for individual i, T_i^C and T_i^U are binary indicators for offers to be in the CCT and the UCT arms, respectively, and X_i is a vector of baseline characteristics. The standard errors ε_i are clustered at the EA level, which accounts both for the design effect of our EA level treatment and for the heteroskedasticity inherent in the linear probability model.

In all regressions, we include baseline values of the following variables as controls: a household asset index, highest grade attended, a dummy variable for having started sexual activity, and dummy variables for age. These variables were chosen because they are strongly predictive of schooling outcomes and, as a result, improve the precision of the impact estimates. We also include indicators for the strata used to perform block randomization—Zomba Town, within 16 km of the town, and beyond 16 km (Bruhn and McKenzie 2009). Age- and stratum-specific sampling weights are used to make the results representative of the target population in the study area.

III.C. Results

Schooling: Enrollment. Table III describes enrollment rates by term, including a cumulative variable for the number of terms the girl was enrolled in school during the 2-year intervention that takes on a value between 0 and 6. When we examine self-reported enrollment in Panel A, we see that dropout rates in the control group steadily increase over time with the sharpest change occurring between school years. Impact estimates suggest that dropout rates were significantly lower in both treatment arms, and that the UCT arm outperformed the CCT arm: the program impact on the number of terms the girls were enrolled in school during the 2-year intervention is 0.41 terms in the UCT arm, compared with 0.23 terms in the CCT arm—a difference that is significant at the 95% confidence level.

Self-reported enrollment data can be subject to reporting bias. For example, comparing program impacts using self-reports vs. monitored data, Barrera-Osorio et al. (2011) report that a significant positive bias in self-reported school enrollment compresses the difference between the treatment and control groups,

TABLE III

PROGRAM IMPACT ON SCHOOL ENROLLMENT

	Dependent variable: =1 if enrolled in school during the relevant term							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Year 1: 2008			Year 2: 2009			Year 3: 2010	
	Term 1	Term 2	Term 3	Term 1	Term 2	Term 3	Total terms (6 terms)	Term 1, post-program
Panel A: Program impacts on *self-reported* school enrollment								
Conditional treatment	0.007	0.019*	0.041**	0.049***	0.056***	0.061***	0.233***	0.005
	(0.011)	(0.011)	(0.017)	(0.017)	(0.018)	(0.019)	(0.070)	(0.025)
Unconditional treatment	0.034***	0.051***	0.054***	0.072***	0.095***	0.101***	0.406***	0.074***
	(0.010)	(0.011)	(0.018)	(0.021)	(0.022)	(0.021)	(0.079)	(0.026)
Mean in the control group	0.958	0.934	0.900	0.831	0.800	0.769	5.191	0.641
Number of observations	2,087	2,087	2,086	2,087	2,087	2,087	2,086	2,086
Prob > *F*(Conditional = Unconditional)	0.006	0.012	0.460	0.299	0.102	0.098	0.038	0.028
Panel B: Program impacts on *teacher-reported* school enrollment								
Conditional treatment	0.043***	0.044***	0.061***	0.094**	0.132***	0.113***	0.535***	0.058*
	(0.015)	(0.016)	(0.018)	(0.041)	(0.035)	(0.039)	(0.129)	(0.033)
Unconditional treatment	0.020	0.038**	0.018	0.027	0.059	0.033	0.231*	0.001
	(0.015)	(0.017)	(0.023)	(0.038)	(0.037)	(0.039)	(0.136)	(0.036)
Mean in the control group	0.906	0.881	0.852	0.764	0.733	0.704	4.793	0.596
Number of observations	2,023	2,023	2,023	852	852	852	852	847
Prob > *F*(Conditional = Unconditional)	0.173	0.732	0.067	0.076	0.014	0.020	0.011	0.108

Notes. The dependent variable in Panel A is whether the core respondent reported being enrolled in school for the relevant year/term. The dependent variable in Panel B is whether the teacher reported the core respondent being enrolled in school for the relevant year/term. Post-program refers to Term 1, 2010, the first term after the program ended. Total terms refers to the total number of terms enrolled during the program. Columns (4)–(8) in Panel B are restricted to the subsample of core respondents sampled for the Round 3 school survey who are also both part of the panel data set and part of the school survey panel. Regressions are OLS models with robust standard errors clustered at the EA level. All regressions are weighted to make them representative of the target population in the study EAs. Baseline values of the following variables are included as controls in the regression analyses: age dummies, strata dummies, household asset index, highest grade attended, and an indicator for never had sex. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

causing a downward bias in observed program impact. Baird and Özler (forthcoming) confirm this finding for Malawi and show that differential misreporting can further bias program impacts. As described in Section II.D, we tried to confirm self-reported enrollment by visiting the schools the girls reported to be enrolled in and asking their teachers about their enrollment statuses. Panel B in Table III reports the same information as Panel A, but as reported by the teachers.

The evidence in Panel B reverses the finding on the relative effectiveness of CCTs and UCTs in reducing dropout rates. First, we note that the dropout rates in the control group are higher by approximately 5–6 percentage points (pp), consistent with over-reporting by the core respondents. Furthermore, although dropout rates are still lower in both treatment arms than the control group, the impacts in the CCT arm are significantly larger than the UCT arm and the difference between the two treatment arms in terms of total number of terms enrolled during the 2-year intervention is significant at the 95% confidence level (p-value = .011). Finally, the impact of the CCT intervention seems to have persisted after the cash payments stopped at the end of 2009, while the enrollment rate in the UCT arm is identical to that in the control group during Term 1 of 2010.[35]

Given the divergent results using self-reports and teacher-reports, which set of findings should we believe? Baird and Özler (forthcoming) use administrative records from the CCT program to establish that the school ledgers collected independently in Round 3 provide a reliable source to measure attendance. Using attendance information from these school ledgers here as our benchmark, we can examine the extent of misreporting by the students and the teachers. Table IV presents this evidence. In column (1), we see that 17.0% of the girls in the control group who reported being enrolled in school during Term 2 of 2009 were found to have never attended school during that period according to the school ledgers. This likelihood to over-report enrollment is reduced by more than 50% (9.3 pp) in the CCT arm but is identical in the UCT arm. Column (2) shows that not only is over-reporting substantially reduced when the information

35. In Online Appendix C, we conduct a battery of exhaustive robustness checks and conclude that the findings presented in Table III are robust to varying the subsamples analyzed and the rules by which missing data for teacher-reported enrollment are treated.

