More is Better, But for Whom?

The Heterogeneous Effects of Cash Transfer Amounts and Conditionality on School Enrollment in Malawi*

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Abstract

Cash transfers, both conditional and unconditional, are a form of wealth redistribution that increase school enrollment and help advance Millennium Development Goals. Transfers could be targeted to maximize cost-effectiveness given finite resources; however, there is limited evidence regarding marginal impacts of conditionality and increasing transfer amounts, or which groups benefit most. In 2011, Baird et al. reported findings of a cluster randomized controlled trial in Malawi demonstrating conditionality increased school enrollment and higher transfer amounts to parents or subjects had no incremental effect on enrollment. We improve upon their analysis by remodeling the categorical outcomes using weighted logistic regressions with a binomial rather than normal stochastic component, and examining incremental effects of total transfer amounts to households. We find higher transfers amounts improve enrollment for both conditional and unconditional schemes and are more influential in the conditional group. Additionally, we find varying effects among unconditional subgroups over time. We conclude that in Sub-Saharan African settings such as Malawi, cash transfer programs to improve school enrollment should be conditional, and minimal transfer amounts are suboptimal to maintain enrollment gains for sustained periods. Unconditional cash transfers in these settings should consider recipients' age and baseline household index, as they influence enrollment outcomes.

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Background

Cash transfers have been promoted as an effective means of increasing household welfare, child school attendance and health outcomes in the developing world. Conditional cash transfer programs provide money in exchange for participants providing a service (e.g. work program), utilizing a service (e.g., school or health clinic attendance) or with the agreement that the transfer will be spent in a specific fashion (e.g., food purchase or investing in a business). Numerous experimental studies have demonstrated the effectiveness of conditional cash transfers in improving health and educational outcomes (Fiszbein, 2009; Lagarde, 2009; Rawlings, 2005). These programs have proliferated in Latin America, and well-developed cash transfer programs exist in countries such as Mexico and Brazil (Skoufias, 2001; Soares, 2006). Unconditional transfer programs have likewise shown to increase schooling and child health (Duflo, 2011; Edmonds, 2006). Donors and taxpayers have traditionally favored conditional cash transfers because they incentivize positive behaviors that decrease future poverty (Fiszbein, 2009). Such incentives are thought to be particularly useful in settings where recipients do not place high value on desired behaviors such as child school attendance. Additionally, conditional transfers may be more politically acceptable to contributors who are not benefiting directly.

However, it remains unknown whether imposing conditionality, targeting specific subgroups, or optimizing the quantity of cash transferred is most effective or necessary to achieve specific outcomes (De Janvry 2005). Conditional cash transfers require greater

infrastructure, administrative resources to monitor compliance, and reliable access to public services to which conditions are tied. Experimental evidence directly comparing conditional and unconditional transfers is limited and suggests that both program types are effective; some studies have found that conditional transfers may achieve higher, but not significant, gains in school enrollment compared to unconditional programs (de Brauw, 2011; Baird, 2013; Robertson, 2013). Furthermore, there is conflicting evidence regarding whether incrementally higher transfer amounts produce significant marginal improvements (De Janvry 2005).

Cash transfer and other social welfare programs have traditionally been means-tested to target the most impoverished and vulnerable social groups. There is little evidence to suggest a uniform approach: targeting subgroups is highly context-specific, and specific approaches work well in certain countries but not in others (Coady, 2004; Hickey, 2007). In many African countries, reliable household income and expenditure data is sparse, requiring alternative approaches to means-testing. Additionally, the effect of transfers on schooling outcomes vary in different countries, depending on whether programs are targeted at specific demographics such as gender, age group, or rural vs. urban residents (Independent Evaluation Group, 2011; Fiszbein, 2009; Rawlings, 2005).

In a 2011 study, Baird et al. report the findings of a cluster randomized controlled trial among schoolgirls aged 13 to 22 years in Malawi examining the effect of cash transfers on school enrollment, marriage and pregnancy rates (Baird et al, 2011). In order to test the effect of conditionality, the study randomized subjects to conditional, unconditional or no

cash transfer programs with randomly assigned transfer amounts to both the schoolgirls and the parents. Both conditional and unconditional transfers reduced school dropout rates, but subjects participating in the conditional cash transfer scheme had higher school enrollment rates and test scores, while unconditional cash transfers were more effective in reducing marriage and pregnancy rates, primarily among subjects who dropped out of school. An analysis of heterogeneity of program impacts by transfer amounts to individuals or guardians, and baseline age, found that increasing amounts in the conditional cash transfer arm were not associated with incremental increases in school enrollment while in the unconditional scheme, increasing amounts were associated with higher school enrollment. They determined that conditional cash transfers raise enrollments more than unconditional transfers in dichotomized age categories (below and above 15 years of age). Based on the observation that the highest amount offered to unconditional transfer recipients had the same effect as the minimum transfer amount in the conditional transfer arm, Baird et al (2011) concluded that offering smaller transfer amounts in the conditional program would be more cost-effective in improving enrollment than increasing transfer amounts in the unconditional program.

