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Girl Power

Cash Transfers and Adolescent Welfare: Evidence from a Cluster-Randomized Experiment in Malawi

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5.1 Introduction

Adolescent girls are a key demographic target group to successfully break the cycle of poverty in developing countries (Levine et al. 2008). In Malawi, the focus of this chapter, the population of fifteen to nineteen-year-old girls is forecast to grow by 66.9 percent from 2005 to 2020, making its projected growth rate the third highest in the world (Warhurst, Molyneux, and Jackson 2010).[1] Interventions that help adolescent girls reach their full potential not only bring immediate benefits to their own lives, but also longer-term benefits to their offspring and communities at large (Lloyd 2009; Duflo 2012). This message is the focus of organizations that favor social inter-

1. As of 2005, there were 849.6 million girls age ten to twenty-four in the world, comprising 13 percent of the global population. Between 2005 and 2020, the population of girls age ten to twenty-four years is forecast to grow by 5.1 percent (Warhurst, Molyneux, and Jackson 2010).

ventions targeted at young women, such as the Coalition for Adolescent Girls founded by the United Nations Foundation and the Nike Foundation.[2]

This chapter examines whether a cash transfer program targeted at adolescent girls in Malawi helped empower its recipients in the short run, that is, during and immediately after the two-year intervention. The Zomba Cash Transfer Program (ZCTP) was a randomized intervention that provided initially never-married females ages thirteen to twenty-two with financial support in the form of monthly cash transfers for two academic years. The intervention had two treatment arms, one where cash was given conditional on regular school attendance, the Conditional Cash Transfer (CCT) arm, and one that transferred cash unconditionally, the Unconditional Cash Transfer (UCT) arm. The program was not only targeted to families of eligible young females as described above, but also, unlike in almost all CCT programs, part of the monthly transfer was given directly to the girl. Giving transfers to girls directly can make the transfers "stick" to them, potentially leading to larger impacts (see Muralidharan and Prakash [2013] for strong enrollment effects of a program that gave girls bicycles in India). Furthermore, as in BRAC's Empowerment and Livelihoods for Adolescents (ELA) program in Uganda, girls who had already dropped out of school were part of the target population under ZCTP, allowing for the identification of impacts separately on this potentially vulnerable group. The target population and the experimental variation in treatment make the ZCTP an ideal intervention within which to evaluate the impact of cash transfers on the empowerment of adolescent girls.[3]

However, it is important to note that the notion of empowerment adopted here for this group of young and largely never-married females at the end of the two-year intervention is different than what we would consider if the target population were mostly married adult women. As is powerfully summarized in Duflo (2012), many academics and policymakers are interested in women's decision-making power within their households, their bargaining power within their marriages, and their voice and political power within their communities. As the study population here are initially never-married adolescent girls, the vast majority of whom still lived with their parents (or another guardian) at the end of the two-year program, outcomes pertaining to bargaining power within marriage, investments in own children, or involvement in local politics are too early to measure. Potential program impacts on these outcomes are the focus of future work using longer-term follow-up data. Here, we focus on the empowerment of adolescent girls and

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2. See, for example, "The Girl Effect," a YouTube sensation that advocates for "the powerful social change brought about when girls have the opportunity to participate." http://www.youtube.com/user/girleffect.

3. We discuss the details of these two different treatment arms in more detail in section 5.2.

summarize program impacts for a wide and rich set of outcomes during and immediately following the completion of the cash transfer experiment.

Empowerment, as a concept, is hard to pin down. Kabeer (1999, 435) defines empowerment as "the process by which those who have been denied the ability to make strategic life choices acquire such . . . ability." Essentially, Kabeer (1999) argues that two elements, resources and agency,[4] determine an individual's ability to exercise choice. Resources can broadly be defined as access and future claims to physical and human resources that are instrumental in making important choices in life. Agency is "people's actual capacity to define their own life-choices and to pursue their own goals" (Kabeer 1999, 435). Agency includes both internal cognitive processes such as reflection and analysis and the social processes of bargaining, negotiation, manipulation, norms, and conventions. Ultimately, improved ability to exercise choice (as a result of enhanced resources and agency) can affect day-to-day functioning of the individual and her family members (for instance, in terms of health status, nutritional intake, and time use).

While the literature approaches empowerment from multiple angles, it can largely be interpreted within the framework of Kabeer (1999) as investigating the impact of enhanced resources or agency on subsequent functioning. In economics, the empirical literature largely focuses on policies or programs that increase a woman's bargaining power within marriage, both improving own outcomes as well as those of her children. For example, policies such as the old-age pension in South Africa (Duflo 2003) and extended alimony rights in Brazil (Rangel 2006) had beneficial impacts on the health and education of the female children of beneficiaries. In addition, there is some evidence that female-targeted interventions improve outcomes for women: a switch from a tax credit to a direct payment to the mother for child benefits was associated with an increase in the consumption of women's and children's clothing (Lundberg, Pollak, and Wales 1997); a savings product in the Philippines improved women's influence on household decisions (Karlan, Ashraf, and Yin 2007); a microcredit program in Bangladesh increased the female beneficiaries' financial resources and mobility (Pitt, Khandker, and Cartwright 2006); a community-level education program for women improved employment and empowerment outcomes (Kandpal, Baylis, and Arends-Kuenning 2013); and Oportunidades, a Mexican CCT scheme, empowered women by encouraging them to negotiate better care from health care providers (Barber and Gertler 2010).[5] More relevant for

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4. Sen (1999) refers to this as capabilities.