TABLE IV
ANALYSIS OF REPORTING BIAS ON ENROLLMENT

	Dependent variable	
	(1)	(2)
	Core respondents over-reporting	Teachers over-reporting
Conditional treatment	−0.093*	−0.021
	(0.052)	(0.035)
Unconditional treatment	−0.001	−0.014
	(0.058)	(0.038)
Mean in the control group	0.170	0.052
Number of observations	325	325
Prob > F(Conditional = Unconditional)	0.02	0.79

Notes. Regressions are restricted to the subsample of core respondents who report being enrolled in school during Term 2, 2009, and have legible ledger data. Over-reporting refers to differences between the student (teacher) reports and the ledger. Regressions are OLS models with robust standard errors clustered at the EA level. All regressions are weighted to make them representative of the target population in the study EAs. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

comes from the teachers (5.2% in the control group), but also that the differential misreporting disappears.

The analysis explains the divergent findings in Table III: girls in both the control group and the UCT arm are significantly more likely than the CCT arm to report being enrolled in school when in fact they are not. Thus, self-reported data attenuate program impacts for the CCT arm and give the impression that UCTs outperform CCTs in reducing dropout rates. Teacher reports substantially reduce the bias caused by this differential misreporting and reveal the true program impacts. As we will see, the evidence regarding program impacts on school attendance and test scores are also consistent with the finding that the CCT arm was more effective in reducing school dropout than the UCT arm.

Schooling: Attendance (intensive margin). We turn to examining the intensity of attendance for those enrolled in school in 2009 and the first term of 2010. The school ledgers from Round 3 provide term-by-term information on the number of days the students were present for each day the school was in session.[36] Table V indicates that the attendance rate in the control group

36. Online Appendix Table B.2 confirms balance in this subsample of girls for whom attendance data from ledgers are available.

TABLE V
PROGRAM IMPACTS ON ATTENDANCE FROM SCHOOL LEDGERS

	Dependent variable: Fraction of days respondent attended school				
	(1)	(2)	(3)	(4)	(5)
	Term 1, 2009	Term 2, 2009	Term 3, 2009	Overall 2009	Term 1, 2010
Conditional treatment	0.139***	0.014	0.169**	0.080**	0.092**
	(0.045)	(0.033)	(0.085)	(0.035)	(0.041)
Unconditional treatment	0.063	0.038	0.118	0.058	−0.038
	(0.056)	(0.033)	(0.102)	(0.037)	(0.053)
Mean in the control group	0.778	0.849	0.688	0.810	0.801
Number of observations	284	285	192	319	211
Prob > F(Conditional = Unconditional)	0.129	0.334	0.358	0.436	0.010

Notes. Regressions are OLS models with robust standard errors clustered at the EA level. All regressions are weighted to make them representative of the target population in the study EAs. The variable "Overall 2009" is defined for all core respondents who have ledger information for any of the three terms and is constructed by dividing the number of days present by the number of days in session for all terms in which there is information. Baseline values of the following variables are included as controls in the regression analyses: age dummies, strata dummies, household asset index, highest grade attended, and an indicator for never had sex. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

among those enrolled in school ranges from a high of 85% in Term 2, 2009, to a low of 69% in Term 3, 2009. Attendance on the intensive margin is uniformly higher in the CCT arm than the control group. The overall attendance rate for 2009 is 8.0 pp higher in the CCT arm than the control group, which translates into approximately 4 school days per term or more than 10 school days over the entire 2009 school year. In the UCT arm, impact estimates are mostly positive, but none of them are statistically significant. Program impacts in the CCT arm are higher than the UCT arm during Term 1 in both 2009 (13.9 pp versus 6.3 pp; p-value = .129) and 2010 (9.2 pp versus. −3.8 pp; p-value = .010). Term 1 coincides with the lean season in Malawi, when food is scarcest and the number of malaria cases reaches its peak.[37] Thus, the condition to attend school regularly seems most effective

37. In 2001, the prevalence of malaria parasitemia among nonpregnant females, ages 15–19, was 24% (Dzinjalamala 2009). The same figure was 47% in school children. Malaria is a frequent cause of absenteeism in school, resulting in poor scholastic performance on the part of the student.

in keeping attendance rates high when households need cash the most.[38]

Test Scores. In Table VI, we present program impacts on tests of English reading comprehension, mathematics, and cognitive ability, which were administered to all study participants at their homes.[39] We see across-the-board improvements in test scores in the CCT arm, while no significant improvement can be detected in the UCT arm. The 0.140 SD improvement (p-value = .010) in English reading comprehension in the CCT arm is significantly

TABLE VI
PROGRAM IMPACTS ON TEST SCORES

	Dependent variable			
	(1)	(2)	(3)	(4)
	English test score (standardized)	TIMMS math score (standardized)	Non-TIMMS math score (standardized)	Cognitive test score (standardized)
Conditional treatment	0.140***	0.120*	0.086	0.174***
	(0.054)	(0.067)	(0.057)	(0.048)
Unconditional treatment	−0.030	0.006	0.063	0.136
	(0.084)	(0.098)	(0.087)	(0.119)
Number of observations	2,057	2,057	2,057	2,057
Prob > F(Conditional= Unconditional)	0.069	0.276	0.797	0.756

