The Education Impacts of Cash Transfers for Children with Multiple Indicators of Vulnerability

David K. Evans, Charles Gale, and Katrina Kosec

Abstract

As more countries approach universal primary school enrollment, the remaining children out of school merit special attention. Multiple studies have demonstrated that cash transfers boost educational outcomes for children on average, but do children with multiple indicators of vulnerability benefit from these safety net programs? This study draws on a randomly assigned pilot of a community implemented cash transfer program targeted to households with low socioeconomic status in Tanzania to examine the educational impacts of cash transfers for children facing different challenges. We find that on average, being assigned to receive cash transfers significantly boosts children's school participation (between 8 and 10 percentage points) and primary completion rates (between 14 and 16 percentage points). But we provide suggestive evidence that these gains are unequally distributed across children. The poorest children in our sample are more likely to experience gains along the extensive margin (i.e., higher likelihood of ever attending school), whereas the less poor children are more likely to experience gains along the intensive margin (i.e., higher likelihood of primary school completion). Girls and boys benefit approximately equally. Finally, educational gains are concentrated among students who were performing better in school at baseline. Cash transfers benefit vulnerable children, but they do not benefit all vulnerable children equally, nor in the same ways.

Keywords: cash transfers, education, Tanzania, poverty

JEL: C93, I21, O12

Center for Global Development
www.cgdev.org

Working Paper 563
December 2020

The Education Impacts of Cash Transfers for Children with Multiple Indicators of Vulnerability

David K. Evans
Center for Global Development; devans@cgdev.org

Charles Gale
Harvard Graduate School of Education; chagale@gmail.com

Katrina Kosec
International Food Policy Research Institute; k.kosec@cgiar.org

This study benefitted at various stages from experts at the World Bank, the International Food Policy Research Institute (IFPRI), the Tanzania Social Action Fund (TASAF) and elsewhere. At TASAF, the evaluation has been supported by the Executive Director Ladislaus Mwamanga, as well as the former Executive Director Servacius Likwelile. Amadeus Kamagenge led TASAF input to the evaluation, and his entire team has contributed with substantive and logistical support. We received financial support from the CGIAR Research Program on Policies, Institutions, and Markets led by IFPRI, the International Initiative for Impact Evaluation (3ie), the Strategic Impact Evaluation Fund (SIEF), and the Trust Fund for Environmentally and Socially Sustainable Development (TFESSD). Each author contributed equally to the paper. This work benefited from helpful comments from Felipe Barrera-Osorio, Shelby Carvalho, Emmerich Davies, Daniel Gilligan, Lawrence Katz, Sikandra Kurdi, and Eric Taylor. Amina Mendez Acosta provided valuable research assistance. Comments and suggestions are welcome.

The Center for Global Development is grateful for contributions from the Bill & Melinda Gates Foundation in support of this work.

David K. Evans, Charles Gale, and Katrina Kosec, 2020. "The Education Impacts of Cash Transfers for Children with Multiple Indicators of Vulnerability." CGD Working Paper 563. Washington, DC: Center for Global Development. https://www.cgdev.org/publication/education-impacts-cash-transfers-children-multiple-indicators-vulnerability

The data from the Tanzania conditional cash transfer pilot are publicly available at https://microdata.worldbank.org/index.php/catalog/2669. More information on CGD's research data and code disclosure policy can be found here: www.cgdev.org/page/research-data-and-code-disclosure.

Center for Global Development
2055 L Street NW
Washington, DC 20036

202.416.4000
(f) 202.416.4050

www.cgdev.org

The Center for Global Development works to reduce global poverty and improve lives through innovative economic research that drives better policy and practice by the world's top decision makers.

1 Introduction

With the attention of international institutions turning to the quality of global education (World Bank, 2018, 2019a), one might imagine that access to school is universal. However, the completion rate for primary school in low-income countries is just 65 percent: a large minority of children still fail to complete primary school or to reap the gains of education.[1] How can policymakers and donors help the last third of children in the world's poorest countries complete their primary education? Cash transfers, which are usually targeted to low-income households, have been used in many parts of the world with the goal of improving educational outcomes, and many studies have verified that they increase educational access for the average beneficiary child, in both the short and long runs (Baird et al., 2014; Molina Millán et al., 2019). But as countries seek to expand educational access to the remaining out-of-school children, evidence on their effectiveness at reaching children who face particularly high levels of poverty and multiple vulnerabilities grows more important. In this paper, we test the impact of cash transfers on schooling outcomes for low-income households in a low-income country. We also test the efficacy of cash transfers for children facing varied challenges, in addition to the fact that all were identified on the basis of their poverty. These varied challenges including those facing girls, children from the very poorest households, orphans, and children with poor initial school performance. We then situate these impacts within the context of one hundred previous evaluations of cash transfers and education. To test the impact, we use a randomized controlled trial of conditional cash transfers targeted by and delivered through community committees in Tanzania, a country with a 68 percent primary school completion rate, just above the average for low-income countries.[2] In order to receive the transfers, households needed to ensure that children aged 7–15 were enrolled in school and attended at least 80 percent of the time. They also had to meet required numbers of health clinic visits (Evans et al., 2019).

With a limited initial budget, the Government of Tanzania worked with researchers to pilot conditional cash transfers in 40 randomly assigned villages, with another 40 villages serving as a comparison group. We compare beneficiaries in treatment villages with non-beneficiaries in comparison villages after 1.75 years of transfers and again after 2.75 years of transfers. We report the robustness of our results to corrections for multiple hypothesis testing.

We find that cash transfers boosted school participation at both points in time and that they increased the proportion of children who had ever attended school, suggesting an impact on the extensive margin of schooling. We observe no impacts on children's absenteeism, which may be unsurprising given that reported baseline attendance was 88 percent, exceeding program condition requirements. We also observe an increase in primary school completion of 16 percentage points for beneficiary children relative to non-beneficiary children after 2.75 years of transfers.

[1]Statistics for primary school completion in this introduction are from World Bank (2019b), the latest available date.

[2]The primary completion rate of 68 percent is for 2020. Tanzania was classified as a low-income country until mid-2020, at which point the World Bank reclassified it as a lower-middle income country (Battaile, 2020).

However, we observe no increase in secondary enrollment. Lower secondary school completion rates were low overall at the time of endline, just 40 percent, and likely much lower in our rural villages.[3] The broad pattern of our results is robust to correction for multiple hypothesis testing.

We further observe clear suggestive evidence that these effects are driven by certain sub-groups within the broad cateogry of poor children targeted by the intervention. By some measures, the most vulnerable children benefited most from the program: the poorest beneficiary children (and all beneficiary children were relatively poor) as proxied by an asset index at baseline experienced statistically significant gains in school participation while their less-poor counterparts did not. Despite this, the gains in primary school completion were reversed, with less poor children showing gains of 19 percentage points at endline, and the poorest children showing insignificant gains half that size. This suggests that education gains on the extensive margin (i.e., increasing the likelihood of ever having attended school) were concentrated among the poorest children, whereas gains on the intensive margin (i.e., reaching primary school completion) may have been concentrated among the less poor children. Further, treatment induced children that had passed their Standard IV exam at baseline (i.e., higher performing students) to complete primary school and attend secondary school, whereas it did not have statistically significant effects on primary completion by children who had not passed this exam.

After 2.75 years of transfers, girls and boys were roughly equally likely to experience gains in school participation and primary completion—with improvements that are only slightly larger, and statistically indistinguishably so, for girls (for both participation and completion). Children who were orphans realized statistically significantly smaller (compared to non-orphans) improvements in school participation after 2.75 years of transfers. Orphans also observed improvements in primary completion that were larger in magnitude compared to those experienced by non-orphans (with significant impacts in both groups that were not significantly different from each other). The pattern of results for orphans is similar to the impact for less poor youth, which reflects the fact that in our data, orphans are only slightly more likely to reside in worse off households.