We focus our analysis on the primary outcome of teacher–reported school enrollment. We re-examine the authors' conclusions regarding heterogeneity of program impacts and compare the effects of total transfer amounts to households, in order to better characterize subgroups to target and optimal transfer amount. First, we compare conditional and unconditional cash transfer effects on sub-groups defined by subject age, school grade and

baseline household asset index for each term to determine heterogeneity of effects on school enrollment. We find the greatest improvements in enrollment among older subjects with highest baseline household assets in the unconditional group, followed by youngest subjects with the lowest baseline household assets. Next, we examine the impact of total transfer amounts to households (subjects and guardians) on school enrollment for each term. Our implicit assumption is that households make this decision collectively based on total amounts received. We find that higher transfer amounts lead to higher enrollment for both conditional and unconditional schemes. Specifically, transfer amounts are more influential in the conditional group, and the difference in school enrollment comparing the lowest to highest household transfer amount increases with each subsequent term.

Study Design

The study is conducted in Malawi, a sub-Saharan African nation with a population of 15.3 million and GNI per capita of $320 in 2009 (World Development Indicators, 2014). Malawi ranks 160th out of 182 countries in the Human Development Index and as of 2010, had a net secondary school enrollment rate of 26% (World Development Indicators, 2014). Zomba district, which was selected for the study, is divided into 550 enumeration areas, each with an average of 250 households across several villages. Enumeration areas were stratified by distance from Zomba city, and 176 EAs were selected to representing far rural (28 EAs), near rural (119 EAs), and urban settings (29 EAs). Half of the EAs were randomly assigned to the cash transfer intervention, and of these, 46 were assigned to conditional cash transfer programs and 27 to unconditional transfers. Subjects were never-married females age 13-22 years, who were enrolled in school at baseline. Transfers were made to both

subjects and their parents/ guardians in 2008 and 2009. Transfer amounts to parents/ guardians were randomized across EAs (uniform within each EA), and transfer amounts to schoolgirls were random within EAs and determined by public lottery at the start of the study period. Transfers were disbursed on a monthly basis by local organizations, and conditional transfers were contingent on subjects having attended school at least 80% of days in the previous month.

Surveys were administered at baseline, one year and two years from the start of the study period. Subject school enrollment was assessed for three terms in each year and one term post-intervention for a total of seven consecutive terms, based on subject self-report from annual surveys and verified by teacher reports. At the conclusion of the study, school ledgers were accessed, when available, to compare attendance to enrollment. To assess educational attainment, subjects were tested in their homes on English reading comprehension, mathematics and cognitive ability at the end of the study period. Further detail is available in the publication by Baird et al (2011).

Methods

We replicate the authors' findings relevant to subsequent analysis using the models specified in their explanation. For the primary outcomes of school enrollment, they performed ordinary least squares regression with robust standard errors clustered at the enumeration area level, weighted to be representative of the target populations in the study area. They utilized a reduced-form linear probability model with binary indicators for enrollment in the conditional and unconditional programs. All models included a vector of

baseline characteristics, including household asset index, highest grade attended, a dummy variable for having started sexual activity, and dummy variables for age, selected a priori because of their association with schooling outcomes. We replicate the study findings as reported in Tables I, III, V, VI, and X exactly, apart from a few exceptions which we attribute to data-cleaning subsequent to publication. We then proceed to carry out our own analyses as described below.

The experimental study design for this study and negligible level of attrition or non-compliance give us a reliable basis of causal identification. For every model, we estimate intention to treat effects of respective treatment on enrollment within each of seven sequential school terms (Term 1 in 2008 to Term 1 in 2010).

For the first hypothesis on heterogeneity of causal effects, we identify groups of girls for whom the cash transfers are most and least beneficial, as a function of age, school grade, and household asset index at baseline, variables which have been found to strongly predict schooling outcomes and should improve precision of our estimates. We use the analytical technique for heterogeneity developed by Kosuke Imai and Marc Ratkovic, implemented through the FindIt package, an open-source software available at the Comprehensive R Archive Network (Imai & Ratkovic, 2013). The model involves a transformation of each binary outcome, to $Y_i^* = 2Y_i - 1 \in \{\pm 1\}$. The model relates the estimated outcome $\hat{Y}_i = \in \{\pm 1\}$ which is related to the estimated latent variable,