Notes. The cognitive test score is based on Raven's Colored Progressive Matrices. Math and English reading comprehension tests were developed based on the Malawian school curricula. Five questions (four from the Fourth Grade test and one from the eighth Grade test) from Trends in Mathematics and Science Study (TIMMS) 2007, which is a cycle of internationally comparative assessments in mathematics and science carried out at the fourth and eighth grades every 4 years, were added to the Math test. All test scores have been standardized to have a mean of 0 and a standard deviation of 1 in the control group. Regressions are OLS models using Round 3 data with robust standard errors clustered at the EA level. All regressions are weighted to make the results representative of the target population in the study EAs. Baseline values of the following variables are included as controls in the regression analyses: age dummies, strata dummies, household asset index, highest grade attended, an indicator for never had sex, and whether the respondent participated in the pilot phase of the development of the testing instruments. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

38. CCT households may have also taken additional measures to minimize school absence by having the girls sleep under bed nets: the share of girls who reported sleeping under a bed net the previous night was 10 pp higher (p-value = .049) in the CCT arm than the UCT arm in Round 3.

39. We presented evidence in Table I that there is no differential attrition between the CCT and UCT arms. Moreover, in the sample used for analysis in this article, that is, those who were successfully interviewed in all three rounds, only 30 girls have missing test scores (22 in the control group, 5 in the CCT arm, and 3 in the UCT arm). Even if all the controls were assigned the highest score for the English test and all treatment girls were assigned the lowest score, the findings on treatment impacts would not change.

higher than the program impact in the UCT arm at the 90% confidence level (p-value = .069). The CCT arm also has a 0.114 SD advantage over the UCT arm in the TIMMS math score, but this difference is not statistically significant.[40] Finally, in terms of cognitive ability, measured by Raven's colored progressive matrices, we see improvements of 0.174 and 0.136 SD in the CCT and UCT arms, respectively, compared with the control group. However, although the improvement in the CCT arm is statistically significant at the 99% confidence level, the impact estimate for the UCT arm is noisy and insignificant.

Summarizing the program impacts on schooling outcomes in the two treatment arms, we find that the CCT arm had significant gains in enrollment on the extensive margin, some improvement in attendance on the intensive margin, and modest gains in achievement in tests of English reading comprehension, mathematics, and cognitive skills.[41] Girls in the UCT arm were also significantly more likely to be enrolled in school compared with the control group, but there was no detectable improvement in their intensity of school attendance or their test scores. The increase in enrollment (measured by the total number of terms enrolled in school during the 2-year program) in the UCT arm was less than half of that achieved in the CCT arm. We conclude that CCTs outperformed UCTs in terms of improvements in schooling outcomes.

Marriage and Pregnancy. We now turn to an analysis of the prevalence of marriage and pregnancy in Rounds 2 and 3. Column (1) of Table VII shows that by Round 2, 4.3% of the initially never-married sample was married in the control group. Marriage rates were unchanged in the CCT arm but significantly lower in the UCT arm. By Round 3, the prevalence of marriage rose to 18.0% in the control group with an insignificant reduction of 1.2 pp in the CCT arm and a significant reduction of 7.9 pp (44%) in the UCT arm (column (2)). The differences in program impacts between

40. TIMMS stands for Trends in Mathematics and Science Study, which is a cycle of internationally comparative assessments in mathematics and science carried out at the fourth and eighth grades every 4 years. We borrowed five mathematics questions from the 2007 TIMMS (four fourth-grade and one eighth-grade question) and incorporated them into our independently developed mathematics test.

41. The improvements in scores is different than what has been previously reported in evaluations of other CCT programs. Behrman, Parker, and Todd (2009) and Filmer and Schady (2009) find no impacts of CCTs on tests of mathematics and language in Mexico and Cambodia, respectively.

TABLE VII
PROGRAM IMPACTS ON MARRIAGE AND PREGNANCY

	Dependent variable			
	(1)	(2)	(3)	(4)
	=1 if ever married		=1 if ever pregnant	
	Round 2	Round 3	Round 2	Round 3
Conditional treatment	0.007	−0.012	0.013	0.029
	(0.012)	(0.024)	(0.014)	(0.027)
Unconditional treatment	−0.026**	−0.079***	−0.009	−0.067***
	(0.012)	(0.022)	(0.017)	(0.024)
Mean in the control group	0.043	0.180	0.089	0.247
Number of observations	2,087	2,084	2,086	2,087
Prob > F(Conditional = Unconditional)	0.024	0.025	0.265	0.003

Notes. The dependent variables are having been ever married or ever pregnant at the time of the relevant survey. Regressions are OLS models with robust standard errors clustered at the EA level. All regressions are weighted to make them representative of the target population in the study EAs. Baseline values of the following variables are included as controls in the regression analyses: age dummies, strata dummies, household asset index, highest grade attended, and an indicator for never had sex. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

the two treatment arms in Rounds 2 and 3 are both statistically significant at the 95% confidence level. Columns (3) and (4) show that although the program had little effect on the prevalence of pregnancy in either treatment arm after 1 year, there is a large decline in the UCT arm by Round 3. While roughly a quarter of the control group and the CCT arm had been ever pregnant by the end of the experiment, this likelihood was reduced by 6.7 pp (or 27%)in the UCT arm—significant at the 99% confidence level. The difference in program impacts between the two treatment arms is also significant at the 99% confidence level by Round 3.