5. While giving women more power is on many occasions both efficiency and equity enhancing, Duflo (2012) notes that women and men have different preferences and women's preferences are not always more benevolent than men's and their decisions are not always more favorable to well-being and development. For example, girls age birth to five years benefited from old-age pensions given to women but not to men, and there was no effect among boys for

the target population of adolescent girls under examination here, Bandiera et al. (2015) find that an intervention that combined vocational training with information on health and risky behaviors led to increases in income-generating activities and decreases in sexual activity and pregnancies among females ages fourteen to twenty in Uganda.

Within the theoretical economics literature, the discourse on female empowerment focuses on shifts in the balance of power within married couples in favor of the woman. A shift in the balance of power can take different forms, such as an increase in the woman's education, an increase in her earning capacity, or her improved access to birth control technologies. Such shifts can result in the reallocation of resources within the household toward the woman (Chiappori, Iyigun, and Weiss 2009; Chiappori and Oreffice 2008), both increasing her welfare and perhaps leading to a reduction in total fertility and even child mortality rates (Iyigun and Walsh 2007; Eswaran 2002). Interestingly, the theoretical empowerment literature suggests that the determinants of empowerment within a relationship are to a large extent shaped earlier in life. Recent research finds positive impacts on adult life outcomes from child sponsorship programs (Wydick, Glewwe, and Rutledge 2013) and a potential mechanism may be higher levels of self-esteem, educational, and occupational aspirations (Glewwe, Ross and Wydick 2013). Hence, theory and some empirical evidence suggests that influencing the resources and agency of females at a young age may lead to improved outcomes in the future—privately and socially. Our study adds to this literature by summarizing the short-term effects of a two-year cash transfer intervention targeted explicitly at never-married females on a broad set of outcomes related to empowerment.

Our discussion first focuses on empowerment impacts of CCTs, before turning to a brief comparison of them with UCTs. The reason for this approach is that the CCT experiment was conducted in two strata—girls who were in school at baseline (baseline schoolgirls) and girls who had already dropped out of school at baseline (baseline dropouts). The UCT intervention, on the other hand, was only conducted among baseline schoolgirls. Previous studies evaluating the impact of the ZCTP focused mostly on baseline schoolgirls, as this group allowed an experimental comparison of impacts between the CCT and the UCT arms of the intervention (see Baird, McIntosh, and Özler 2011). However, CCTs had large and statistically significant impacts on a number of outcomes among baseline dropouts as well—a group that is often left out of programs that are school based (Bandiera et al. 2015). Thus, we first present findings on the impacts of CCTs for both baseline schoolgirls and baseline dropouts, before turning

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either recipient (Duflo 2003). Under the same program, schooling for children age thirteen to seventeen increased more when the eligible recipient was male (Edmonds 2006). Ashraf, Field, and Lee (2014) show that women who are empowered to take charge of birth control decisions through concealable contraceptives in Zambia report a lower subjective well-being.

to a comparison of CCT and UCT impacts among baseline schoolgirls only.[6] We examine impacts while the program was ongoing (Round 2) and immediately after the program ended (Round 3).

When examining the effects of CCTs on female empowerment, we focus on baseline schoolgirls and baseline dropouts separately throughout the analysis for a number of reasons. First, the schooling condition works differently on these two groups—for baseline dropouts it brings them back into school, while for baseline schoolgirls it prevents them from dropping out. Second, as described in section 5.3 below, these groups look vastly different across a host of baseline characteristics and thus are best viewed as separate populations. Finally, baseline dropouts are a group that is often ignored in the analysis of CCT programs, even though the size of this population is nonnegligible. Thus, we feel that providing results separately for this group may provide the reader with some important insights.

The remainder of this chapter proceeds as follows. Section 5.2 describes the cash transfer intervention and the experimental design of this study. Section 5.3 discusses the estimation strategy. Section 5.4 presents the main results for the CCT arm and focuses on program impacts on the dimensions of resources, agency, and functioning, with section 5.5 discussing the results for the UCT arm and comparing the two interventions. Section 5.6 concludes.

5.2   Research Setting and Design[7]

5.2.1   Location

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 gross national income (GNI) per capita figure of $760 (purchasing power parity [PPP], current international $) is less than 40 percent of the sub-Saharan African average of $1,973 (World Development Indicators Database 2010).

5.2.2   Sample

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

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6. The decision to leave the discussion of the impact of UCTs until after the discussion of CCTs is not a reflection on the relative importance of these two interventions. It is simply for ease of exposition—allowing for an explicit focus on *baseline dropouts*.

7. This section draws heavily from section 2 in Baird, McIntosh, and Özler (2011), which provides more detail on the study design and the intervention.

city, while 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). In these 176 EAs, each dwelling was visited to obtain a full listing of never-married females, age thirteen to twenty-two.[8] The target population was then divided into two main groups: baseline dropouts and baseline schoolgirls. In each selected EA, 100 percent of all eligible baseline dropouts and 14–100 percent of all eligible baseline schoolgirls were randomly sampled to participate in the study, where the percentage depended on the core respondent's age at baseline and the strata she lived in (urban, near rural, and far rural). This sampling procedure resulted in a total study sample of 3,796 women with an average of 5.1 baseline dropouts and 16.5 baseline schoolgirls per EA.