This research contributes to two principal literatures. The first is on the effectiveness of cash transfers in achieving human capital objectives. A recent review identifying 20 studies measuring the impact of cash transfers on school attendance found that nearly two-thirds found significant positive impacts (Bastagli et al., 2016; Hagen-Zanker et al., 2016). A meta-analysis likewise found positive impacts for both conditional and unconditional transfer programs (Baird et al., 2014). However, few of those studies separately identied impacts for the poorest children or for those who faced other vulnerabilities. From a sample of more than one hundred cash transfer evaluations with educational outcomes drawn from Bastagli et al. (2016) and Baird et al. (2014), we find that while most (65 percent) report outcomes separately by gender, only 25 percent differentiate by poverty level within cash transfer recipients, and only 5 percent by

[3] The number for secondary school completion is from the World Development Indicators for Tanzania for 2012, the year the endline survey was implemented.

baseline student performance. None of these previous studies separated results by orphan status, although some programs do include orphans as a target recipient group. Our study shows that conditions may not by themselves overcome all constraints for the most vulnerable children, and how the effectiveness of transfers varies across children with multiple vulnerabilities.

The second relevant literature explores ways to ensure educational access for the most vulnerable children, often facing a variety of vulnerabilities. Recent research has examined a range of interventions to reach children who remain out of school. Some of these focus on reaching children in the poorest communities (Fazzio et al., 2020) or ensuring that the most vulnerable girls remain in school (Sabates et al., 2020), while others focus on boosting educational outcomes for orphans (Cho et al., 2017; Hallfors et al., 2011; Thomas et al., 2020) or for lower performing children (Banerjee et al., 2017; Duflo et al., 2020). Our study examines the efficacy of a commonly used policy instrument in boosting outcomes for children with a range of vulnerabilities.

These findings suggest that to achieve truly universal school enrollment, policymakers will likely need to draw on an array of interventions to get the most vulnerable children into school and to help them stay there. Further, the relative importance of different vulnerabilities—gender, parent death, relative poverty, academic performance—will vary across contexts.

While our results provide insights on the role of multiple vulnerabilities in moderating the impacts of cash transfers, they come with limitations. We observe outcomes 2.75 years after the initiation of transfers, but the ultimate objectives of school completion and improved life outcomes come much later. A handful of studies identify the long-term human capital impacts of cash transfer programs (Molina Millán et al., 2019; Barrera-Osorio et al., 2019; Molina Millán et al., 2020; Araujo et al., 2019), and more work is needed on identifying those impacts for the most vulnerable children and youth in particular. In addition, while receipt of cash transfers was randomly assigned, vulnerabilities are not, and so we cannot rule out the existence of other differences between, for example, orphans and non-orphans that could confound our results. That said, our results do indicate that many children are not reaping the educational benefits of cash transfers.

The rest of this paper proceeds as follows. Section 2 characterizes the context and the intervention. Section 3 describes the data, and Section 4 discusses the identification strategy. Section 5 presents the results, including robustness to adjustments for multiple hypothesis testing. Section 6 discusses the implications of our findings in the context of atwo reviews of one hundred papers. Finally, Section 7 concludes.

2 Background

2.1 Context

Tanzania is a country with high rates of poverty and other vulnerability. As of 2018, more than one in four people were classified as poor. Ten years earlier, close to the start of the program under evaluation, that number was more than one in three (Belghith et al., 2019). At the same time, many children face vulnerabilities in addition to poverty. Previous evidence from East Africa—both Kenya and Tanzania—suggests that orphaned children (i.e., children with at least one deceased parent, as "orphan" is commonly defined in the literature) have worse educational outcomes (Evans and Miguel, 2007; Ainsworth et al., 2021). In many countries in Sub-Saharan Africa, girls continue to have worse educational outcomes than boys (Evans et al., 2021). Children with poorer educational performance are more likely to drop out of school (Akresh et al., 2012). Combinations of these vulnerabilities can result in multiple exclusions and further decreases in access to educational opportunities (Moodley and Graham, 2015; World Bank, 2018).

Cash transfers may interact distinctly across these different vulnerabilities. Transfers most obviously alleviate a poverty constraint. However, in the face of relaxed constraints, parents may still make choices about which children's education to invest in more heavily. How cash transfers affect educational outcomes for different children and youth will depend on the interaction between relaxing the poverty constraint (to a degree) and other characteristics of the child and the household. In this context, we collaborated with the government of Tanzania to evaluate the impact of cash transfers.

The Tanzania Community-Based Conditional Cash Transfer (CB-CCT) pilot was conceived in 2007 in discussions between the Tanzania Social Action Fund (TASAF) and World Bank officials. TASAF is a government program tasked with promoting economic and social development,[4] established in 2000. In its early stages, the program provided funding and support to communities who applied for support in implementing locally-managed infrastructure projects, such as rehabilitating schools and health clinics. Despite receiving extensive donor funding, according to Hickey et al. (2019), the emphasis on community-based programming reflected stated government goals of self-reliance and community participation in markets. The CB-CCT pilot was one of several projects the government wanted to test for possible scale-up,[5] and enlisted the help of World Bank researchers.

[4]Though nominally pro-poor, Baird et al. (2013) find regressive elements in the process by which villages apply to the program, in that communities with knowledge of program availability and requirements may be slightly better off than the poorest of the poor. In this way the program should be thought of as "moderately pro-poor" rather than explicitly.

[5]In 2012 the government began the Productive Social Safety Net (PSSN), a nationwide CCT that aimed to eventually reach five million people (Hickey et al., 2019) that was informed by preliminary findings from the CB-CCT pilot

2.2 Selection of Villages

Officials overseeing the design of the CB-CCT decided to leverage TASAF connections to villages through its community-based targeting approach, prioritizing villages that had successfully managed projects and thus had experience with procurement, budgeting and contracting. This is important to note for the purposes of external validity, as findings may not generalize to other villages in Tanzania without this experience. All villages in selected districts were eligible for sub-interventions through the first phase of TASAF, though villages that participated in one pilot were not eligible for a different one. Given the policy environment, it is important to establish that villages participating in the CB-CCT pilot were not also receiving other government or donor funding. Our houehold and school-based data collections showed this. For example, schools in villages participating in the CB-CCT pilot reported receiving no TASAF project funding during the intervention period.

Three districts (first level administrative division) were chosen for the pilot: Chamwino, Bagamoyo and Kibaha. Two of these are relatively close to the largest city Dar es Salaam while the third is in the Dodoma region. 80 eligible study villages across the three districts were randomized into treatment and control groups of 40 villages each, stratified on village size and district.[6] Randomization was conducted by the data collection firm Economic Development Initiatives. Control villages were told they would begin receiving the program in 2012 after the endline survey was conducted.

2.3 Selection of Households

Administration, oversight and enforcing of conditions were carried out by community management committees (CMCs), which were elected by villages. The CMCs were trained to undertake a survey where they collected data on the housing conditions and access to food for approximately the poorest 50 percent of households in each community. Using this information, TASAF ranked households to form a list which they proposed to the CMCs, and compiled a final list of beneficiaries and would-be beneficiaries. Household poverty status was verified on a sample basis by TASAF (see Table 1). On average, the number of beneficiary households represented 23 percent of the total households in each village. Due to a combination of household refusal and last-minute changes in community decisions, roughly 9 percent of households in treatment villages did not receive treatment.[7] Data from the midline survey showed that 58 percent of households reported a CMC member was a neighbor, and 23 percent reported that a CMC member was a blood relative.[8]

[6] At baseline, villages ranged from 64 to 10,078 households. The average size was 980 households, and the median size was 560 households

[7] These individuals remain in our sample.