How do we reconcile the fact that the CCT arm led to schooling increases with the fact that the sharp decreases in marriage and pregnancy are found only among the UCTs? Existing evidence from Sub-Saharan Africa suggests that reducing school dropout should lead to declines in teen marriage and pregnancy rates (Osili and Long 2008; Ferré 2009; Duflo, Dupas, and Kremer 2011; Ozier 2011). There could also be an income effect on marriage if girls are marrying early due to poor economic circumstances (Field and Ambrus 2008) and on fertility if monthly transfers allow girls to reduce transactional sex with sugar daddies (Dupas 2011).

To understand the differential impacts of CCTs and UCTs on any outcome other than schooling itself, it is useful to consider

three latent strata of baseline schoolgirls. In the first stratum, UCT offers would be sufficient to keep baseline schoolgirls enrolled in school (*UCT compliers*).[42] This group would receive transfers under both treatments, so program impacts could only differ due to behavior changes on the intensive margin arising from the conditionality. In the second stratum are girls who would be enrolled in school in the CCT arm but not in the UCT arm (*CCT compliers*). In this group, which would also receive transfers under both treatments, conditionality would generate a differential impact on enrollment by lowering the opportunity cost of schooling in the CCT arm. Finally, in the third stratum are girls who would drop out of school under either treatment arm and would receive transfers only under the UCT (*noncompliers*).

The differential impacts of CCTs and UCTs on outcomes other than schooling, such as marriage and pregnancy,will depend on the relative sizes of these three strata and the extent to which the relevant outcomes are driven by schooling and income. We would expect CCTs to be more effective for outcomes such as test scores, which would likely improve with schooling (and the incentives associated with schooling) and are not likely to be affected by increased income among dropouts. Conversely, UCTs may be more effective for other outcomes if there is a large group of noncompliers, a strong income effect among them, and small incentive effects among those enrolled in school. In the analysis that follows, we apply this simple framework to our data and show that in our setting, marriage is a perfect example of the latter case.

A simple way of presenting the sizes of these three strata and the approximate magnitude of the effects within each of them is provided in Table VIII, which gives marriage rates by treatment status and teacher-reported enrollment status in Round 3.[43] For each treatment arm, the top row summarizes the marriage rates

42. This group includes those who would be enrolled in school in the absence of either treatment. We assume that UCT offers have no perverse effects on schooling, meaning that an offer of UCTs would not cause a girl to drop out of school if she would be enrolled otherwise. We make a similar no defiers assumption with respect to the conditionality: if a girl would be enrolled in school under a UCT, then she would also be enrolled under a CCT offering the same transfer amount.

43. The three strata are defined over latent choices, so Table VIII presents instead compliance and marriage rates as actually observed in the control group and each treatment arm in Round 3. This gives us the unadjusted size of each stratum (UCT compliers are 61%, CCT compliers are 8%, and noncompliers are 31%), but does not tell us which individuals belong to which stratum.

TABLE VIII

PREVALENCE OF BEING EVER MARRIED BY SCHOOL ENROLLMENT STATUS DURING TERM 1, 2010

	(1)	(2)	(3)
	Enrolled	Not enrolled	Total
Control, %	1.7	46.9	19.9
(row %)	(59.8)	(40.2)	(100.0)
Conditional treatment, %	0.5	50.8	16.0
(row %)	(69.2)	(30.8)	(100.0)
Unconditional treatment, %	0.3	25.2	10.1
(row %)	(60.5)	(39.5)	(100.0)
Total, %	1.1	44.2	17.2
(row %)	(62.7)	(37.3)	(100.0)

Notes. This table presents the marriage rates by Round 3 enrollment status in Term 1, 2010 and treatment status. For each treatment arm, the top row summarizes the marriage rates by follow-up enrollment status, and the bottom row shows the raw follow-up row percentage in each cell. Means are weighted to make them representative of the target population in the study EAs.

by follow-up enrollment status, and the bottom row shows the raw follow-up enrollment rate in that group.[44]

The first thing to note in Table VIII is that on average, only 1.1% of those still enrolled in school are married (column (1)). This confirms the notion that marriage and school enrollment are practically mutually exclusive in this setting and implies that CCTs cannot generate a differential impact on marriage among UCT compliers.[45] Second, a comparison of marriage rates in the CCT arm and the control group among those not enrolled in school confirms the prediction that CCTs are unlikely to have an impact among noncompliers as girls in this group stop receiving transfers when they drop out of school (column (2)). Approximately half of the girls not enrolled in school were married in both the control group and the CCT arm in Round 3.

44. For expositional purposes, it is easiest to present marriage and enrollment rates at the time of Round 3 data collection, that is, during Term 1 of 2010. The findings from the analysis that follows remain qualitatively the same if we use enrollment data for any school term in 2008 or 2009, utilize marriage rates reported in Round 2, or replace marriage with pregnancy.

45. Ozier (2011), summarizing the literature on schooling and fertility, states that they are in practice nearly mutually exclusive in Kenya and argues that this is true in many other contexts.

These two simple observations imply that the only way CCTs can reduce marriage rates at follow-up is by averting dropouts. Column (1) in Table IX shows that school dropout was 5.8 pp lower in the CCT arm than the control group. Column (2) shows that the reduction in the likelihood of being ever married in the CCT arm is 2.6 pp in this subsample of girls for whom data on teacher-reported enrollment are available in Round 3. This small CCT effect on marriage is exactly what we would expect to find given the roughly 50 pp difference in marriage rates between girls enrolled in school and those not enrolled ($-0.058 * 0.5 = -0.029$). We conclude that the program impact on dropout in the CCT arm was too small to cause a significant reduction in the likelihood of being married.