5.2.3 Research Design

Treatment status was assigned at the EA level and the sample of 176 EAs was randomly divided into two equally sized groups: eighty-eight treatment EAs and eighty-eight control EAs. In the eighty-eight treatment EAs, all baseline dropouts were offered conditional cash transfers. The eighty-eight treatment EAs were then randomly assigned to one of three groups to determine the treatment status of baseline schoolgirls: in forty-six EAs baseline schoolgirls received transfer offers conditional on regular school attendance (CCT arm), while in twenty-seven EAs they received offers for unconditional cash transfers (UCT arm). In the remaining fifteen EAs no baseline schoolgirls received any transfer offers.[9]

5.2.4 CCT Intervention

After the random selection of EAs and individuals into the treatment group, the local non-governmental 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 being offered to the beneficiary and to her parents, the condition to regularly attend school, and the duration of the program. It was possible for more than one eligible girl from a household to be invited to participate in the program.

The offer to participate in the program consisted of a transfer to the

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8. The target population of thirteen to twenty-two-year-old, never-married females was selected for a variety of reasons. For details, we refer the reader to Baird, McIntosh, and Özler (2011).

9. Girls who live in treatment EAs but do not receive transfers allow for the measurement of spillover effects within treatment EAs.

parents, a transfer directly to the girl, and payment of school fees for girls attending secondary school. 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, which was equal to $1, $2, $3, $4, or $5 per month. The lottery was held publicly to ensure that the process was transparent.[10] Secondary school fees were paid in full directly to the schools.[11] This chapter focuses on the average effect of these transfers and does not delve into elasticities of the outcomes under investigation with respect to the transfer amounts.

Monthly school attendance of all the conditional cash transfer recipients was checked and payment for the following month was withheld for any student whose attendance was below 80 percent of the number of days that the school was in session for the previous month. However, participants were never removed from the program for failing to meet the monthly 80 percent attendance rate, meaning that if they subsequently had satisfactory attendance their payments would resume. Offers to everyone, identical to the previous ones 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.

5.2.5   UCT Intervention

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. Other design aspects of the intervention were kept identical so as to be able to isolate the effect of imposing a schooling conditionality on primary outcomes of interest.[12] Attendance was never checked for recipients in the UCT arm and they received their payments by simply presenting at the transfer locations each month.

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10. The lottery was held among those selected to participate in the program. Hence, each girl present at the lottery was offered at least $1 per month individually and $4 per month for her parents. Girls not selected for the program were never contacted by the implementing NGO and were not present at the lottery.

11. Primary schools are free in Malawi, but student 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.

12. 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. 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.

5.2.6 Data

The data used in this chapter were collected in three household survey rounds. Baseline data, or Round 1, was 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 twelve 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 two-year intervention at the end of 2009. The intervention period coincided with the 2008 and 2009 school years.[13]

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. In addition to the household survey, biological data on HIV and Herpes Simplex Virus-2 (HSV-2) were collected eighteen months after baseline (approximately six months after the Round 2 household survey).[14] Finally, the entire sample was given three achievement tests (Mathematics, English Reading Comprehension, and Raven's Colored Matrices), conducted at home, in Round 3.

5.3 Estimation Strategy

5.3.1 Attrition and Balance

Before turning to our overall estimation strategy, this subsection first examines two potential sources of bias: (a) differential attrition and (b) imbalance in baseline characteristics between treatment and control. Table 5.1 investigates attrition by regressing a binary indicator that takes on the value of one if a respondent was surveyed in all three rounds on the treatment indicator. Column (1) shows that among baseline dropouts the attrition rate in the control group was 15.7 percent with no significant difference between treatment and control. Column (2) shows that among baseline schoolgirls, the attrition rate among the control group was even lower at 10.7 percent,

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13. At the time of the intervention, the Malawian school year corresponded with the calendar year.
14. See Baird et al. (2012) for more details.

Table 5.1   **Analysis of attrition**

	Dependent variable: = 1 if surveyed in all three rounds	
	Dropouts (1)	Schoolgirls (2)
Conditional treatment	0.005	0.021
	(0.028)	(0.030)
Unconditional treatment		0.030
		(0.024)
Mean in the control group	0.843	0.893
Number of observations	889	2,284
Prob. > F(conditional = unconditional)		0.797

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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

with again no significant differences between either treatment arm and the control group, nor between the two treatment arms ($p = 0.797$). These findings suggest that the results we present in this chapter are unlikely to be biased due to differential attrition between the study arms.

Table 5.2 investigates the balance of the experiment by regressing baseline covariates (including parental, personal, and household characteristics) that the literature suggests are correlated with outcomes of interest on treatment indicators. Column (1) presents the mean value of each of the baseline characteristics in the control group among baseline dropouts, while column (2) presents the coefficient on the difference between treatment and control for baseline dropouts. We observe no violations of balance among this group. Column (3) presents the mean in the control for baseline schoolgirls, with column (3) indicating the CCT difference with the control group, column (4) the UCT difference with the control group, and column (5) the *p*-value for the difference between the two treatment arms. First of all we observe that, on average, baseline schoolgirls come from better socioeconomic backgrounds than baseline dropouts. Baseline schoolgirls have completed more schooling, come from households with better access to durable goods, are more likely to have parents who are still alive, and less likely to be sexually active. There is one violation in baseline balance for the CCT baseline schoolgirls, and one for the UCT arm, with these two variables also being different between the two treatment arms: girls in the CCT arm are approximately half a year younger than those in the UCT arm and, hence, have a similarly low grade attainment at baseline. As described in the subsection