[8] Household survey data available through the World Bank microdata catalogue https://microdata.worldbank.org/

2.4 Intervention

TASAF began delivering cash transfers in January 2010. Payments to households were made every 2 months for the duration of the pilot (2 years and 9 months, or from January 2010 through September 2012), as long as households remained eligible. The amount of the transfer was determined based on the food poverty line, and was initially 3,600 Tanzanian shillings (approximately US$3 at the onset of the program) per month for each child up to 15 years of age, and 7,200 TSh (approximately US$6) per month for elderly persons aged 60 and over. We estimate that transfers amounted to roughly 13 percent of total household expenditures over the period. The average initial payment was reported to be about 17,400 TSh (US$14.50). Transfers were adjusted over time for inflation, such that the average (most recent) transfer amount was 25,000 TSh per household, which would equal roughly 150,000 Tsh over the course of a year.

Total annual household education expenditures for households with at least one 6 to 12 year old at baseline, adjusted using a CPI conversion, increased over the evaluation period; these households were spending about 46,000 TSh in annual education expenditures at baseline, on average. This amount increased to 83,000 TSh by endline in control villages, while it increased to 67,000 TSh in treatment villages. For households with at least one 13 to 18 year old at baseline, annual education expenditures increased from 51,000 to 86,000 TSh in control villages and 40,000 to 77,000 TSh in treatment villages. Thus, the transfers were more than enough to cover all annual educational expenditures. (Transfers were not solely targeted towards educational outcomes.)

While payments were made at the household level, conditions applied at the individual level. The conditions were that children age 0-5 would visit health clinics at least 6 times per year, children age 7-15 be enrolled in school with 80% attendance, and elderly persons visit clinics at least 1 time per year. The CMC oversaw monitoring, which involved collecting forms from health clinics and schools and maintaining records, delivering warnings to households that were found to be out of compliance, making regular visits to beneficiary households. Monitoring was conducted every four months and records were submitted to TASAF and entered into a centralized database, where a final payment list was generated.

If households were found to be out of compliance, a first warning was issued, and if after 8 months beneficiaries were still out of compliance, payments were reduced by 25% and a second warning was issued. If after a year households were still out of compliance they were suspended from the program, though they were allowed to return upon review by the CMCs and TASAF. During the midline and endline surveys, few households (2-3%) reported receiving payments that were smaller than usual due to program non-compliance. Households also reported high satisfaction with their CMCs. Roughly 93% of households reported receiving their transfer from a community office during the midline.

3 Data

In this section we describe the data collection process. The impact evaluation comprised three waves of household-level data collection. The baseline was carried out during January–May 2009, prior to the January 2010 start of transfer payments. The midline was carried out from July–September 2011 (approximately 2.5 years after baseline, and 1.75 years after treatment began), and the endline was carried out during August-October 2012 (approximately 3.5 years after baseline, and 2.75 years after treatment began). The endline concluded prior to the extension of transfers to control villages. Table 1 presents the chronology of both the program and the evaluation.

The baseline survey included 1,764 households comprised of 6,918 individuals. The number of households to be interviewed per community was set at 25, determined by power calculations and known effect sizes from other CCT studies. Detailed individual and household-level data were collected on assets and consumption, health-seeking behavior, education and other factors. During the endline survey, enumerators also collected data on schools and health clinics in treatment and control villages.

Members of the enumeration team were not identified or affiliated with representatives from TASAF or the CMCs, and all interviews were conducted without TASAF representatives present. In addition, steps were taken during questionnaire construction to limit respondent bias. For example consumption items were grouped into lists and not singled out or highlighted, such as in the case of temptation goods (i.e., cigarettes or alcohol). Additionally we present evidence in Table A1 from the school survey on verified child attendance measures, which suggests that households reported faithfully on measures affected by program conditions.

3.1 Outcomes

The main individual-level education variables analyzed as outcomes in this study include variables related to school attendance and to school progression and completion. Attendance variables include school participation, which takes a value of 1 for those who are reported as currently attending and zero otherwise; ever attended, which takes a value of 1 for those who have ever attended school and zero otherwise; missed last week, which takes a value of 1 if the child missed school in the last schooling week for a reason other than public holiday, school closure or teacher absence, and is measured for the subpopulation of children who report currently participating in school. Progression and completion variables include completed primary, which takes a value of 1 for those who are reported to have completed primary school and zero otherwise; and attending secondary, which takes a value of 1 for those who report currently attending a secondary grade and zero otherwise. Over 90% of respondents to the household survey are either the household head or the spouse of the household head. The research team also collected a verified measure of child attendance by compiling a list of all children under 17 in study households at baseline and midline, and checking against attendance logs kept by all schools known to be in study villages.

We examine our participation outcome by all dimensions of vulnerability except for baseline exam performance, and we examine completion and progression outcomes by all vulnerability measures. Of course, indicators of vulnerability are not independent (Table A1). The correlation between some vulnerability measures at baseline is low; for example, the relationship between the female dummy and each of the other measures is under 0.1, for both age groups. The correlation between orphan status and poverty is likewise weak (0.07). On the other hand, the correlation between orphanhood and foster status is moderate (0.18 and 0.32 for the 6 to 12 and 13 to 18 age groups, respectively) and the correlation between low exam performance (not passed) and low education of the household head is 0.30 for the 6 to 12 age group.

4 Estimation Strategy

Given that assignment to treatment was random, we can estimate the causal intent-to-treat effect of the CCT. We estimate midline and endline effects for the CCT treatment using the following pooled specification:

$$Y_{ihvt} = \alpha_0 + \beta_1(T_v * M_t) + \beta_2(T_v * E_t) + \delta_1 M_t + \delta_2 E_t + \eta_i + \epsilon_{ihvt} \tag{1}$$

Where Y_{ihvt} is the outcome for individual i in household h and village v at time t, M and E are survey phase dummies for the midline and endline respectively, T_v=1 if the individual lives in a treatment village, and 0 otherwise, η_i are individual fixed effects, and e_{ihvt} is an idiosyncratic individual-level error term. We recover causal estimates of the effects of treatment at midline and at endline through the coefficients β_1 and β_2, respectively. All regressions include standard errors clustered at the village level, which is the unit of randomization.

4.1 Selection of ages

Our analytic strategy requires identifying a cohort of children for whom to estimate treatment effects. Our main estimation sample for school participation and other attendance-related outcome includes children aged 6–12 at baseline. These children would be between 6–13 when their household first began receiving transfers (which occurred 8–12 months after baseline), 8–15 by midline (approximately 2.5 years after baseline), and between 9–16 by endline (approximately 3.5 years after baseline). This age range encompasses a cohort that was entirely subject to the program conditions at midline (these applied to 7–15 year olds), and in most cases also at endline (i.e. with those who turned 16 being the only exception).

The CB-CCT pilot did not have an explicit condition on children completing primary schooling, but this was a goal of the program. As such, we choose the 13–18 age group to represent a wide distribution of ages of children whose completion of primary school, or advancement to secondary school, could ostensibly be affected by the program (either due to the conditions directly incentivizing them to stay in school up to age 15, or due to the indirect effect of having greater resources in their household from the transfers). These children would be between 13–19 when their household first began receiving transfers (which occurred 8–12 months after baseline), 15–21 by midline (approximately 2.5 years after baseline), and between 16–22 by endline (approximately 3.5 years after baseline). The timeline in Table 1 clarifies the age ranges at various points in time that our cohorts that were 6–12 years old at baseline, or 13–18 years old at baseline, exhibited.

4.2 Outcome of the randomization

While villages were randomized into treatment and control, it is possible that some characteristics of treatment villages are significantly different than those of control villages. If imbalances were present, one might worry if they—rather than treatment itself—explain differences in our outcomes. We address this concern in two ways. First, we show that randomization generally led to balance across treatment and control villages. Second, we use individual fixed effects to account for baseline imbalances.