$$\widehat{W}_i \in \mathbb{R} \text{ as } \hat{Y}_i = sgn(\widehat{W})$$

$$\widehat{W}_i = \hat{\mu} + \hat{\beta}^T Z_i + \hat{\gamma}^T V_i$$

Where Z_i are the two-way interaction terms between pre-treatment covariates (baseline age, school grade and household asset index) and the treatment variables, and V_i represents the main effects of the pre-treatment covariates and the square of the pre-treatment covariates. The algorithm is based on a generalized cross-validation (GCV) statistic and estimates β and γ for binary outcomes with the L2-SVM[1] (Wang et al., 2006), implementing separate LASSO[2] constraints (λ_z and λ_v) for these coefficients. The coefficients thus range from main effects to three way interactions. We present the highest and lowest estimated treatment effects as increase or decrease in probability of enrollment in the respective term relative to a hypothetical baseline case who is a 13 year old control school girl with a household asset index of zero. In keeping with the potential outcomes framework for causal inference, this method assumes no interference among units, that there is a unique version of each treatment, the each unit has a nonzero probability of assignment to each treatment level, and the treatment level is independent of the potential outcomes (Rubin 1990, Rosenbaum & Rubin 1983).

[1] The authors of this package adapt the support vector machine (SVM) classifier to place separate LASSO constraints over each set of coefficients. They formulate the SVM as a penalized squared hinge-loss objective function (L2-SVM) that returns a difference in means estimate for the treatment effect in the absence of pre-treatment coefficients.

[2] The Lasso is a shrinkage and selection method for linear regression. It minimizes the usual sum of squared errors, with a bound on the sum of the absolute values of the coefficients.

For our second hypothesis, we define a new treatment variable – total household transfer amount – as the sum of transfers to guardian or parent and schoolgirl.[3] We then estimate the effect of increasing total household transfer amounts on school enrollment for each term. We carry out separate analyses for conditional and unconditional cash transfers as specified below:

$$Y_i \sim Bernoulli\ (\pi_i)$$

$$\pi_i = \frac{1}{1 + e^{-X_i\beta}}$$

Where X_i represents the specified treatment variable and pre-treatment covariates. We estimate $\hat{\beta}_{MLE}$ and the Hessian matrix and simulate from a multivariate normal distribution of $\hat{\beta}_{MLE}$ to incorporate estimation uncertainty. Following transformation using the logistic link function specified above, we simulate from a binomial distribution to further incorporate fundamental uncertainty. We define our quantity of interest as the difference in probability of enrollment in the respective term between recipients of the highest and lowest transfer amounts of treatment, defined in differences from the lowest total amount offered for the treatment arm ($5). Our simulations represent first differences for a 14-year-old schoolgirl who at baseline has the mean asset index, is in the median grade and has never had sex. For both hypotheses, age- and stratum-specific sampling weights are used to make the results generalizable to the target population in the study area.

[3] We actually estimate marginal effects by deducting the minimum total household transfer from the total amount so estimated, which does not change the effect estimated but facilitates interpretation of our results.

Results

We report impacts of cash transfers on binary school enrollment variables for each of seven terms during and immediately after the two-year study period (three terms each in 2008 and 2009, one term in 2010).