Table 5.2   Balance of baseline characteristics

	Dropouts			Schoolgirls		
	Control mean	Treatment difference	Control mean	Conditional treatment difference	Unconditional treatment difference	P-value (conditional-unconditional)
	(1)	(2)	(3)	(4)	(5)	(6)
Parental characteristics						
Respondent's mother is alive	0.791	−0.037	0.842	−0.040	−0.006	0.360
Respondent's father is alive	0.642	0.004	0.705	0.008	0.054	0.288
Personal characteristics						
Respondent's age	17.579	−0.403	15.252	−0.299*	0.173	0.007
Respondent never had sex	0.302	−0.015	0.797	−0.001	−0.023	0.582
Highest grade attended by respondent	6.223	−0.195	7.479	−0.233	0.417**	0.004
Household characteristics						
Asset index (first principal component of 15 durable goods)	−0.772	0.081	0.581	0.402	0.639	0.623
Household size	6.128	−0.050	6.432	−0.049	0.230	0.202
Geographic strata (used for block randomization)						
Respondent lives in Zomba city	0.181	−0.035	0.347	0.112	0.070	0.807
Respondent lives in rural area within 16 km from Zomba city	0.699	−0.029	0.563	−0.122	−0.031	0.577
Respondent lives in rural area more than 16 km from Zomba city	0.120	0.063	0.090	0.010	−0.039	0.458
Observations						

Notes: Observations are weighted to make results representative of all study EAs.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

below, we control for the baseline values of these variables in our analysis of program impacts.

5.3.2   Specification

We analyze the intention-to-treat (ITT) effects of the intervention separately on Round 2 and Round 3 indicators using cross-sectional regressions. This approach allows us to investigate whether there were any empowering impacts of the program while the young women were still participating in the cash transfer program, as well as whether such impacts were still present immediately after the program ended. The regression-adjusted ITT impact of the program is estimated with ordinary least squares (OLS) using the following linear regression model:

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

where Y_i is the empowerment outcome for individual i in Round 2 or Round 3; X_i is a vector that contains a set of baseline controls; $T_i^C (T_i^U)$ is a binary indicator that is equal to one if a girl was offered a CCT (UCT) and zero otherwise. For baseline dropouts, equation (1) excludes the UCT indicator. The standard errors ε_i are clustered at the EA level to account for the design effect of the EA-level treatment assignment. Age- and stratum-specific sampling weights are used to make the results representative of the target population in the study area. To make the results comparable across survey rounds, the analysis includes respondents if and only if they were interviewed in all three rounds.

In choosing the covariates, X_i, included in this analysis, we follow the approach advocated by Bruhn and McKenzie (2009) and control for two types of variables: strata that were used for block randomization in the trial and baseline characteristics that are predictive of the outcome. In this study, these covariates include dummy variables for age and geographic strata along with an index of household assets, highest grade attained, and sexual activity status—all measured at baseline.

5.4   Impacts of CCTs

5.4.1   Resources

We first investigate to what extent the ZCTP influenced the physical resources available to the core respondent. As explained above, the monthly transfers consisted of two components, one component paid to the parents (or the guardian) of the core respondent and one component paid directly to the core respondent herself. Table 5.3 examines whether the cash transfers translated into higher monthly expenditures by respondents on themselves. Columns (1) and (2) present the impacts in Round 2 (during the intervention) while columns (3) and (4) present impacts in Round 3 (after the intervention ended).

Table 5.3   **Program impacts on expenditure by the respondents on themselves (in USD) over the past thirty days**

	Round 2		Round 3	
	Dropouts (1)	Schoolgirls (2)	Dropouts (3)	Schoolgirls (4)
Conditional treatment	1.530***	1.799***	0.334	0.788**
	(0.573)	(0.497)	(0.544)	(0.324)
Unconditional treatment		1.434***		−0.229
		(0.528)		(0.885)
Mean in the control group	3.593	2.263	3.740	2.488
Number of observations	750	2,087	749	2,086
Prob. > F(conditional = unconditional)		0.619		0.276

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. 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Baseline dropouts spent US$1.5 per month more than the control group while the program was ongoing, an increase of approximately 42 percent (column [1]). This increase amounts to approximately half of the mean amount of US$3 per month transferred directly to the respondents. The increase in expenditures was similar for baseline schoolgirls at US$1.8 per month, an increase of approximately 80 percent over the control group (column [2]). The impact of the program remained after the program ended for baseline schoolgirls, although the magnitude had declined by over 50 percent with no significant effect among baseline dropouts. The results suggest that the cash transfer program led to an increase in the beneficiaries' control of cash resources during the program, and that these impacts declined or disappeared shortly after the end of the program.

Table 5.4 shows that the direct transfers to the beneficiaries most likely drove this increase in personal consumption. Panel A shows that respondents had little influence on the way the component paid to the household was spent. Roughly 90 percent of the respondents indicated that the decision on how to spend these funds was made by someone else. Panel B, on the other hand, shows that approximately 80 percent of the young female beneficiaries had control over how to spend the transfers made directly to them.[15]

15. This finding accords with findings from focus group interviews at the design stage of ZCTP, during which females eligible for the program indicated that they would be able to "keep their transfers" if these were physically given to them.

Table 5.4   **Who decides how the transfer is spent (in percentages)?**

	Dropouts (1)	Conditional schoolgirl (2)	Unconditional schoolgirl (3)
A. Transfer to household			
Father	10.09	11.02	7.64
Mother	55.05	59.70	64.23
Self	13.76	6.77	4.96
Other	21.10	22.52	23.18
B. Transfer to respondent			
Father	3.37	2.77	0.81
Mother	11.66	13.70	7.08
Self	80.98	77.51	86.01
Other	3.99	6.02	6.10
Number of observations	326	448	253

Notes: These results are from Round 2 only. Observations are weighted to make results representative of all study EAs. These results are for treatment girls only.