In Table 2, we examine differences in baseline means between treatment and control groups for an array of child (we consider 6–12 year olds and 13–18 year olds separately, as our regressions analyze them separately), household, and village characteristics. In total, we examine 23 characteristics; we find baseline imbalances at the 0.10 level of significance or higher for three characteristics, all outcomes among the sample of 13–18 year olds: whether the child is an orphan (i.e., at least one parent is deceased), whether they both sat for and passed a Standard IV (i.e., 4th grade) exam, and whether or not they have attended secondary school. Children aged 13–18 living in treatment communities are about 7 percentage points less likely to be orphans, but 11 percentage points less likely to have sat for and passed a Standard IV exam, and 9 percentage points less likely to have attended secondary school (both signs that they are worse off). Of the three, only two (the Standard IV exam and attending secondary school indicators) are significant at the 0.05 level or higher, and only one (the Standard IV exam indicator) is significant at the 0.01 level or higher. We deal with these imbalances by always including individual fixed effects in our regressions. These absorb not only these imbalances, but also the effects of all other time-invariant unobservables.

Despite imbalances among the population of children aged 13–18 at baseline, we find balance for all characteristics of 6–12 year old that we consider in Table 2, including all of their outcomes. Examining household-level characteristics, we find no imbalances on whether or not the head has education, whether or not the household is in the bottom three quintiles of an asset index constructed using the first principal component of a principal components analysis (PCA), or whether the head believe school quality is good or excellent. Considering village characteristics, we further see that treatment and control villages do not exhibit any statistically significant differences with regard to size (number of households), connectedness to higher levels of government (whether the village houses the ward, or whether the village executive officer (VEO) lives in the village), frequency of village meetings (number held in the last year—where local leaders are supposed to hold four), or poverty level (specifically, the village's poverty rank in TASAF's database, or the village's poverty score in TASAF's database).

4.3 Attrition

Between baseline and midline, 8.6 percent of households attrited from the sample, and between baseline and endline, 13.2 percent of households attrited. Table 3, columns 1–4 consider correlates of a household remaining in the sample at midline (columns 1–2) and at endline (columns 3–4). We estimate two specifications for each follow-up round: one with only district fixed effects and a dummy for living in a treatment village, and another with district fixed effects, a dummy for treatment, and interactions of treatment with household-level characteristics including head gender, head age and age squared, and dummies for the head having no education and for the household being in the bottom three quintiles of asset wealth.

In no cases is treatment, or its interaction with any household-level covariates, a statistically significant predictor of a household remaining in the sample. While low wealth households are significantly more likely to remain in the household, we cannot reject that wealth has the same effect on remaining in the sample across treatment and control villages. For each of the regressions with interactions with treatment, we further compute the F-statistic for the joint significance of treatment and all of these interaction terms. At both midline (column 2) and endline (column 4), we can reject that treatment and its interactions with covariates jointly predict a household remaining in the sample. Overall, we conclude that treatment does not affect which households remain in our sample.

We additionally conduct a similar, individual-level attrition analysis for each of our two groups of children: those aged 6–12 at baseline, and those aged 13–18 at baseline. For these groups, in addition to interacting treatment with household-level variables, we also interact it with the child's own gender, age, and age squared. For children aged 6–12, treatment alone is not a statistically significant predictor of remaining in the sample either at midline (column 5) or at endline (column 7). While at endline, the interaction of treatment and being male is statistically significant and negative in column 8 (indicating that boys are less likely to remain in the sample as a result of being treated), there are no other interaction terms that have a statistically significant effect on remaining in the sample at endline, and we find no statistically significant coefficients on treatment or its interactions with covariates at midline (column 6). At midline, we also see that treatment and its interactions with covariates are not jointly significant predictors of remaining in the sample (column 6)—though at endline, an F-test reveals joint significance at the 0.10 level (column 8).

For children aged 13–18 at baseline, we in no cases find a statistically significant coefficient on an interaction term between treatment and either household- or child-level covariates. The coefficient on treatment itself is statistically significant in column 9, where we include only a dummy for treatment and district fixed effects (indicating that treatment predicts an 11.4 percentage point increase in the probability of being in the sample at midline). However, it is not significant in a similar specification for endline (column 11), and it is not statistically significant in specifications that flexibly allow the effects of treatment to vary according to household- and individual-level covariates (columns 10 and 12). F-tests in this case provide no evidence of the joint significance of treatment and its interactions at endline (column 12), though we cannot reject that they are jointly significant at midline (column 10). Overall, these findings of some cases in which treatment and its interactions with covariates predict remaining in the sample for some individuals motivates our use of individual fixed effects.

5 Results

In this section, we present the impacts of Tanzania's CCT program on a variety of education-related outcomes. First, we consider outcomes related to school participation and attendance. Second, we examine the impacts on school progression and completion. Then, we consider how impacts vary across an array of individual and household-level markers of vulnerability. These include child gender, asset wealth, head education, foster status, orphan status, and baseline exam performance.

5.1 School participation and attendance

We first consider the impacts of the CCT program on school participation and attendance among children aged 6–12 at baseline, in Table 4. We consider an indicator for the child currently participating in school (i.e., for being currently enrolled) (column 1), for having ever attended school (whether or not they currently attend) (column 2), and for the parent reporting that they missed school last week for a reason other than school holiday or closure (e.g., due to illness, having to work, or another reason unrelated to a decision by school officials) (column 3). The coefficient on treatment, along with the 90 percent confidence interval around the estimate, is depicted for each of these regressions in Figure 1.

The CCT program appears to have affected enrollment of children with a baseline age of 6–12 more than their attendance conditional upon enrollment. At midline, treatment led to an 8 percentage point increase in the likelihood of participating in school (significant at the 0.01 level), which rose to a 10 percentage point increase by endline (significant at the 0.05 level). It is important to take the ages of the children at midline and at endline into account; they would be between ages 8 and 15 at midline, and between ages 9 and 16 at endline. Children in Tanzania typically start Standard I at age 7, though we counted attendance of pre-primary school as attending school as well. Primary school lasts 7 years, such that children would be transitioning to secondary school at age 14 (there are 4 years of ordinary secondary school, and 2 years of advanced secondary school). Thus, we find no evidence that the effect size of the CCT decreases between midline and endline, even though a larger share of this sample has past the age of 14—when school drop out becomes more likely as it marks the beginning of transition to secondary school.

We identify similar effects on whether children ever enrolled in school. Specifically, the CCT leads to an 8 percentage point increase at midline that expanded to a 10 percentage point increase at endline (both effects statistically significant at the 0.01 level).

Despite increases in enrollment, reflected in higher rates of school participation and a greater likelihood of having ever attended school, we find no evidence from parental self-reports about the past week that the likelihood of missing school—conditional on participation—changed.

The cash transfer was conditioned on households keeping their children between the ages of 7 and 15 enrolled and attending school at least 80 percent of the time. Though there were multiple, widely known mechanisms for auditing the attendance of children in the community, it is possible that households misreported attendance or other education outcomes. Most obviously, parental reports could be influenced by social desirability bias, thus providing a noisy signal of true absences. More problematically, this social desirability bias may be greater specifically in treatment villages, as parents are aware that attending school 80 percent of the time is a condition of the program, and they may have perceived enumerators to be connected to TASAF or the government. This could bias downward estimates of the effect of the CCT on missing school (i.e., it may appear that the CCT reduces absenteeism by students more than it actually does). Further, a single week cannot capture broader patterns of attendance over a longer period—and may not pick up on a lack of attendance if, for example, school was closed in the past week (in which case our indicator for the child missing school due to a reason unrelated to school closure would take on a 0 simply because there was not an opportunity for it to take on a 1). This motivates us to use administrative records of attendance as a second source.

Therefore, during endline data collection the research team sent enumerators to all schools in treatment and comparison villages that were attended by children in the village during the midline survey. They collected administrative data on recorded child presence in school during May 2012 (i.e., the last full month which pre-dated our endline survey and for which school was generally in session across Tanzania). The school year begins in January and has two terms (January to June and July to December; as a result, May would be in the latter part of the first term (Shupavu, 2020). Appendix Table A2 considers several specific measures: the share of days attended (with the denominator being days school was in session during this month) (column 2), and indicators for attending at least 80 percent of days that the school was in session (a condition of the program) (column 3), and for attending at least 90 percent of days (column 4). These data are only present at endline, and thus we cannot use individual fixed effects (instead we use district fixed effects). Accordingly, we do not use them as our central measures of student absenteeism. However, they provide a more accurate measure of patterns of absenteeism. Given that conditions only applied to children aged 7–15, we consider in this analysis all children who were 7–15 at endline and participating in school. As this analysis only utilizes endline data and also employed a different age group compared to our self-reported absenteeism panel data analysis (in which children were aged 6–12 at baseline, 8–15 at midline, and 9–16 at endline), we also compute a comparable result using our self-reported measure of absenteeism (column 1). Specifically, we code it using parent self-reported absenteeism data from only endline, and for children aged 7–15 at endline.