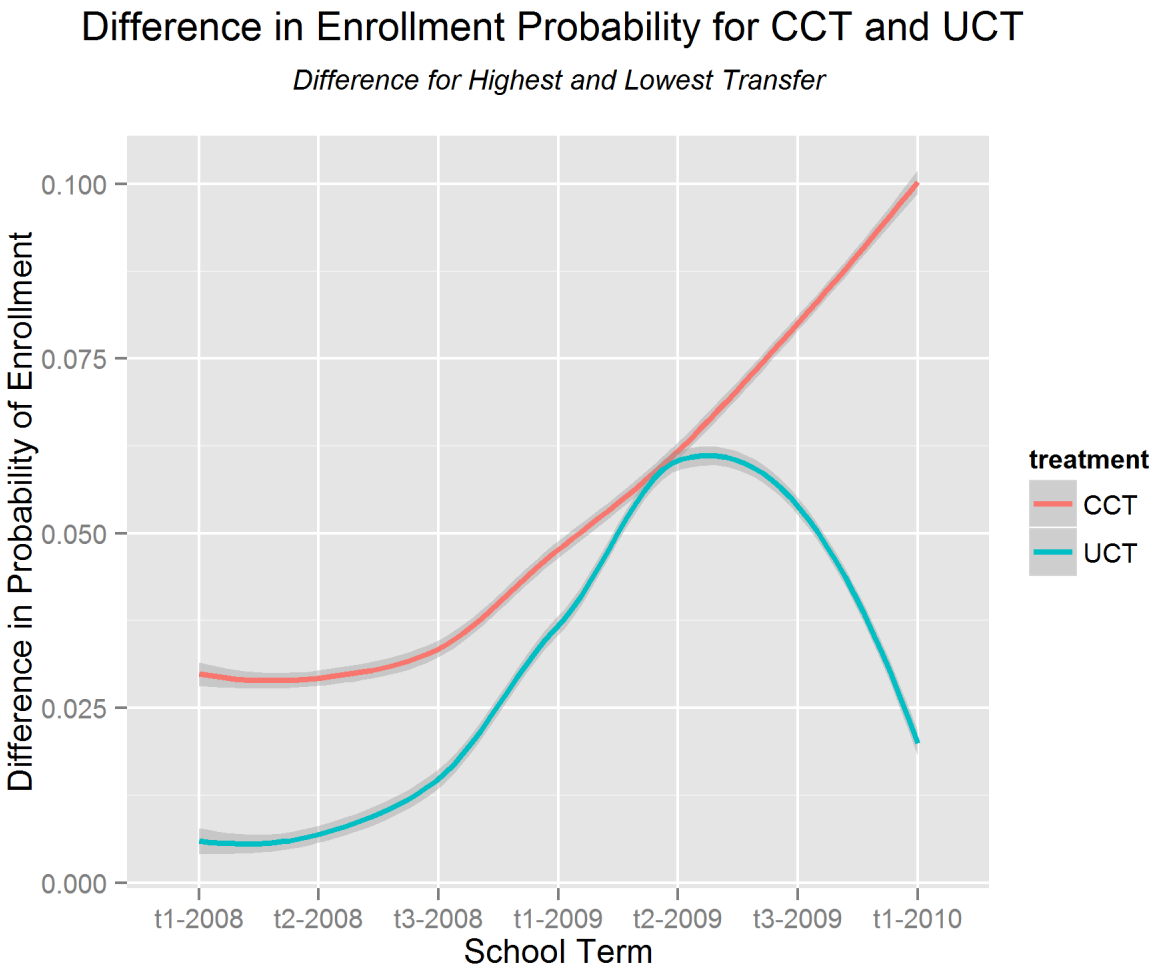
Our heterogeneity analysis reveals differing effects over time among subjects in the unconditional cash transfer scheme, particularly among older students with higher baseline assets over the entire study period (Tables 1-7, Appendix). We find that at the beginning of the study period, the greatest improvement in enrollment is observed among older students age 18-21 with lowest asset index, whereas the smallest effect is observed among the same age group with highest assets. By the third term, these effects become reversed, and we see enrollment gains among older students age 18-20 with higher household assets compared to their younger counterparts or those with lower household assets. In subsequent terms, this trend continues, with the highest impact on enrollment observed among subjects age 18-20 with the highest household assets. There are gains in enrollment among young students age 13-14, however these are consistently among those in the lowest 20% of baseline household assets. Among subjects in the conditional cash transfer arm, we find statistically significant heterogeneity among subjects of varying age and baseline household assets during all but two terms (term 2 in 2009 and term 1 in 2010). However, the heterogeneous effects are inconsistent across terms, which limits the utility of these findings for targeting specific subgroups (Tables 8-14, Appendix).[4]

[4] Among conditional cash transfer recipients in 2008, we find the lowest effects on enrollment among youngest girls (13 and 14). In the first term of 2009, the lowest effects are found in older

With respect to our analysis of the impact of total household transfer amounts, we find a steady increase in difference in probability of enrollment between highest and lowest cash transfer amounts over time among subjects in the conditional cash transfer scheme (Figure). Specifically, the difference in probability of enrollment during the first term of 2008 is 0.03 comparing highest to lowest transfer amounts, which increases to 0.12 in the first term of 2010. We calculate first differences in probability of enrollment and find statistically significant results, with larger increases in probability of enrollment in all but one term for conditional compared to unconditional cash transfer recipients.[5] Among unconditional program participants, we see smaller differences in probability of enrollment for the same cash amounts apart from term 2 of 2009 when the effects of both CCT and UCT converge. These findings are reflected in the figure, which highlights higher effects on school enrollment in conditional cash transfer with increasing transfer amounts over time.

girls with low baseline household assets, mirroring the unconditional treatment group. However, by term 3 2009 effects among varying age groups and household assets become mixed.

[5] Of note, term 1 of 2010 seems to be an outlier, as enrollment for all subjects declines regardless of transfer amount. Per the original study, this may have been due to term 1 of the year coinciding with the dry season in Malawi when food is scarce and malaria incidence peaks, limiting school enrollment. Additionally, 2010 was after the study period, so cash transfers (conditional or unconditional) were not being disbursed.