5.4.2   Agency

Schooling

Next, we turn to the impact of the intervention on social patterns that can be detrimental to the development of adolescent girls. We first investigate how the intervention affected schooling outcomes. Baird, McIntosh, and Özler (2011) show that the CCT program had a strong effect on school enrollment among baseline schoolgirls. Among this group the intervention significantly increased the average number of terms enrolled by 0.54 according to teacher reports (over a base of 4.79 terms in the control out of a maximum of six during the two academic years the program ran). Baseline dropouts experienced an increase of 2.35 terms (compared to 1.02 in the control group, using self-reported enrollment data—see table 5A.1, column [1], appendix)—an impact that more than tripled the number of terms enrolled.[16]

An important question is whether the program empowered respondents by providing them with additional knowledge and skills to make important life decisions. Baird, McIntosh, and Özler (2011) provide a first indication that the intervention indeed resulted in increased skills. They show that among baseline schoolgirls, there were significant improvements in math,

16. We did not collect teacher reports of enrollment or attendance for this stratum in Round 3. Hence, these findings should be treated with some caution due to the fact that the underlying data for school enrollment are self-reported. Please see Baird and Özler (2012) for more on the reliability of self-reported data on school participation. However, significant improvements in learning presented in table 5A.1 support the finding of a significant increase in the reenrollment rate for this group.

Table 5.5.   **Program impacts on participation in health training over the past twelve months**

	Any			
	Round 2		Round 3	
	Dropouts (1)	Schoolgirls (2)	Dropouts (3)	Schoolgirls (4)
Conditional treatment	0.152***	–0.041	0.025	0.071**
	(0.041)	(0.029)	(0.034)	(0.033)
Unconditional treatment		0.012		0.068**
		(0.035)		(0.035)
Mean in the control group	0.547	0.879	0.696	0.775
Number of observations	750	2,087	749	2,086
Prob. > F(conditional = unconditional)		0.215		0.939

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. 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.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

English and cognitive test scores during Round 3.[17] Among baseline dropouts there were also modest, but statistically significant, improvements across the three tests with impacts ranging from 0.13 standard deviations in English Reading Comprehension ($p < 0.10$) to 0.16 standard deviations in mathematics ($p < 0.05$) (table 5A.1, columns [2]–[4], appendix). Table 5.5 adds to this evidence by investigating whether the program increased participation in any form of health training (including nutrition, personal hygiene, food hygiene, sexual education, and HIV/AIDS) and shows that both groups of CCT beneficiaries saw increases in the probability of participating in health training during the past twelve months.

Fertility and Marriage

Having shown that the ZCTP improved the physical resources available to respondents and increased their school participation and learning, we now turn to other important outcomes, such as childbearing and marriage, which may be influenced by the intervention either through an income effect or an effect of the condition to regularly attend school. We first investigate the impact of the intervention on respondents' fertility decisions, one of the

17. For more details on the specifics of these achievement tests, see Baird, McIntosh, and Özler (2011). These tests were only conducted in Round 3.

prime outcomes in the theoretical empowerment literature (see, for instance, Eswaran [2002] or Iyigun and Walsh [2007]).

Baird et al. (2010) shows that the conditional cash transfers significantly reduced pregnancy among treated baseline dropouts during the first year of the program, with no significant impact among baseline schoolgirls.[18] Baseline dropouts were 5.1 percentage points less likely to ever have been pregnant (61 percent in the control group) in Round 2 and 8.2 percentage points (78 percent in the control group) in Round 3 (table 5A.1, column [5], appendix). In table 5.6, we investigate whether the program also impacted preferred fertility timing or desired lifetime fertility. Looking at the impact of the intervention on the number of months the respondent would like to wait before having a child, we observe significant increases for baseline dropouts in both rounds and for baseline schoolgirls in Round 3 (columns [1]–[4]).[19] The CCT effects on the ideal number of children are generally negative but only significant among baseline schoolgirls at the end of the intervention, who, on average, want 0.184 less children over their lifetimes. The evidence suggests a desire to delay childbearing as a result of the intervention rather than lowering the ideal number of children.

Turning now to marriage, the CCT program led to a significant decline in marriage rates among baseline dropouts. At baseline all of the respondents were never married, while 28 percent of the baseline dropouts in the control group had gotten married by Round 2. In the treated group the marriage rate was 11 percentage points lower (Baird et al. 2010). This difference persisted after the program ended—baseline dropouts were 12.6 percentage points less likely to be married by Round 3—from a mean of 55.1 percent in the control group (table 5A.1, column [6], appendix). Among baseline schoolgirls, on the other hand, there was no evidence of a significant impact of the CCT intervention on marriage during or after the program (Baird, McIntosh, and Özler 2011).

It may be puzzling to the reader as to why a large effect on marriage is found among baseline dropouts but not baseline schoolgirls. As discussed in detail in Baird, McIntosh, and Özler (2011), the cash transfer effect on marriage works through two channels in Malawi, where marriage and schooling are exclusive: an income effect and a schooling effect. In the case of CCTs, the income effect is zero for those who drop out of school during the

18. For an explanation of why a significant effect on childbearing is observed among *baseline dropouts* but not *baseline schoolgirls*, please refer to Baird, McIntosh, and Özler (2011, 1735–40).