The administrative data also indicate a broadly null effect of the CCT on absenteeism. We confirm that our null results on the effect of the CCT when using panel data on parental self-reports of absenteeism in the last week are similarly null here, when using only endline data and using children aged 7–15 at endline. But we also find that results are null when using the continuous measure for share of days absent in the last month, and the indicator for attending at least 80 percent of days on which school was held in the last month. When we use as an outcome an indicator for attending at least 90 percent of the days on which school was held in the last month, however, the CCT as a statistically significant, positive impact on attending school. Residing in a treatment village yields an 8 percentage point increase in the likelihood of attending school at least 90 percent of the time. Thus, while we cannot reject that the CCT had no impact on meeting the conditions of the program, it did appear to increase the prevalence of very high attendance rates. This would be consistent with the case of conditions having little effect on school attendance, but other channels—such as increased household income—permitting select students already attending over 80 percent of the time to push their attendance above 90 percent.

Our finding of significant impacts primarily on school participation rather than attendance may be due to a number of possible reasons. First, the CCT was conditional on enrollment at age 7, but 35 percent of children aged 7 had never been to school in the control group at baseline. The program may have incentivized earlier enrollment then would have otherwise occurred. Second, cash transfers may have allowed households to pay fixed costs related to enrollment (e.g., for shoes, school materials, uniforms, and required contributions for children's benches).

5.2 School progression and completion

We next consider the impacts of the CCT program on school progression and completion among children aged 13–18 at baseline, in Table 4. (Children younger than this age group would not necessarily be expected to have completed primary school.) We consider two outcomes: an indicator for the child having completed primary school (column 4), and an indicator for the child attending secondary school (column 5). Separately considering younger and older children is particularly interesting given that the opportunity cost of education influences decisions about attending (De Janvry and Sadoulet, 2015), and may increase with age. Do older children benefit in the same ways as younger children?

The CCT program appears to have affected completion of primary school by children with a baseline age of 13–18 more than their likelihood of attending secondary school. At midline, treatment led to a 14 percentage point increase in the likelihood of completing primary school, which rose to a 16 percentage point increase by endline (both estimates significant at the 0.01 level). Again, it is helpful to take the ages of the children at midline and at endline into account; they would be between ages 15 and 21 at midline, and between ages 16 and 22 at endline. Children in Tanzania would complete primary school at age 14 (if there is no grade repetition), and may or may not move on to secondary school. These completion effects are large, and reflect compliance with conditions to stay in school through age 15 (though it is not clear if the presence of the conditions can explain this result, or if it is due to other channels such as an income effect).

Despite increases in completion of primary school due to the CCT, we find no evidence that the likelihood of attending secondary school increased. While the coefficient on the CCT program is positive at both midline (indicating a 4 percentage point increase in the likelihood of attending secondary school) and endline (indicating a 15 percentage point increase in the likelihood of attending secondary school), these effects are not statistically significant. Thus, we find no evidence that overall, children having completed primary school due to the CCT also enroll in secondary school as a result of the program.

5.3 Heterogeneous effects by common markers of vulnerability

Our overall estimates suggest broader increases in school participation and primary completion. In this section, we examine how the results vary across children who do and do not exhibit common markers of vulnerability. These include child gender, household poverty (i.e., whether or not the household is in the bottom three quintiles on an asset index), orphan status (i.e., whether or not at least one parent is deceased), and whether they had both sat for and passed a Standard IV (i.e., 4th grade) exam at baseline. We describe our tables of results below; the coefficients on treatment, along with the 90 percent confidence intervals around the estimates, are depicted for each of these regressions in Figures 2 and 3.

5.3.1 Gender

We observe important heterogeneity, but sometimes in unexpected ways. In the case of gender, at midline, we see effects of transfers on girls' school participation (a 12 percentage point increase) but not that of boys—though the difference is not statistically significant (Table 5). However, at endline, after 2.75 years of transfers, girls and boys experience similar increases in school participation (a 10 percentage point increase for girls compared to a 9 percentage point increase for boys) that are also statistically indistinguishable. Gendered impacts of transfers on whether individuals have ever attended school are similar. Transfers did not affect the likelihood of missing school last week (among 6–12 year olds) or attending secondary school (among 13–18 year olds) for either girls or boys during either follow up survey. We find similar and always statistically significant impacts of transfers on primary completion for girls and for boys in both rounds. The coefficient on transfers for girls is larger than boys, indicating by endline a 20 percentage point increase in the primary completion rate for girls compared to a 14 percent increase for boys, though this difference is not statistically significant.

At baseline, girls actually enrolled in and participated in school more often then boys, and were reported by their parents as being less likely than boys to miss school. They were also equally likely to complete primary school—though they were less likely than boys to attend secondary school (17 percent of boys aged 13–18 attended secondary school at baseline, vs. 11 percent of girls).

5.3.2 Household poverty and head education

When we test for heterogeneous treatment effects by household poverty level (captured by an indicator for being in the bottom three quintiles of asset wealth) in Table 6, we identify the largest gains in school participation being for the poorest children. At both midline and endline, we find no impacts of the CCT on school participation of less-poor (top two quintiles of asset wealth) households. However, transfers increase school participation of children from the poorest households by 11 percentage points at both midline and endline. Nonetheless, the results are not statistically significantly different. Results for having ever attended school mirror those for school participation at midline, though by endline they suggest that the benefits of transfers are more equally shared across wealth groups. Again, neither group sees changes in whether or not they missed school last week as a result of receiving transfers.

Considering our school participation results, it may be the case that high baseline rates of attendance among children in less-poor households (81 percent participated in school, on average) mean that neither the attendance conditions nor access to income were binding constraints on attendance that would be alleviated by the program. In contrast, less-highly-resources households (for whom only 70 percent participated in school at baseline) might need conditions, transfer income, or both to nudge them into participating in school.

When we consider primary school completion, we find a reversed pattern of results—with children from less-poor households benefiting more than those from the poorest household, if anything (though we note that the effects on each group are not statistically significantly different). At midline, Both the poorest and less-poor households realize increases in primary completion due to transfers—specifically, an 11 percentage point increase in primary completion by the poorest, and a 17 percentage point increase for the less-poor. And by endline, we no longer see statistically significant increases in primary completion by the poorest, and we see a 19 percentage point increase in primary completion for the less-poor. That more well-resources households benefited more from transfers on this outcome may reflect rising costs (and opportunity costs) of school attendance towards the end of primary (compared to other points in the child's schooling) which might make school participation prohibitive for less-poor children if not for the program conditions and transfer income.

When our outcome is attending secondary school—for which the CCT had a statistically insignificant impact for the full sample of children with baseline ages between 13 and 18—we do not identify any statistically significant impacts for either sub-group of wealth, and nor do we identify statistically significant differences in the effects of transfers across the two groups. Overall, we take these findins as evidence that on one measure of vulnerability, household asset poverty, the most vulnerable children benefitted equally from the program—though there is suggestive evidence that this is only on school enrollment and participation, and not on completion.