Difference in Enrollment Probability for CCT and UCT
Difference for Highest and Lowest Transfer
Difference in Probability of Enrollment
0.100
0.075
0.050
0.025
0.000
t1-2008
t2-2008
t3-2008
t1-2009
t2-2009
t3-2009
t1-2010
School Term
treatment
CCT
UCT

Discussion and Policy Implications

Our analysis extends the investigation by Baird et al (2011) in definite ways. First, we model the binary outcomes according to the data generation process using a binominal rather than normal stochastic component. Second, we expand the number and range of covariates in exploring treatment heterogeneity and rather than resort to ad-hoc subgroup analysis, we use an algorithm that more efficiently classifies and selects variables. Finally, we model a treatment variable for combined household transfer amount that accounts for the decision-making structure in Malawian households – one in which a collective decision is made by a guardian regarding the use of total household resources (Kinoshita, 2003).

Analysis of Heterogeneity for Program Targeting:

We find varying effects among subgroups enrolled in the unconditional cash transfer program. Over time, the greatest improvements in school enrollment are observed in older students age 18-20 with highest baseline household assets (top 15%). In our population, such findings may reflect increasing value on the activities that older girls can contribute outside of school, and social norms of lower enrollment among older girls, thus requiring higher transfer amounts to justify school enrollment. Older girls from more financially stable households may also be more educated and make positive decisions regarding school enrollment, and may not require conditionality to the extent of younger girls when presented with cash transfer incentives. In comparison, households with fewer baseline assets and subjects enrolled in unconditional transfer programs may not have perceived sufficient household financial stability for subjects to enroll in school. The monetary

incentive may attract poorer families to engage in temporary behavior change, but this effect fails to potentiate over time.

We observe the next greatest improvement in school enrollment among younger students age 13-14 years with the lowest 20% of baseline household assets. This may be because younger girls are perceived to benefit more from education and are encouraged to attend school unless their family financial situation does not allow it, in which case a cash transfer would help those with the lowest household assets to conform to social norms.

Regarding subgroups to target for conditional cash transfer schemes, we find less clear patterns for specific targeting recommendations. Thus, for unconditional cash transfer programs in similar settings, we would recommend targeting older girls with relatively higher household assets assets or younger girls from households with lower assets. The effect of cash transfers on school enrollment outcomes might be augmented by supplementary social services, such as case workers, which may help overcome barriers to school enrollment among subgroups with low enrollment gains.

Effect of Total Household Transfer Amounts:

Our analysis of total household transfer amounts on school enrollment indicates that maximizing transfer amounts in conditional schemes appear to result in the greatest improvement in school enrollment, contrary to the original study conclusions that school enrollments gains were entirely attributable to the minimum transfer amount. Not only do our findings contrast with prior findings, we also find that the difference between

maximum and minimum transfer amounts in the conditional group becomes steadily more pronounced over time. Thus, we concur that conditional cash transfers appear to be more cost-effective, but we also find that long-term conditional transfers with higher transfer amounts are preferable compared to minimum transfer amounts to sustain improvements in school enrollment. This finding should encourage external donors to augment the effectiveness of cash transfer programs with limited funding, particularly in poorer regions.

Limitations

Our analysis is limited by the fact that we do not account for estimation and fundamental uncertainty in our heterogeneity analyses due to the limitations within the deployed algorithm; however, we incorporate estimation uncertainty in prior analyses of average effects (Appendix, Table 15). Additionally, our heterogeneity analysis assumes independence between subjects, which may be violated in certain households in which more than one subject was enrolled (page 8, Baird 2011). The original study does not report which girls, or what proportion of subjects, were from the same household, so we are unable to separate these effects or further account for correlation. Similarly, in our analysis comparing effects of total household transfer amounts, the maximum is based on the combined transfer amount to one subject and one guardian, as again we are unable to account for households receiving multiple payments for more than one subject enrolled. The original study notes that subjects in the same household may have been randomized to different conditional or unconditional treatment arms, resulting in potential spillover effects and bias in the findings regarding the effects of conditionality.

Another important limitation is the fact that the results of our analysis vary over time, which may explain to some extent the discrepancy between our findings and the original study. For example, if we had formed conclusions based on the effect of total household transfers at the end of the second term of 2009, our findings would have differed. However, as the difference between maximum and minimum transfers increases incrementally with time, we feel that school enrollment outcomes in subsequent terms would likely increase even further, further contradicting the original study findings. Similarly, in the heterogeneity analysis, we observe the greatest gains in enrollment among older girls with high baseline assets in the unconditional treatment arm; however, this subgroup was also observed to have lowest effects in early 2008. Thus, ending the study period in mid-2008 compared to early 2010 yields differing findings regarding optimal subgroups for targeting; similarly, extending the study period further into subsequent terms may lead to alternate conclusions.

Lastly, we would like to emphasize that even the wealthiest families in the study population are deeply impoverished by global standards—they are just better off than the very poor. Our findings should not be interpreted as evidence that cash transfers are ineffective among specific subgroups; they are conditional given the study setting and design, and should stimulate further study into the cause and generalizability of our conclusions.