19. This variable is missing for respondents who want zero children or do not want any more children. We find no impact of the intervention on whether or not this variable is missing for baseline dropouts. However, in Round 3, there is a significant and positive coefficient on this relationship for baseline schoolgirls. This result suggests that the CCT intervention may have also decreased the number of respondents wanting any additional children among *baseline schoolgirls*. The significant decline in the ideal number of children in this same group (table 5.6, column [8]) supports this interpretation.

Table 5.6 Program impact on preferred fertility timing or desired lifetime fertility

| | Months before next child | | | | Ideal number of children | | | |
| | Round 2 | | Round 3 | | Round 2 | | Round 3 | |
	Dropouts (1)	Schoolgirls (2)	Dropouts (3)	Schoolgirls (4)	Dropouts (5)	Schoolgirls (6)	Dropouts (7)	Schoolgirls (8)
Conditional treatment	7.383**	4.934	3.658*	9.056***	−0.064	−0.112	0.006	−0.184*
	(2.992)	(3.110)	(2.093)	(3.158)	(0.101)	(0.104)	(0.086)	(0.099)
Unconditional treatment		2.790		2.138		0.084		0.025
		(3.765)		(3.130)		(0.122)		(0.103)
Mean in the control group	52.056	79.092	48.657	70.207	2.919	2.813	2.942	2.909
Number of observations	729	2,047	699	1,994	749	2,087	747	2,080
Prob. > F(conditional = unconditional)		0.616		0.100		0.186		0.119

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. 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.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

program because they cease to receive payments. Hence, the primary channel through which CCTs can delay marriage is through increased school enrollment. In this experiment, the marginal effect of CCTs on school enrollment was, while significant, relatively small among baseline schoolgirls, meaning that the knock-on effect on marriage was undetectable. Baseline dropouts, on the other hand, experienced a massive surge in their odds of reenrollment, which translated into delayed marriage and pregnancy. The CCT intervention also had a significant impact on baseline dropouts viewing education as an important characteristic of a future spouse (results not shown). We come back to this issue in section 5.5, when we contrast the effects of UCTs on these outcomes, where the channels of impacts are different yet again.

Overall, the results presented in this section indicate that CCTs had a strong impact on agency, as they caused beneficiaries to increase their school participation and learning, and, among baseline dropouts, beneficiaries postponed marriage and pregnancy. These changes may well affect the well-being of these respondents later in life as well as their bargaining power in future relationships. Moreover, as the next section will show, the intervention also had a substantial impact on the contemporary functioning of respondents.

5.4.3   Functioning

Position in the Household

Finally, we examine how the intervention affected two areas of functioning within the household: (a) self-perceived position within the household, and (b) nutrition and health. Table 5.7 first examines how the intervention affected respondents' answer to the question "Would you say your household cares more about your education now compared to twelve months ago?" Baseline dropouts and baseline schoolgirls in the CCT arm are significantly more likely to agree with the statement than those in the control group during the program (columns [1] and [2]), with no lasting effects once the program is over. The pattern is the same when we analyze responses to the question "Would you say your household cares more about your health now compared to twelve months ago?" These findings suggest that the intervention may have improved the standing of the school-age beneficiaries within their households by making them more of an asset to their families during the program period.

Nutrition and Health

Above, we showed that beneficiaries perceived their households to care more about their health while the program was ongoing. We now look at whether this perception is accompanied by tangible improvements in

Table 5.7   Program impact on self-perceived standing in the household

	Dependent variable							
	= 1 if household cares more about respondent's education than twelve months ago				= 1 if household cares more about respondent's health than twelve months ago			
	Round 2		Round 3		Round 2		Round 3	
	Dropouts (1)	Schoolgirls (2)	Dropouts (3)	Schoolgirls (4)	Dropouts (5)	Schoolgirls (6)	Dropouts (7)	Schoolgirls (8)
Conditional treatment	0.334***	0.075**	0.017	0.032	0.067**	0.158***	0.016	0.026
	(0.038)	(0.034)	(0.027)	(0.033)	(0.034)	(0.045)	(0.030)	(0.029)
Unconditional treatment		0.165***		0.034		0.183***		0.081*
		(0.036)		(0.036)		(0.043)		(0.043)
Mean in the control group	0.161	0.524	0.141	0.416	0.242	0.255	0.201	0.288
Number of observations	733	2,077	728	2,076	732	2,075	728	2,072
Prob. > F(conditional = unconditional)		0.034		0.961		0.686		0.262

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. 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

investments in nutrition and health, as well as measurable health outcomes. Kabeer (1999) argues that nutrition and health outcomes are prime examples of "universally valued functionings," and as such they are highly relevant empowerment outcomes.