There are of course many ways to measure household poverty, and an asset wealth index is only one of them. To assess the robustness of these household poverty results, we also consider the effects of the CCT program by whether or not the household head at least some education[9]. These results exhibit some differences compared to those for household asset wealth, as shown in Appendix Table A3. Gains in school participation were concentrated among children with a poorly educated household head. This is similar to our findings that the poorest households benefited most in terms of increased school participation. However, in contrast to the results using low household wealth as a measure of vulnerability, after 2.75 years of transfers, gains in primary school completion were concentrated among households with uneducated heads. Further, we also identify statistically significant increases in secondary school attendance among children from uneducated heads. Specifically, children with uneducated heads became 23 percentage points more likely to complete primary school and 20 percentage points more like to attend secondary school, while there are no statistically significant impacts on their counterparts from households where heads have some education.

5.3.3 Orphan and foster status

To the extent that we view being an orphan as a marker of vulnerability, results considering heterogeneous treatment effects by orphan status (Table 7) contrast markedly with results using household wealth as a marker of vulnerability. At both midline and endline, we find no impacts of the CCT on either school participation or whether children have ever enrolled in school for orphan children, while the effects of the CCT are always statistically significant and positive for non-orphan children (the CCT results in an 11 percentage point increase in school participation at both midline and endline, and a 10 percentage point increase in school enrollment at both midline and endline). For midline (though not for endline), the difference between effects of the CCT on school participation for orphan and non-orphan children is statistically significant at the 0.10 level. Thus, in contrast to our results using household wealth to indicate vulnerability,

[9]We do not examine parents' education specifically, which is on average higher than that of the household head, because this information is captured only for children whose parents are alive and in the household

orphanhood as an indicator of vulnerability suggests that the *less vulnerable* children benefited more in terms of school participation. This is consistent with evidence that orphanhood and poverty are not always well correlated, and may measure quite different aspects of vulnerability (Ainsworth and Filmer, 2006). Indeed, at baseline, school participation was higher for orphan children (77 percent participated) compared to non-orphan children (73 percent participated). Again, we see no impacts of transfers on missing school last week for either orphans or non-orphans.

Turning to primary completion, the coefficient on transfers at midline is identical for orphans and non-orphans (indicating a 14 percentage point increase in completion—only statistically significant for non-orphans, but we cannot statistically reject that the effects are the same). However, at endline and thus after 2.75 years of transfers, we see a 22 percentage point increase in primary completion for orphans compared to a smaller, 13 percentage point increase for non-orphans (though again, the difference is not statistically significant). Thus, we find evidence consistent with broadly shared increases in primary completion at endline that if anything favor orphans (in contrast to our school participation results). We find no increases in secondary school attendance, however, for either group.

It is useful to consider a distinct but related way to split children: by whether or not they are foster children. We do so in Appendix Table A4. Similar to the pattern for orphan status, we find that school participation increases for non-foster children (by 11 percentage points at midline and 13 percentage points at endline), but identify no statistically significant impacts on foster children (though we cannot reject, for either round, that the effects on the two groups are the same). However, transfers had similar effects across both groups and both rounds for the outcome of having ever attended school, where the effect of transfers is in all cases statistically significant, positive, and indistinguishable across groups. Again, there are no impacts on whether or not children in either gorup missed school last week. Turning to primary completion, while only non-foster children benefited at midline (a 16 percentage point increases in primary completion that remains at endline), foster children realize an 18 percentage point gain in primary completion at endline, in addition to an 18 percentage point increase in attendance of secondary school at endline not seen for non-foster children.

5.3.4 Baseline exam performance

When we consider our two outcomes for older (aged 13–18 at baseline) children, we can capture vulnerability using another indicator: whether or not the child sat for and passing a Standard IV (i.e., 4th grade) exam at baseline. In this analysis, shown in Table 8, we again find evidence of *less vulnerable* children at baseline benefiting more from the CCT program. The CCT program causes children who passed this exam at baseline to be 19 percentage points more likely to complete primary school at midline and 21 percentage points more likely to complete primary school at endline. The CCT does not have statistically significant impacts on primary completion for those who had not passed this exam at baseline, and the point estimates are furthermore smaller in magnitude. Further, at endline, we see that the CCT increases attendance of secondary school, but only for children who had passed the Standard IV exam at baseline (they experience a 23 percentage point increase in the likelihood of attending secondary school). The CCT did not have a statistically significant effect on secondary school attendance for those who had not passed the Standard IV exam at baseline, and in fact, the coefficient on receipt of transfers is negative for this group. Overall, we take this as evidence that on some important measures of vulnerability for which the CCT was not specifically designed, the program's benefits were concentrated on the least vulnerable.

5.4 Adjustments for multiple hypothesis testing

Because this program tests five education outcomes, and considers a number of heterogeneous treatment effects which were identified after the program was underway, it is important to correct our findings for potential false-positive results due to testing multiple hypotheses. One popular method is the Benjamini-Hochberg method (BH) method, which controls for the false discovery rate (Benjamini and Hochberg, 1995). In other words, it limits the "expected proportion of rejections are type I errors," or false positives (Anderson 2008).[10] Another popular method for controlling the false discovery rate is the Benjamini-Krieger-Yekutieli (BKY) method, outlined in (Benjamini et al., 2006). We compute the q-values (i.e., p-values corrected for multiple testing) for each method.

Correction for multiple hypothesis testing is sensitive to how hypotheses are grouped, underscoring the importance of clarifying and motivating this grouping. Our paper considers two main research questions, motivating two groupings of hypotheses. These are: 1) What is the impact of conditional cash transfers (CCTs) on schooling outcomes for low-income households in a low-income country? and 2) Do CCTs improve schooling outcomes for children who experience vulnerability? Our first grouping of hypotheses, which addresses this first question, groups together the 10 hypotheses in Table 4 and depicted visually in Figure 1 (five outcomes and two survey rounds). Our second grouping of hypotheses, which addresses this second question, groups together the 68 hypotheses considered in Tables 5 - 8 and depicted visually in Figures 2 and 3. We do not include in our groupings the supplementary regressions presented in the Appendix in order to check the robustness of our results to alternative measures of poverty (i.e., household head education) and parental presence (i.e., fostering status).

Appendix Table A4 shows the results of our analysis; Panel A contains the first grouping of hypotheses while Panel B contains the second. We find broad agreement across the two methods. In Panel A, we find that not only are all of our results preserved when we use either method, but also a result not previously statistically significant at convention levels—the effect of transfers

[10]Controlling the false discovery rate contrasts with controlling the family-wise error rate, or the "probability of rejecting at least one null hypothesis." FWER is most appropriate for cases where the cost of a false rejection of the null hypothesis has strong policy implications Anderson (2008).

on secondary attendance at endline—becomes statistically significant in the BKY results. Thus, all of our main effects are preserved.

Panel B further indicates that our heterogeneous effects are also broadly preserved. There are seven previously significant results (i.e., for which the p-value was under 0.10) that have q-values above 0.10 for both the BH and BKY methods, but 15 significant results that are preserved (i.e., either the BH or BKY q-value is under 0.10). In total, across the two panels, out of 28 total previously significant results, 21 survive. We take this as evidence that at least the overall pattern of our results is broadly unchanged.

6 Discussion

Our results contribute to a large literature on cash transfers and education. Between them, two recent reviews – Baird et al. (2014) and Bastagli et al. (2016) – cite more than one hundred separate studies of the impact of cash transfers on education. Our average impacts are consistent with the literature, with positive impacts on school participation. We add to that our sizeable, significant impacts on primary completion rates, of which neither earlier review makes explicit mention. Both reviews highlight that few studies measure impacts on test scores (and that results are mixed for those that do), and while we do not provide test score results, primary school completion is one indicator that children are not merely participation in school but are progressing therein.

In the 101 studies examined between the two reviews, outcomes for certain sub-groups are much more commonly reported than others (Table 9). Most studies report results separately by gender. Bastagli et al. (2016) do not synthesize differential effects for girls versus boys, but they do summarize impacts for girls and find them to be consistent with impacts for boys: positive impacts on school participation, with both less evidence and less promising evidence on test scores. Baird et al. (2014) report comparable but slightly larger effects on enrollment for girls, although the differences are unlikely to be statistically significant. These are consistent with our results with slightly larger point estimates for girls, although those differences are not statistically significantly different than the impacts for boys (Table 5).