Conclusions

In settings such as Zomba district in Malawi where information on income or household consumption is unreliable, indicators such as baseline household assets, schoolgirl age and grade are useful to guide program targeting and maximize the effect of cash transfer programs. Additionally, these indicators may be much easier to ascertain, since various asset indices are routinely collected routinely as proxies for income in sub-Saharan Africa (Sahn 2000).

Our findings suggest the need for qualitative studies to elucidate the varying influence of age and household assets. Furthermore, heterogeneity analyses of other cash transfer programs should be undertaken to determine whether our findings are unique to this study population, or whether they might be generalizable to other settings. We conclude that in Sub-Saharan African settings such as Malawi, cash transfer programs to improve school enrollment should be conditional and that minimal transfer amounts are suboptimal to maintain school enrollment gains for a sustained period of time. For unconditional cash transfer schemes in these settings, programs should consider recipients' age and baseline household index, as they are influential in affecting school enrollment outcomes. With improved targeting to improve cost-effectiveness, cash transfer programs can be vital in helping achieve the Millennium Development Goals of universal primary education and greater gender equity.

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Appendix

Treatment Heterogeneity - Unconditional Cash Transfers (UCT)

Table 1: Highest and Lowest Effects of UCT in Term 1, 2008

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	20	-3.27	7	0.38
2	21	-1.86	7	0.33
3	19	-1.97	8	0.32
4	18	-3.32	8	0.30
5	18	-1.76	6	0.30
6	21	-1.99	9	0.29
7	22	-1.41	10	0.28
8	18	-2.16	10	0.28
9	18	-1.79	9	0.27
10	19	-2.62	7	0.27
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	20	5.74	7	-0.17
2	18	5.11	7	-0.11
3	19	4.84	7	-0.08
4	16	4.68	6	-0.08
5	19	4.34	8	-0.08
6	15	3.77	5	-0.04
7	17	3.73	6	-0.04
8	22	4.12	9	-0.04
9	16	4.64	6	-0.03
10	17	6.03	7	-0.01

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 2: Highest and Lowest Effects of UCT in Term 2, 2008

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	18	-3.15	11	0.38
2	18	-2.82	11	0.37
3	18	0.27	11	0.33
4	20	-3.24	11	0.30
5	18	-2.16	10	0.30
6	18	-2.49	12	0.29
7	19	1.62	11	0.29
8	17	-2.96	11	0.28
9	17	-2.40	11	0.27
10	17	-2.23	11	0.27
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	20	-0.15	5	-0.02
2	16	4.68	6	-0.00
3	14	4.01	6	0.00
4	13	3.70	5	0.00
5	14	2.13	6	0.00
6	15	6.27	7	0.00
7	14	3.18	8	0.00
8	13	-3.26	6	0.00
9	13	2.98	6	0.00
10	14	1.09	6	0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 3: Highest and Lowest Effects of UCT in Term 3, 2008

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	18	-3.15	11	0.36
2	18	-2.82	11	0.35
3	18	3.00	10	0.33
4	20	4.97	10	0.33
5	18	0.27	11	0.31
6	20	3.18	12	0.30
7	18	-2.49	12	0.29
8	19	1.62	11	0.29
9	17	0.57	11	0.28
10	20	5.32	10	0.28
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	20	-0.15	5	-0.11
2	14	-0.07	5	-0.05
3	14	1.09	6	-0.02
4	21	-1.86	7	-0.02
5	14	2.59	6	-0.01
6	13	1.97	5	-0.01
7	14	2.03	6	-0.01
8	13	2.05	5	-0.01
9	13	0.93	5	-0.00
10	14	2.29	5	-0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 4: Highest and Lowest Effects of UCT in Term 1, 2009

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	19	4.34	8	0.53
2	20	3.18	12	0.45
3	19	5.11	10	0.44
4	19	4.06	10	0.37
5	18	5.11	7	0.34
6	18	3.90	10	0.29
7	20	5.32	10	0.22
8	19	4.29	11	0.22
9	18	4.56	11	0.21
10	17	5.20	12	0.19
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	18	-3.15	11	-0.67
2	19	-3.26	11	-0.56
3	21	-1.87	10	-0.55
4	20	-2.02	11	-0.50
5	19	-2.67	9	-0.50
6	18	-3.66	7	-0.48
7	21	-1.26	10	-0.46
8	19	-2.07	10	-0.43
9	18	-1.79	9	-0.43
10	18	-2.94	6	-0.42

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 5: Highest and Lowest Effects of UCT in Term 2, 2009