The UCT arm presents a contrasting case, in which the program impacts on marriage and fertility arise only through changes in behavior among those who dropped out of school. Table VIII shows that the raw marriage rate in the UCT arm among those not enrolled in school is approximately 50% lower than the CCT arm and the control group. In Table IX, we see

TABLE IX

TEACHER-REPORTED SCHOOL ENROLLMENT AND MARITAL STATUS IN ROUND 3

	Dependent variable			
	(1)	(2)	(3)	(4)
	=1 if enrolled term 1 2010	=1 if ever married	=1 if ever married	=1 if ever married
	All	All	Enrolled	Not enrolled
Conditional treatment	0.058*	−0.026	−0.012	0.033
	(0.034)	(0.037)	(0.015)	(0.097)
Unconditional treatment	−0.000	−0.088***	−0.011	−0.159**
	(0.036)	(0.030)	(0.010)	(0.067)
Mean in the control group	0.598	0.199	0.017	0.469
Sample size	844	844	490	354
Prob > *F*(Conditional = Unconditional)	0.099	0.106	0.857	0.088

Notes. The first two columns are impact regressions on Term 1, 2010 *teacher-reported* enrollment and being ever married by Round 3, respectively. Columns (3) and (4) present regressions of being ever married among those who were enrolled and not enrolled during Term 1, 2010, respectively. Regressions are OLS models with robust standard errors clustered at the EA level. All regressions are weighted to make them representative of the target population in the study EAs. Baseline values of the following variables are included as controls in the regression analyses: age dummies, strata dummies, household asset index, highest grade attended, and an indicator for never had sex. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

that the UCT impact on marriage was −8.8 pp, while there was no impact on dropping out of school. Hence, there can be no schooling channel operating on marriage and the decline in the UCT arm is entirely due to the effect of cash transfers among those who dropped out of school. The final two columns in Table IX present regressions of being ever married for those enrolled in school and those who dropped out, respectively, and shows that the marriage rate among those who dropped out is significantly lower in the UCT arm compared with both the control group and the CCT arm.[46]

The evidence presented here is consistent with the literature that posits female schooling as a means of delaying marriage and fertility, but the product of the treatment effect on dropping out in the CCT arm (5.8 pp) and the sizable effect of school enrollment on marriage (reduced by nearly 50 pp) is too small to be detectable given our sample size.[47] The novel finding here is the large impact of UCTs on delaying marriage and pregnancy among adolescent girls who dropped out of school. The effect of UCTs on marriage among this group (reduced by more than 20 pp) is smaller than the schooling effect on marriage, but experienced in a large enough subgroup (40% of the UCTs had dropped out by Round 3) to be strongly significant in the whole UCT arm. The schooling channel on marriage operates through the dropouts averted by the CCT arm, while the income effect on marriage operates through those who dropped out of school; the latter group is substantially larger than the former in this experiment.

Heterogeneity of Program Impacts. We now turn to an examination of impact heterogeneity, using only covariates that were used to stratify our initial study design (age) or were directly randomized (the transfer amounts made to the girls and their

46. It should be noted that these are not proper subsamples on which experimental program impacts can be estimated because they are constructed using enrollment status at follow-up, which is endogenous to treatment. We only present these regressions to give a sense of the magnitude and the statistical significance of the raw differences in Table VIII.

47. To confirm the potential importance of the schooling channel, note that Baird et al. (2010) show that CCTs had a significant impact on reducing marriage and pregnancy rates among baseline dropouts by 11.3 pp and 5.1 pp, respectively. In this sample the CCT led to a very large increase in self-reported reenrollment in comparison with the control group (61% versus 17%), and hence the impact through the schooling channel is detectable.

TABLE X

HETEROGENEITY OF PROGRAM IMPACTS BY AGE GROUP AT BASELINE

	Dependent variable			
	(1)	(2)	(3)	(4)
	Total number of terms enrolled (school survey)	Standardized English test score	=1 if ever married	=1 if ever pregnant
Conditional treatment	0.467***	0.141*	−0.023	−0.008
	(0.159)	(0.073)	(0.017)	(0.028)
Unconditional treatment	0.257	−0.116	−0.051**	−0.059***
	(0.157)	(0.102)	(0.020)	(0.020)
=1 if Over 15	−0.786***	−0.546***	0.122***	0.176***
	(0.244)	(0.058)	(0.026)	(0.027)
Conditional treatment * Over 15	0.290	0.017	0.037	0.104*
	(0.291)	(0.089)	(0.056)	(0.054)
Unconditional treatment * Over 15	0.103	0.245**	−0.067	−0.032
	(0.255)	(0.110)	(0.042)	(0.046)
Number of unique observations	852	2,057	2,084	2,087
Prob > *F*(Conditional = Unconditional)	0.095	0.031	0.188	0.067
Prob > *F*(Conditional * Older = Unconditional * Older)	0.364	0.059	0.097	0.027

Notes. An indicator variable is constructed that takes on a value of 1 if the core respondent was older than fifteen at baseline, and is 0 otherwise. Regressions are OLS models using Round 3 data with robust standard errors clustered at the EA level. Baseline values of the following variables are included as controls in the regression analyses: an indicator for being over 15, strata dummies, household asset index, highest grade attended, and an indicator for never had sex. All regressions are weighted to make the results representative of the target population in the study EAs. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

parents).[48] Table X examines heterogeneity using an indicator for being 16 years or older.[49] With respect to enrollment, program impacts do not vary significantly by age group: CCTs outperform UCTs in raising enrollment among early adolescents as well as

48. The study sample was stratified by age group at baseline as program impacts on enrollment, marriage, and pregnancy can reasonably vary along this dimension. Transfer amounts made separately to girls and their parents were also randomized to assess whether transfer size or the identity of the recipient within the household—two key policy parameters in cash transfer programs—affected the outcomes. We limit ourselves to examining heterogeneity of program impacts only along these baseline characteristics that were defined ex ante to avoid data mining.