Only one quarter of the 101 earlier studies separate impacts for the poorest and the less poor. Cash transfers are almost always targeted to households that are poor in general, so these distinctions are within that context. We find mixed results on poverty: the poorest in our sample have higher point estimates on school participation but lower point estimates on primary completion—none of these differences are statistically significant. Barrera-Osorio et al. (2011) find stronger enrollment effects for the poorest students in Colombia. In Brazil, Cardoso and Souza (2009) report the strongest impacts on school enrollment for children with uneducated parents. The same is true for attendance and enrollment among secondary school girls in Cambodia (Filmer and Schady, 2008). A study in Kenya does find positive impacts on school completion, concentrated among the poorest (Merttens et al., 2013). In China, alternatively, the impact of a cash transfer program on dropout rates is smaller among the poorest (Mo et al., 2013). The fact that we find suggestive evidence of an opposite effect when we examine a less studied outcome that may matter more for longer term outcomes—school completion—points to a need for more study.

We look at outcomes for orphans versus non-orphans and find apparently larger school participation impacts on orphans, but not so with primary completion. These are consistent with the already discussed counterintuitive finding that in our sample, orphans tend to reside in less poor households. No previous studies that we identified separate children by orphan status, despite the fact that orphanhood can have significant educational impacts (Case et al., 2004; Evans and Miguel, 2007).

7 Conclusion

In this paper, we identify the impact of a community-based conditional cash transfer program – targeted to poor children in Tanzania – on those children's educational engagement. We find positive average effects both on primary school engagement and on primary school completion, broadly in line with the existing literature. We go on to show that some other indicators of vulnerability – like gender – do not show differential impacts, whereas others – like orphan status and school performance – do show differential impacts, with the more vulnerable children benefiting less from the transfer program.

We observe children for 2.75 years after transfers, which is longer than the vast majority of impact evaluations of education interventions. McEwan (2015) shows that most education evaluations examine impacts after one year or less. However, the ultimate objective is not only to keep children in school in the short run, but to help them achieve more total schooling and better post-school life outcomes. Longer run data on the impact of cash transfers on children with a variety of indicators of vulnerability would be valuable. Furthermore, this work does not evaluate the impact of cash transfers for non-beneficiaries within the same village, and existing work suggests some evidence both for positive economic impacts (Egger et al., 2019; Handa et al., 2019) and adverse psychological impacts (Haushofer et al., 2019). Future work can further document spillovers to a variety of vulnerable groups within villages. Additionally, while our pattern of relative significance remains after we adjust for multiple hypothesis testing, some statistical significance disappears, potentially due to limited samples of different heterogeneous groups.

Ultimately, because cash transfers provide cash, they may be most effective at alleviating constraints related to poverty, which may be why we see positive average effects (among poor children) and some pattern of stronger positive effects for the poorest of those poor children. However, cash transfers are not inherently built to address other vulnerabilities, which means that either they may need to be adapted to benefit children with vulnerabilities beyond poverty or that other policy programs may need to be brought to bear to ensure that those children as well have access to investments in quality human capital.

References

Ainsworth, M., K. Beegle, and G. Koda (2021). The impact of adult mortality and parental deaths on primary schooling in north-western tanzania. *Journal of Development Studies*.

Ainsworth, M. and D. Filmer (2006). Inequalities in childrens schooling: AIDS, orphanhood, poverty, and gender. *World Development 34*, 1099–1128.

Akresh, R., E. Bagby, D. de Walque, and H. Kazianga (2012). Child ability and household human capital investment decisions in burkina faso. *Economic Development and Cultural Change*.

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American Statistical Association 103*.

Araujo, M. C., M. Bosch, and N. Schady (2019). Can cash transfers help households escape an intergenerational poverty trap? In C. B. Barrett, J.-P. Chavas, and M. R. Carter (Eds.), *The Economics of Poverty Traps*. Chicago, IL: University of Chicago Press.

Baird, S., F. H. Ferreira, B. Özler, and M. Woolcock (2014). Conditional, unconditional and everything in between: a systematic review of the effects of cash transfer programmes on schooling outcomes. *Journal of Development Effectiveness 6*(1), 1–43.

Baird, S., C. McIntosh, and B. Özler (2013, October). The regressive demands of demand-driven development. *Journal of Public Economics 106*, 27–41.

Banerjee, A., R. Banerji, J. Berry, E. Duflo, H. Kannan, S. Mukerji, M. Shotland, and M. Walton (2017). From proof of concept to scalable policies: Challenges and solutions, with an application. *Journal of Economic Perspectives 31*(4), 73–102.

Barrera-Osorio, F., M. Bertrand, L. Linden, and F. Perez-Calle (2011). Improving the design of conditional transfer programs: Evidence from a randomized education experiment in colombia. *American Economic Journal: Applied Economics 3*, 167195.

Barrera-Osorio, F., L. L. Linden, and J. E. Saavedra (2019). Medium- and long-term educational consequences of alternative conditional cash transfer designs: Experimental evidence from Colombia. *American Economic Journal: Applied Economics 11*(3), 54–91.

Bastagli, F., J. Hagen-Zanker, L. Harman, V. Barca, G. Sturge, T. Schmidt, and L. Pellerano (2016). Cash transfers: what does the evidence say? a rigorous review of programme impact and of the role of design and implementation features.

Battaile, W. G. (2020). What does Tanzania's move to lower-middle income status mean? *Africa Can End Poverty blog*.

Belghith, y. N. B. H., W. Karamba, E. Talbert, and P. de Boisseson (2019). Tanzania: Mainland poverty assessment.

Benjamini, Y. and Y. Hochberg (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal statistical society: series B (Methodological) 57*(1), 289–300.

Benjamini, Y., A. M. Krieger, and D. Yekutieli (2006). Adaptive linear step-up procedures that control the false discovery rate. *Biometrika 93*(3), 491–507.

Cardoso, E. and A. P. F. de Souza (2009). The impact of cash transfers on child labor and school enrollment in brazil. In P. Orazem, G. Sedlacek, and Z. Tzannatos (Eds.), *Child Labor and Education in Latin America*. New York: Palgrave Macmillan.

Case, A., C. Paxson, and J. Ableidinger (2004). Orphans in africa: parental death, poverty, and school enrollments. *Demography 41*, 483508.

Cho, H., R. C. Ryberg, K. Hwang, L. D. Pearce, and B. J. Iritani (2017). A school support intervention and educational outcomes among orphaned adolescents: Results of a cluster randomized controlled trial in Kenya. *Prevention Science 18*, 943954.

De Janvry, A. and E. Sadoulet (2015). *Development economics: Theory and practice*. Routledge.

Duflo, A., J. Kiessel, and A. Lucas (2020). External validity: Four models of improving student achievement. *National Bureau of Economic Research (NBER)*.

Egger, D., J. Haushofer, E. Miguel, P. Niehaus, and M. W. Walker (2019). General equilibrium effects of cash transfers: Experimental evidence from Kenya. *National Bureau of Economic Research (NBER)*.

Evans, D. K., M. Akmal, and P. Jakiela (2021). Gender gaps in education: The long view. *IZA Journal of Development and Migration*.

Evans, D. K., B. Holtemeyer, and K. Kosec (2019). Cash transfers and health: Evidence from Tanzania. *World Bank Economic Review 33*(2), 394–412.

Evans, D. K. and E. Miguel (2007). Orphans and schooling in africa: a longitudinal analysis. *Demography 44*, 35–57.

Fazzio, I., A. Eble, R. L. Lumsdaine, P. Boone, B. Bouy, P.-T. J. Hsieh, C. Jayanty, S. Johnson, and A. F. Silva (2020). Large learning gains in pockets of extreme poverty: Experimental evidence from Guinea Bissau. *NBER Working Paper 27799*.

Filmer, D. and N. Schady (2008). Getting girls into school: Evidence from a scholarship program in cambodia. *Economic Development and Cultural Change 56*(3), 581–617.