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	-2.73	4	0.81
2	19	4.34	8	0.69
3	14	-2.40	10	0.47
4	21	2.66	9	0.45
5	19	5.11	10	0.39
6	18	5.11	7	0.38
7	19	4.06	10	0.32
8	18	1.19	6	0.31
9	14	-2.39	9	0.31
10	17	5.20	12	0.30
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	19	-3.26	11	-0.76
2	18	-3.15	11	-0.70
3	20	-2.02	11	-0.67
4	21	-1.87	10	-0.62
5	19	-2.07	10	-0.55
6	19	-2.67	9	-0.54
7	21	-1.26	10	-0.53
8	20	-2.40	11	-0.52
9	19	-1.82	10	-0.52
10	18	-2.78	10	-0.49

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 6: Highest and Lowest Effects of UCT in Term 3, 2009

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	-2.73	4	0.82
2	19	4.34	8	0.65
3	21	2.66	9	0.48
4	14	-2.40	10	0.45
5	19	5.11	10	0.36
6	13	-2.93	5	0.35
7	20	3.18	12	0.34
8	13	-3.70	5	0.34
9	19	4.06	10	0.33
10	18	5.11	7	0.32
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	19	-3.26	11	-0.82
2	18	-3.15	11	-0.81
3	20	-2.02	11	-0.67
4	19	-2.67	9	-0.64
5	19	-2.07	10	-0.61
6	21	-1.87	10	-0.60
7	18	-2.78	10	-0.58
8	18	-1.79	9	-0.57
9	19	-1.82	10	-0.57
10	20	-2.40	11	-0.55

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 7: Highest and Lowest Effects of UCT in Term 1, 2010

Highest effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on enrollment
1	18	-3.15	11	0.00
2	13	2.98	6	0.00
3	20	3.18	12	0.00
4	14	3.18	8	0.00
5	14	4.01	6	0.00
6	17	5.20	12	0.00
7	13	-1.15	8	0.00
8	14	3.83	7	0.00
9	14	-2.73	4	0.00
10	14	1.94	8	0.00
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	13	-1.41	6	-0.05
2	17	-2.05	7	-0.05
3	14	-1.32	8	-0.05
4	18	1.35	10	-0.05
5	14	0.83	7	-0.05
6	14	-0.69	7	-0.05
7	15	-1.36	6	-0.05
8	15	2.03	8	-0.05
9	15	0.94	8	-0.05
10	16	-1.39	8	-0.05

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Treatment Heterogeneity - Conditional Cash Transfers (CCT)

Table 8: Highest and Lowest Effects of the CCT in Term 1, 2008

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	20	-2.33	9	0.17
2	18	-3.15	11	0.17
3	17	-0.72	7	0.17
4	18	0.46	9	0.17
5	18	-2.82	7	0.17
6	19	4.43	5	0.17
7	21	-0.55	9	0.17
8	19	-2.67	9	0.17
9	18	-2.87	7	0.17
10	18	-3.27	6	0.17
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	3.18	8	0.00
2	15	5.35	7	0.00
3	14	5.35	8	0.00
4	14	3.41	7	0.00
5	15	5.30	9	0.00
6	14	4.87	9	0.00
7	13	-0.11	7	0.00
8	13	-3.26	6	0.00
9	13	2.98	6	0.00
10	14	1.09	6	0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 9: Highest and Lowest Effects of the CCT in Term 2, 2008

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	15	-0.22	7	0.13
2	15	-2.11	5	0.13
3	17	3.69	9	0.13
4	17	-0.72	7	0.13
5	15	-3.43	7	0.13
6	15	-2.22	6	0.13
7	15	0.32	7	0.13
8	16	1.82	6	0.13
9	17	3.18	10	0.13
10	20	3.18	12	0.13
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	3.18	8	0.00
2	15	5.35	7	0.00
3	14	5.35	8	0.00
4	14	3.41	7	0.00
5	15	5.30	9	0.00
6	14	4.87	9	0.00
7	13	-0.11	7	0.00
8	13	-3.26	6	0.00
9	13	2.98	6	0.00
10	14	1.09	6	0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 10: Highest and Lowest Effects of the CCT in Term 3, 2008

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	19	-1.47	7	0.14
2	20	2.90	9	0.14
3	19	-0.28	8	0.14
4	19	0.57	8	0.14
5	16	-1.39	8	0.14
6	17	-0.72	7	0.14
7	17	-0.11	8	0.14
8	19	4.34	8	0.14
9	18	0.46	9	0.14
10	16	-0.91	4	0.14
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	15	6.27	7	0.00
2	14	3.18	8	0.00
3	15	5.35	7	0.00
4	14	5.35	8	0.00
5	14	3.41	7	0.00
6	15	5.30	9	0.00
7	14	4.87	9	0.00
8	13	-0.11	7	0.00
9	13	-3.26	6	0.00
10	13	2.98	6	0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 11: Highest and Lowest Effects of CCT in Term 1, 2009