49. The legal age of marriage stood at 16 in Malawi by late 2009 (Nyasa Times September 22, 2009) and many girls aged 16 or older are attending primary or secondary school. In our study sample, 22% of students eligible to attend primary school at baseline were aged 16 or older. This share increases to 56% for those eligible to attend secondary school at baseline.

older teenagers. However, when it comes to the other three main outcomes examined in this study, we see that the advantage in English test scores in the CCT arm among early adolescents disappears among girls 16 or older at baseline, whereas the advantage the UCT arm has in preventing marriages and pregnancies is substantially larger among older teenagers. For each of these three outcomes, the difference in the coefficients for the interaction terms between the CCT and UCT arms is large and statistically significant. Our results indicate that among early adolescents, there is a clear trade-off between a CCT and a UCT program in terms of improved schooling outcomes versus a reduction in teenage pregnancies. Among older teenage girls, however, this trade-off largely disappears and UCTs become relatively more attractive.[50]

We next examine the heterogeneity in program impacts by two important program features: the identity of the transfer recipient within the household and transfer size. Bursztyn and Coffman (2011) argue that policies designed to promote school attendance might be more effective if they target the child instead of focusing on parents because suboptimal school attendance may be due to a parent–child conflict, where the parents cannot enforce their desire that their children attend school. Similarly, World Bank (2009) argues that "the key parameter in setting benefit levels is the size of the elasticity of the relevant outcomes to the benefit level." However, random variation in these design parameters is rarely observed in cash transfer programs around the world. In this experiment, separate transfers were offered to girls and their parents (or guardians), the size of each of which were randomly determined.

We analyze the impact of transfer amounts by estimating the following linear probability model:

$$(2) \quad Y_i = I_i^C \delta^C + I_i^U \delta^U + H_i^C \phi^C + H_i^U \phi^U + T_i^C \gamma^C + T_i^U \gamma^U + X_i \beta + \varepsilon_i.$$

T_i^C and T_i^U are again dummy variables for the CCT and UCT offers, I_i^C, I_i^U and H_i^C, H_i^U give the individual and household transfer amounts for each treatment, defined in differences from the lowest

50. We have also examined heterogeneity of program impacts using a household asset index to imitate the means-tested targeting schemes like the ones in Brazil or Mexico. In Online Appendix Table D.1, we show that enrollment impacts in the UCT arm may improve somewhat under such a targeting scheme, but otherwise program impacts would not be significantly altered compared with a universal cash transfer program for never-married adolescent girls.

amount offered in the treatment arms ($1 for the individual transfer, $4 for the household transfer). The estimates of δ and ϕ thus give the marginal effect of increasing individual and household amounts by $1 under each treatment arm, and the estimates of γ measure the impact of each treatment at the lowest total transfer amount. The standard errors ε_i are again clustered at the EA level.

Table XI presents the heterogeneity in program impacts by transfer amounts. Column (1) shows program impacts on enrollment: the minimum total transfer amount offered to the household ($5/month) seems to be responsible for the entire program impact on the total number of terms enrolled in the CCT arm. Additional transfers offered to either party make little difference. In the UCT arm, however, the effect of the minimum transfer offer is small and insignificant, but enrollment increases 0.081 terms with each additional dollar offered to the parents over and above $4/month.[51] The analysis implies that giving an additional $5/month in transfers to the parents would barely allow the UCT arm to reach the level of enrollment attained by the minimum transfer amounts in the CCT arm. As the marginal administrative cost of a CCT program is likely to be only a small share of each dollar transferred to beneficiary households, a CCT program offering $5/month would be substantially more cost-effective in increasing enrollment than a UCT program offering the same amount.

For English test scores, the results are similar for the CCT arm: there is no indication that additional amounts to the girls or their parents would improve test scores over and above the minimum monthly transfer. Here, unlike enrollment, the coefficient in the UCT arm is similar to the CCT arm at the minimum transfer amounts, although insignificant. For marriage, there is no treatment impact at the minimum transfer amounts in the UCT arm, but each additional dollar offered to the parents of a girl reduces her likelihood of getting married by Round 3 by 1.7 pp.[52] The marginal effect of a dollar offered to parents in delaying marriages is significantly higher in the UCT arm than that in the CCT arm (p-value = .069). The large negative and statistically significant gradient between transfer size and likelihood

51. The difference between the CCT and UCT impacts on enrollment at the minimum transfer amount ($5/month total to the household) is significant at the 95% confidence level.

52. Similarly, each additional dollar offered to the girl in the UCT arm reduces her chances of being ever married at follow-up by 1.6 pp (p-value = .148).

TABLE XI

IMPACTS OF HOUSEHOLD AND INDIVIDUAL TRANSFER AMOUNTS

	Dependent variable			
	(1)	(2)	(3)	(4)
	Total number of terms enrolled (school survey)	Standardized English test score	=1 if ever married	=1 if ever pregnant
Conditional treatment, individual amount	0.024	−0.032	−0.002	0.006
	(0.051)	(0.029)	(0.008)	(0.012)
Unconditional treatment, individual amount	−0.048	−0.019	−0.016	0.013
	(0.064)	(0.038)	(0.011)	(0.013)
Conditional treatment, household amount	−0.027	−0.000	0.001	0.005
	(0.035)	(0.016)	(0.007)	(0.010)
Unconditional treatment, household amount	0.081***	−0.058**	−0.017**	−0.002
	(0.031)	(0.029)	(0.007)	(0.009)
Conditional treatment, minimum transfer amounts	0.572***	0.202*	−0.011	0.001
	(0.213)	(0.118)	(0.044)	(0.052)
Unconditional treatment, minimum transfer amounts	0.094	0.175	0.001	−0.089*
	(0.167)	(0.132)	(0.040)	(0.050)
Number of unique observations	852	2,057	2,084	2,087
Prob > F(Conditional = Unconditional), individual amount	0.390	0.788	0.300	0.702
Prob > F(Conditional = Unconditional), household amount	0.025	0.082	0.069	0.614
Prob > F(Conditional = Unconditional), minimum amount	0.046	0.877	0.834	0.203

Notes. Regressions are OLS models using Round 3 data with robust standard errors clustered at the EA level. Baseline values of the following variables are included as controls in the regression analyses: age dummies, strata dummies, household asset index, highest grade attended, and an indicator for never had sex. All regressions are weighted to make the results representative of the target population in the study EAs. Parameter estimates statistically different than 0 at 99% (***), 95% (**), and 90% (*) confidence.

of marriage at follow-up is consistent with parents marrying off their daughters early due to economic considerations. Finally, the minimum amounts transferred in the UCT arm seem to be responsible for almost the entire program effect on preventing pregnancies in this group.