In table 5.8, we first look at the impact of the intervention on the intake of three sources of protein: meat, eggs, and fish (columns [1]–[4]). The outcome variable counts the number of days respondents ate any of these three items over the seven days prior to the interview.[20] We find that CCTs caused a modest increase (approximately 10 percent) in the consumption of protein rich food items among both baseline dropouts and baseline schoolgirls during the program (columns [1] and [2]) and that this effect persisted among baseline schoolgirls after the program ended, with no such lasting effect among baseline dropouts (columns [3] and [4]). We then investigate whether the intervention affected the probability that respondents usually sleep under a bed net (columns [5]–[8]). We observe a significant improvement among baseline schoolgirls: they are 7.8 (8.5 percent) percentage points more likely to sleep under a bed net in Round 2 (Round 3)—representing a large increase compared to the control group mean of 49.3 (65.9 percent). Given the high prevalence of malaria parasitemia in this area, which is a frequent cause of school absenteeism, it makes sense for families to devote resources to preventive health measures in an effort to minimize the probability of missing transfer payments due to noncompliance. These findings are also consistent with the effects of CCTs on school attendance being highest during term 1, when the number of malaria cases reaches its peak in Malawi (Baird, McIntosh, and Özler 2011). We observe no similar improvements among baseline dropouts during or after the program. This is somewhat surprising, but it is worth noting that baseline dropouts come from significantly poorer households than baseline schoolgirls.

There is also evidence that the intervention improved the mental health of its beneficiaries—at least during the two-year period while it was ongoing. Using the General Health Questionnaire 12, a screening instrument widely used in clinical settings to detect individuals who are likely to suffer from psychological distress, Baird, de Hoop, and Özler (2013) show that baseline schoolgirls in the CCT arm were approximately 6 percentage points (or 17 percent) less likely to be suffering from psychological distress than those in the control group during the program. These effects had become smaller and statistically insignificant soon after the program ended. No similar effects were detected among baseline dropouts. The authors suggest that the significant changes in the daily life of baseline dropouts due to reenrollment in school compared with the relative lack of such changes among baseline

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20. This variable thus takes values from 0 to 21:0 if the respondent ate none of these food items and 21 if the respondent ate all of the ingredients every day during the seven days prior to the interview.

Table 5.8 **Program impact on nutrition and health**

| | Number of times respondent ate protein-rich food in past seven days (out of twenty-one) | | | | = 1 if respondent usually sleeps under a bed net | | | |
| | Round 2 | | Round 3 | | Round 2 | | Round 3 | |
	Dropouts (1)	Schoolgirls (2)	Dropouts (3)	Schoolgirls (4)	Dropouts (5)	Schoolgirls (6)	Dropouts (7)	Schoolgirls (8)
Conditional treatment	0.340*	0.489**	0.120	0.530***	−0.022	0.078*	0.028	0.085*
	(0.189)	(0.202)	(0.188)	(0.179)	(0.040)	(0.040)	(0.032)	(0.044)
Unconditional treatment		0.434**		0.282*		−0.015		0.009
		(0.184)		(0.165)		(0.066)		(0.055)
Mean in the control group	3.662	3.954	4.076	4.084	0.584	0.493	0.724	0.659
Number of observations	750	2,087	749	2,086	746	2,081	748	2,084
Prob. > F(conditional = unconditional)	0.827		0.255		0.204		0.228	

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. 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

schoolgirls may partly explain the differential impact of the intervention on the psychological well-being of adolescents in these two strata.

Together, the presented results suggest that the impact of the intervention differed substantially between baseline schoolgirls and baseline dropouts. We observe stronger effects on health and nutrition among baseline schoolgirls and larger changes in marriage, pregnancy, and school enrollment among baseline dropouts. A likely explanation for this finding lies in the baseline differences between these two strata and the actions needed to be taken by the beneficiaries and their households to ensure compliance with the condition to attend school regularly. As we saw earlier, pregnancy and marriage are much bigger hazards to school enrollment among baseline dropouts, so their households would have encouraged beneficiaries not to get married or pregnant, to spend more time attending school and less time on household chores and labor. In the households of baseline schoolgirls, the potential payoff from following this strategy was limited because the children were already spending a relatively large share of their time on schooling—that is, most of the transfers to baseline schoolgirls were inframarginal. These households are more likely to decrease the probability of noncompliance by investing in the health of their eligible children to minimize school absences due to illness.

5.5   Impacts of UCTs

Previous analysis contrasted the effects observed in the CCT and the UCT arms on schooling outcomes, marriage, pregnancy, and mental health among baseline schoolgirls (Baird, McIntosh, and Özler 2011; Baird, de Hoop, and Özler 2013). These papers report some interesting findings, which suggest that attaching conditions to cash transfers have costs and benefits. For example, while the UCT program significantly reduced the dropout rate among its beneficiaries, this impact was only 43 percent as large as the impact of the CCT arm at the end of the two-year program. Moreover, as confirmed by differential impacts on attendance and test scores favoring the CCT arm, CCTs were found to be more cost effective than UCTs in raising enrollment rates. However, as discussed above, while the CCT program had no impact on marriage and pregnancy among baseline schoolgirls, the UCT treatment led to a significant reduction on both marriage and pregnancy by the end of the program. Furthermore, while both programs caused significant declines in psychological distress while the program was ongoing, the effects in the UCT arm were significantly larger than those in the CCT arm.

Baird, McIntosh, and Özler (2011) provide an explanation for the differential effects of CCTs and UCTs on marriage and pregnancy. While CCTs only had an indirect effect on these outcomes through their effect on increased schooling participation, UCTs had a direct effect on marriage and pregnancy through an income effect. This difference between the two study

arms was due almost entirely to the effect of UCTs among girls who dropped out of school during the two-year program—as this large group of girls still received regular transfer payments. The authors argue that schooling conditions, while effective in improving school participation and learning, may undermine the social protection aspect of cash transfer programs by denying support to noncompliant households.