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	20	3.18	12	0.63
2	14	-2.40	10	0.50
3	17	2.69	11	0.42
4	17	-0.01	11	0.35
5	17	1.46	11	0.35
6	17	1.03	10	0.30
7	15	1.44	11	0.29
8	17	5.20	12	0.27
9	13	-1.15	8	0.26
10	15	-0.71	9	0.26
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	18	-3.66	7	-0.72
2	18	-2.82	7	-0.69
3	20	-2.33	9	-0.65
4	20	-3.43	9	-0.61
5	21	-1.87	10	-0.56
6	18	-2.94	6	-0.56
7	19	-2.67	9	-0.53
8	17	-3.27	7	-0.49
9	19	-3.23	10	-0.47
10	20	-2.23	7	-0.47

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 12: Highest and Lowest Effects of CCT in Term 2, 2009

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	18	-3.15	11	0.00
2	16	-1.39	8	0.00
3	15	-0.22	7	0.00
4	13	2.98	6	0.00
5	17	-0.11	8	0.00
6	15	0.72	9	0.00
7	15	-2.22	6	0.00
8	15	0.32	7	0.00
9	15	5.30	9	0.00
10	14	5.35	8	0.00
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	5.35	8	0.00
2	15	5.30	9	0.00
3	15	0.32	7	0.00
4	15	-2.22	6	0.00
5	15	0.72	9	0.00
6	17	-0.11	8	0.00
7	13	2.98	6	0.00
8	15	-0.22	7	0.00
9	16	-1.39	8	0.00
10	18	-3.15	11	0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 13: Highest and Lowest Effects of CCT in Term 3, 2009

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	-2.73	4	0.82
2	19	4.34	8	0.65
3	21	2.66	9	0.48
4	14	-2.40	10	0.45
5	19	5.11	10	0.36
6	13	-2.93	5	0.35
7	20	3.18	12	0.34
8	13	-3.70	5	0.34
9	19	4.06	10	0.33
10	18	5.11	7	0.32
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	5.35	8	0.00
2	15	5.30	9	0.00
3	15	0.32	7	0.00
4	15	-2.22	6	0.00
5	15	0.72	9	0.00
6	17	-0.11	8	0.00
7	13	2.98	6	0.00
8	15	-0.22	7	0.00
9	16	-1.39	8	0.00
10	18	-3.15	11	0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 14: Highest and Lowest Effects of CCT in Term 1, 2010

Highest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	18	-3.15	11	0.00
2	16	-1.39	8	0.00
3	15	-0.22	7	0.00
4	13	2.98	6	0.00
5	17	-0.11	8	0.00
6	15	0.72	9	0.00
7	15	-2.22	6	0.00
8	15	0.32	7	0.00
9	15	5.30	9	0.00
10	14	5.35	8	0.00
Lowest Effects				
	Age (years) (13 to 22)	Assets (-3.69 to 6.83)	Grade (3 to 12)	Estimated Effect on Enrollment
1	14	5.35	8	0.00
2	15	5.30	9	0.00
3	15	0.32	7	0.00
4	15	-2.22	6	0.00
5	15	0.72	9	0.00
6	17	-0.11	8	0.00
7	13	2.98	6	0.00
8	15	-0.22	7	0.00
9	16	-1.39	8	0.00
10	18	-3.15	11	0.00

Notes: Estimated effects represent increases or decreases in probability relative to a hypothetical baseline case = 13 year old control school girl in the Grade 3 with zero household assets.

Table 15: Program Impact on Teacher-Reported School Enrollment

	Dependent variable: =1 if enrolled in school during relevant term						
	Term 1, 2008 (1)	Term 2, 2008 (2)	Term 3, 2008 (3)	Term 1, 2009 (4)	Term 2, 2009 (5)	Term 3, 2009 (6)	Term 1, 2010 (7))
Conditional Treatment	0.043***	0.044***	0.061***	0.094**	0.132***	0.113***	0.058*
	(0.015)	(0.016)	(0.018)	(0.041)	(0.035)	(0.039)	(0.033)
Unconditional Treatment	0.020	0.038**	0.018	0.027	0.059	0.033	0.001
	(0.015)	(0.017)	(0.023)	(0.037)	(0.037)	(0.039)	(0.036)
N	2,023	2,023	2,023	852	852	852	847
Mean(Control)	0.816	0.758	0.663	0.460	0.479	0.417	0.747
P-value(Cond.= Uncond)	0.172	0.731	0.066	0.074	0.013	0.019	0.105

*p<0.1; **p<0.05; ***p<0.01

The Dependent variable is whether the teacher reported the core respondent being enrolled in school for the relevant year/term. Term 1, 2010 is the first term after the program ended. Columns (4) - (8) are restricted to the sub-sample of core respondents sampled for Round 3 school survey who are also both part of the panel data set and part of the school survey panel. 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.