In summary, increasing transfer amounts or varying the recipient within the household has no effect on any of the outcomes examined in this article in the CCT arm; this contract variation simply does not seem to matter for CCTs.[53] In contrast, we find that outcomes vary with increased transfer offers to the parents in the UCT arm: enrollment rates increase and the incidence of marriage declines as parents are offered more money, but performance in test scores seems to suffer. Still, however, replacing a CCT program that offers the minimum transfer amounts of $1 to the girl and $4 to her parents with a UCT program that offers the parents larger transfer amounts would not be cost-effective in improving schooling outcomes, but it would reduce marriage rates among teenage girls. Furthermore, we find no evidence that increasing the amount of transfers made directly to the girl rather than her parents would be effective in improving any of the outcomes studied here.

Before we move to the concluding section, we briefly discuss the robustness of our findings with respect to two issues. First, treatment and conditionality status were assigned at the EA level in this experiment. Due to the proximity of EAs to each other, it is possible that intermingling students in the two treatment arms led to a change in behavior in the outcomes of interest, thus biasing our estimates of the marginal impact of the conditionality. Second, as described in Section II, the offers in the CCT arm included a promise to pay secondary school fees directly to the schools on confirmation of enrollment by the program administrators. To make the average transfer offers in the UCT arm equal to that in the CCT arm, the average school fee amount was added to the cash transfer offers of girls in the UCT arm who were eligible to attend secondary school at the beginning of the program. In Online Appendix E, we discuss these issues in

53. This finding is consistent with the evidence presented in Filmer and Schady (2011), who found substantial impacts on enrollment of a modest CCT offer in Cambodia, but no additional enrollment gains from a larger transfer offer. On the other hand, Barrera-Osorio, Linden, and Urquiola (2007) find enrollment responses of students to be sensitive to the size of the implied subsidies in Bogotá Gratuidad school fee reduction initiative.

some detail and conclude that it is unlikely that our estimates of differential program impacts are influenced by either spillovers due to the proximity of girls with discordant treatment statuses or by the manner in which school fee compensation was handled.

IV. Concluding Discussion and Policy Implications

This article presented experimental evidence on the relative effectiveness of conditional and unconditional cash transfer programs. The analysis focused on two sets of outcomes that are of central importance to the long-term prospects of school-age girls: schooling and human capital formation on the one hand, marriage and fertility on the other. The results show that CCTs increased enrollment rates and improved regular attendance for those in school, both of which likely contributed to a modest but significant improvement in English test scores over the UCT arm. Teenage pregnancy and marriage rates, on the other hand, were substantially lower in the UCT than the CCT arm.

The results on school enrollment differ from previous studies that considered the relative effectiveness of UCTs versus CCTs, although the difference is a matter of degree rather than direction. Analyses of Bolsa Escola in Brazil and PROGRESA in Mexico found that UCTs would have little, if any, impact on school enrollment—implying that almost all of the impacts of these programs were due to the schooling condition (Bourguignon, Ferreira, and Leite 2003; Todd and Wolpin 2006). In the Malawi experiment, on the other hand, we find a modest impact on enrollment in the UCT arm,which is 43% as large as the impact in the CCT arm. Our study thus confirms that conditions attached to cash transfer programs are effective in increasing enrollment, but the size of this effect is likely to be smaller than suggested by earlier studies, at least for poorer countries like Malawi.

Not only is school enrollment significantly improved in the CCT arm over the UCT arm, but the evidence presented shows that CCTs are more cost-effective in raising enrollment than UCTs in this context. To achieve the same enrollment gain obtained from a $5/month total transfer in the CCT arm, a transfer of more than $10 to the parents in the UCT arm is needed. This difference is much larger than the additional cost of administering a CCT program—possibly by an order of magnitude.[54] Further-

54. World Bank (2009) cites Grosh et al. (2008) to report that the median administrative cost is 8% of total program costs in 10 CCT programs for which

more, the average number of payments in the CCT arm was approximately 14.1 (out of a possible total of 20 over 2 years), compared with 17.9 in the UCT arm, implying that the actual amount of transfers made per person was 19% lower in the CCT arm over the 2-year intervention. Savings of this magnitude due to noncompliance with the schooling condition would more than make up for the additional administrative cost of monitoring in most programs.

Although CCTs were more cost-effective than UCTs in increasing school enrollment and attendance, they had little effect on reducing the likelihood of teenage pregnancies or marriages. By contrast, UCT offers were very effective in delaying marriage and childbearing—by 44% and 27%, respectively, after 2 years. These impacts in the UCT arm were experienced almost entirely among those who dropped out of school after the start of the 2-year intervention, whereas the likelihood of marriage and pregnancy were negligible among those who stayed in school regardless of treatment status.