Baird, de Hoop, and Özler (2013) exploit the random variation in amounts that were transferred separately to the girls and their parents (guardians) in each study arm and find that CCT and UCT effects on mental health were similar at the lowest amounts given to the households. However, increasing transfers to parents conditional on school attendance by the beneficiary led to significant increases in psychological distress of the adolescent girls—no such gradient was found in the UCT arm or for the transfers to the girls. The authors speculate that the burden of becoming the main source of income for their families may have become too much for these school-aged children to shoulder when a large monthly transfer was conditional on their actions.

The new results presented in this chapter provide some additional insights to these earlier findings. With respect to resources, like the CCT arm, the UCT arm significantly increased the personal consumption of the respondent while the program was ongoing, but this impact appears to have dissipated faster than that of the CCT arm (table 5.3). The UCT recipients also report controlling the transfer that was directly transferred to them (86 percent), with very few controlling the household-level transfer (5 percent). Thus, these results suggest that the cash transfer program led to an increase in the control of resources for the UCT beneficiaries during the program, but that the effect did not last once the program ended. In terms of agency, unlike the CCT arm, we find no impacts of UCTs on either the number of months before having their next child or the ideal number of children (table 5.6), and these effects differ from the CCT arm ($p = 0.100$ and $p = 0.119$, respectively). Given the significantly larger delays in fertility among this group, it will be interesting to see whether the pregnancy and marriage rates in the UCT arm quickly catch up with the CCT arm in the near future. Finally, turning to functionings, respondents in the UCT arm report similar or larger effects than the CCT arm on their self-perceived standing within their households (table 5.7), similar increases in the consumption of food items rich in protein, but a lower likelihood of sleeping under bed nets (table 5.8). Income effects likely explain the improvements in their position within the household and increases in their personal and food consumption, while the lack of incentives to attend school is consistent with the lack of effect in preventive health investments, such as bed nets.

These findings make clear that the impacts of CCT and UCT programs are likely to differ—at least in the short run—and that one is not clearly preferable to the other. The choice between these two approaches may depend on the aims of the intervention, the target population, and relative weights

the policymaker or the social planner assigns to various outcomes. This is, of course, only true under the assumption that there is a legitimate reason to attach conditions to cash transfer programs, such as market failures, externalities, or political economy reasons. Furthermore, it is not clear how these short-term impacts might translate into longer-term outcomes with respect to women's empowerment. It will be interesting to observe whether there are longer-term impacts on a broad range of outcomes, such as subjective welfare, bargaining power within marriages, fertility choices, early childhood development of own children, labor force participation, voice and political participation, and so forth, and whether and how these differ between the experimental study arms.

5.6   Concluding Discussion

Adolescent girls in developing countries are considered to be an important target group for policymakers. Targeted interventions for this group may not only affect their welfare directly, but they also have the potential to bring benefits to future generations. This chapter investigates whether one such intervention, the Zomba Cash Transfer Program in Malawi, helped empower adolescent girls in the short run. Summarizing evidence from multiple papers examining the impacts of this program on a broad range of outcomes and providing some new analysis here, this chapter suggests that the answer is a clear "yes." The program effectively increased access to financial resources, increased schooling outcomes, decreased teen pregnancies and early marriages, improved health, and generally enabled beneficiaries to improve their agency within their households.

The intervention provided its beneficiaries with financial support conditional on attending school in the CCT arm, and unconditionally in the UCT arm. Furthermore, the CCT arm was targeted to two distinct groups of school-age girls: those that were in school at baseline and those that had already dropped out before the intervention began. The latter group, albeit small at approximately 15 percent of the eligible population, saw substantial increases in schooling outcomes, as well as large declines in early marriage and pregnancy rates. As many interventions targeting school-age populations are school based, these findings point to the importance of conducting population-based interventions to avoid exclusion of this particularly vulnerable group of young people.

The CCT program changed some common socioeconomic patterns that affect young women in Malawi, as it induced beneficiaries to delay childbearing and marriage. There is some evidence that these changed socioeconomic patterns are accompanied with changed marital and fertility preferences, suggesting that empowering adolescent women may not only increase their bargaining power within future relationships, but it may also affect the type of relationship they enter into in the first place. The experiment also

revealed contrasting findings between the CCT and the UCT arms. While eligible girls in both arms experienced benefits, the domains in which they experienced these benefits and the strength of the effects differed between the two groups. The findings teach us that while there may be good reasons to implement CCT programs, there are serious trade-offs associated with attaching conditions to cash transfer programs. The design choice will depend on the target population and the goals at hand.

Overall, the results presented here indicate that cash transfers targeted at adolescent girls and young women can empower them in significant ways in the short run—at least in this or similar settings. It can alter social patterns that cause suboptimal investments in the human capital of young women and it can improve both their standing within the household and their day-to-day functioning. While these short-run impacts are promising, the ultimate impact of the ZCTP will depend on whether this relatively short (two-year) cash transfer program—introduced at a particularly important period of transition from adolescence to adulthood—can have long-lasting effects on the lives of this cohort of young females and their future families.

Appendix

Table 5A.1   **Additional program impacts on baseline dropouts in Round 3**

	Number terms enrolled (out of 6) (1)	English test score (standardized) (2)	Math test score (standardized) (3)	Cognitive test score (standardized) (4)	Ever pregnant (5)	Ever married (6)
Conditional treatment	2.348***	0.131*	0.164**	0.142**	–0.082***	–0.126***
	(0.163)	(0.070)	(0.066)	(0.071)	(0.027)	(0.036)
Mean in the control group	1.021	0.000	0.000	0.000	0.780	0.551
Number of observations	749	729	729	729	749	749

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. 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.

*** Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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