When we examine the random variation in transfer amounts to girls and their parents, we find that this contract variation did not alter outcomes in the CCT arm. This finding is encouraging for policy makers because it implies that the smallest transfer amounts ($4/month to the parents and $1/month to the school-age girl) offered in this experiment were sufficient to attain the average schooling impacts observed under the CCT arm. Furthermore, our results suggest that reallocating some of the transfers from the parents to the girls would not improve program impacts under either treatment. Hence, while the idea of making part of the transfers directly to adolescent girls may be attractive on its face, we find no evidence that it would be effective.[55]

The experiment highlights the underlying causes of school

administrative cost data were compiled by that study. Given that monitoring compliance with the schooling condition is only part of the administrative costs, the additional cost would be a few cents on the dollar. However, monitoring and enforcement of conditionality in many of these programs may not have been as thorough as they were in our experiment, which could reduce the relative effectiveness of CCTs compared with UCTs.

55. The readers should note, however, that there was no cell in the experiment in which the transfer to the girl (or her parents) was equal to 0. It is possible that even a $1/month transfer made directly to the girls was more effective than adding that small amount to the transfers made to the parents. Our results are clear that marginal effect of reallocating any amount above the $1/month given to the girl would not improve outcomes.

dropout, marriage, and pregnancy in the Malawian context. First, the fact that the UCT has some effect on school enrollment indicates that poverty is a cause of school dropout in this population, and that poor parents will invest at least some of the additional funds from a positive income shock toward the education of their daughters. The reduction in marriage and pregnancy rates in the UCT arm, as well as the responsiveness of marriage to unconditional transfer amounts, seems consistent with the idea that adolescent girls who drop out of school undergo a rapid transition into adulthood that is also strongly influenced by economic circumstances.[56]

The informal framework we introduced in Section III.C is helpful in considering the generalizability of differential program impacts on marriage and pregnancy found in this study. A necessary condition for UCTs to delay marriage and pregnancy more effectively than CCTs is the presence of a large group of noncompliers (relative to the group of CCT compliers), a finding that is confirmed in most evaluations of CCT programs around the world.[57] Given a large stratum of noncompliers, UCTs may be more effective in delaying marriage and pregnancy if there are significant beneficial income effects among this group. In southern and eastern Africa, where these outcomes are more or less mutually exclusive with schooling and decisions regarding sexual behavior and marriage among adolescent girls are influenced by poverty, UCTs may indeed be more effective than schooling CCTs in reducing teenage marriage and pregnancy. In countries like Bangladesh, where dropout and marriage rates among adolescent girls are also high but, unlike Sub-Saharan Africa, dowry pay-

56. In our study sample, approximately 25% of the young women who were sexually active at baseline reported that they started their sexual relationships because they "needed his assistance" or "wanted gifts/money."

57. Most studies examining school dropout in CCT programs find that dropout rates are reduced by less than 50%, implying that the stratum of noncompliers is usually larger than the stratum of CCT compliers. For example, Behrman, Sengupta, and Todd (2005) report that, among 15-year-olds enrolled in school before the start of PROGRESA, the dropout rate in the treatment group was 31.3%, while the program impact on reducing dropouts was 6.4 pp. These numbers are remarkably similar to those presented in Table IX. Even in countries like Cambodia, where the program impacts on enrollment were very large in absolute terms at approximately 25 pp, the reduction in the dropout rate was less than 50% as the enrollment rate in the control group was 44% (Filmer and Schady 2011). It should be noted that program impacts on dropout rates reported in studies of CCT programs are upper bounds for the size of the CCT complier group as there were no counterfactuals for a UCT intervention.

ments are made from the bride's family to the groom's, UCTs may have no effect, or perhaps even the opposite effect, on the timing of marriage. In such settings, CCTs for schooling or for staying unmarried may be more effective than UCTs.[58]

Our framework also allows us to consider the relative merits of CCT and UCT programs on outcomes other than schooling, marriage, and pregnancy. CCTs are likely to be more effective in improving outcomes that may be strongly affected by compliance with the conditions, such as test scores. UCTs may be preferred if there are many noncompliers who might experience strong and socially beneficial effects from regular income support. If noncompliers can be thought of as a vulnerable group in a given context, UCTs may deserve careful consideration given the possible trade-offs indicated in this study. Though similar experiments would be useful in determining the extent to which our findings generalize to other contexts, in the absence of such an experiment, policy makers can use evidence from previous evaluations of CCT programs to estimate the sizes of the relevant strata and use existing household survey data to predict the relative effects of schooling and income on outcomes of interest within each stratum.

Whatever the overall merits of the two transfer schemes, this study found evidence that schooling CCTs are a much more cost-effective means of reducing dropouts than are UCTs. However, in the absence of a market failure, such a distortion is inefficient.[59] Policy makers planning to implement a CCT program should clearly articulate the market failures behind this paternalistic motivation and, if possible, provide evidence of these externalities—private or social. For example, we have not found any evidence of incomplete altruism—that is, a conflict of interest between the girls and their parents with respect to her education, which is sometimes mentioned as a justification of a preference for CCTs over UCTs.

58. Such an intervention is being currently evaluated in Bangladesh, where girls between 15 and 17 years old living in treatment areas are offered cooking oil on the condition that they remain unmarried. The amount of oil offered is designed to be larger than the cost of delaying marriage, which comes in the form of increased dowry payments from the bride's family to the groom's: http://www.povertyactionlab.org/evaluation/empowering-girls-rural-bangladesh.

59. In this context, credit market failures would not be a justification for CCTs as UCTs would be sufficient to relax credit constraints. Another market failure, such as a lack of information, hyperbolic discounting, or positive spillover effects, is necessary to justify preferring CCTs over UCTs.

CCT programs create incentives for individuals to change their behaviors by denying transfers to those who fail to satisfy the conditions. However, at least some of these individuals come from vulnerable households and are also in need of income support. Our findings suggest that UCTs to such households can improve important outcomes even though they are not as successful in improving schooling outcomes as CCTs. This study makes clear that while CCT programs may be more effective than UCTs in obtaining the desired behavior change, they can also undermine the social protection dimension of cash transfer programs.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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