Hagen-Zanker, J., F. Bastagli, L. Harman, V. Barca, G. Sturge, and T. Schmidt (2016). Understanding the impact of cash transfers: the evidence. Technical report, Overseas Development Institute.

Hallfors, D., H. Cho, S. Rusakaniko, B. Iritani, J. Mapfumo, and C. Halpern (2011). Supporting adolescent orphan girls to stay in school as hiv risk prevention: Evidence from a randomized controlled trial in Zimbabwe. *American Journal of Public Health 101*, 1082–1088.

Handa, S., L. Natali, D. Seidenfeld, G. Tembo, and B. Davis (2019). Can unconditional cash transfers raise long-term living standards? evidence from zambia. *Journal of Development Economics 133*, 42–65.

Haushofer, J., J. Reisinger, and J. Shapiro (2019). Is your gain my pain? effects of relative income and inequality on psychological well-being. *Working Paper*.

Hickey, S., T. Lavers, M. Nio-Zaraza, and J. Seekings (Eds.) (2019, November). *The Politics of Social Protection in Eastern and Southern Africa*. WIDER Studies in Development Economics. Oxford, New York: Oxford University Press.

McEwan, P. (2015). Improving learning in primary schools of developing countries: A meta-analysis of randomized experiments. *Review of Educational Research 85*.

Merttens, F., A. Hurrell, M. Marzi, R. Attah, M. Farhat, A. Kardan, and I. MacAuslan (2013). Kenya hunger safety net programme monitoring and evaluation component impact evaluation final report: 2009 to 2012.

Mo, D., L. Zhang, H. Yi, R. Luo, S. Rozelle, and C. Brinton (2013). School dropouts and conditional cash transfers: Evidence from a randomised controlled trial in rural chinas junior high schools. *Journal of Development Studies 49*(2), 190–207.

Molina Millán, T., T. Barham, K. Macours, and J. A. Maluccio (2019). Long-term impacts of conditional cash transfers: Review of the evidence. *World Bank Research Observer 34*(1), 119–159.

Molina Millán, T., K. Macours, J. A. Maluccio, and L. Tejerina (2020). Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. *Journal of Development Economics 143*.

Moodley, J. and L. Graham (2015). The importance of intersectionality in disability and gender studies. *Agenda: Empowering Women for Gender Equity*.

Sabates, R., P. Rose, B. Alcott, and M. Delprato (2020). Assessing cost-effectiveness with equity of a programme targeting marginalised girls in secondary schools in Tanzania. *Journal of Development Effectiveness*.

Shupavu, B. (2020). Tanzania's school system: An overview. *AfricaAid*.

Thomas, T., Y. Ahmed, M. Tan, and E. L. Grigorenko (2020). Cognitive and educational interventions for orphans and vulnerable children affected by HIV/AIDS: A metaanalysis. *Child Development*.

World Bank (2018). *World Development Report 2018: Learning to Realize Educations Promise*. World Bank.

World Bank (2019a). *Ending Learning Poverty: What Will It Take?* World Bank.

World Bank (2019b). World development indicators.

Table 1: Timeline for implementation of CCT and accompanying impact evaluation

Timing	Activity
November 2007 - September 2008	Program Design (completion of Operational Manual, set up of MIS, preparation of guidelines, forms, and materials for training activities)
September - November 2008	Sensitization at regional, district, ward, and community levels
October - November 2008	Targeting activities (field data collection, data entry, and community validation of beneficiaries)
October - November 2008	Training of district officers and community management committees on the targeting process
January - May 2009	Baseline survey
September - October 2009	Enrollment of beneficiaries
January 2010	First payments made to beneficiary households
November 2010 - February 2011	Community Scorecard Exercise
July - September 2011	Midline survey & first round of focus group interviews
August - October 2012	Endline survey
July - August 2013	Second round of qualitative data collection, including in-depth and focus group interviews

Table 2: Balance Table

	Treatment (T)		Control (C)		Difference (T-C)	
	Mean	N	Mean	N	Mean	S.E.
Individuals aged 6-12						
Age in yrs.	9.01	661	8.94	594	0.06	0.11
Dummy - female	0.49	661	0.50	594	-0.02	0.03
Dummy - either parent is deceased (orphan)	0.26	661	0.26	594	-0.00	0.03
Dummy - neither parent in household (foster)	0.47	641	0.50	581	-0.03	0.04
Dummy - current school participation	0.72	661	0.77	594	-0.05	0.04
Dummy - ever attended school	0.74	661	0.78	594	-0.05	0.03
Dummy - missed school in last week	0.11	474	0.12	458	-0.00	0.03
F-test joint sig.	0.51					
Individuals aged 13-18						
Age in yrs.	15.14	447	15.15	447	-0.01	0.10
Dummy - female	0.45	447	0.45	447	0.01	0.04
Dummy - either parent is deceased (orphan)	0.31	447	0.38	447	-0.07*	0.04
Dummy - neither parent in household (foster)	0.43	395	0.47	373	-0.04	0.05
Dummy - sat and passed Std. IV exam at baseline	0.71	361	0.82	335	-0.11***	0.03
Dummy - completed primary	0.32	416	0.37	404	-0.05	0.04
Dummy - attended secondary	0.10	447	0.19	447	-0.09**	0.04
F-test joint sig.	3.11					
Households						
Dummy - head has no education	0.61	879	0.66	876	-0.05	0.03
Dummy - household in lowest 3 quintiles of asset index	0.70	879	0.66	878	0.04	0.03
Dummy - head says school quality is good or excellent	0.84	878	0.86	878	-0.02	0.02
F-test joint sig.	2.65					
Villages						
Number of HH 2009	868.02	39	1091.42	39	-223.40	344.08
Dummy - village houses ward	0.27	40	0.43	40	-0.16	0.11
Dummy- VEO lives in this village	0.83	40	0.70	40	0.13	0.09
Num. general village meetings held	3.15	40	3.35	40	-0.21	0.24
Poverty rank (TASAF)	1.19	40	1.13	40	0.06	0.10
PMT Poverty score (TASAF)	7575.99	40	7398.65	40	177.34	236.07
F-test joint sig.	1.21					

Note: Table presents results from an OLS regression of each treatment separately on covariates, including district dummies and standard errors clustered at the village. The column N displays the number of observations, and the column Mean displays the coefficient for each covariate. Whether child missed school in last schooling week includes those who missed for a reason other than public holiday, school closure or teacher absence. Variables labeled are taken from proxy means test conducted prior to randomization; the PMT poverty score is estimated from household consumption and assets including roofing, source of power and the presence of appliances, and the poverty rank is a score (0 to 5) assigned to households calculated based on the number of would-be benficiaries, status of the household head and household amenities.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Table 3: Attrition Table

	Household is in ... survey				Individual 6-12 is in ... survey				Individual 13-18 is in ... survey			
	Midline		Endline		Midline		Endline		Midline		Endline	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	-0.011	0.092	-0.024	-0.318	-0.018	0.130	-0.027	0.641	0.114***	-2.302	0.061	-0.231
	(0.015)	(0.188)	(0.019)	(0.259)	(0.025)	(0.618)	(0.032)	(0.672)	(0.035)	(2.948)	(0.042)	(3.313)
Head no ed.		-0.008		-0.029		-0.006		0.021		0.101*		0.002
		(0.020)		(0.028)		(0.040)		(0.046)		(0.055)		(0.066)
Treat * Head no ed.		0.014		0.016		0.036		-0.023		0.006		0.136
		(0.029)		(0.037)		(0.050)		(0.060)		(0.083)		(0.087)
Low wealth		0.054**		0.081***		0.051		0.079		0.138**		0.110*
		(0.022)		(0.024)		(0.051)		(0.064)		(0.053)		(0.061)
Treat * Low wealth		0.001		-0.014		-0.084		-0.063		-0.117		-0.047
		(0.028)		(0.033)		(0.054)		(0.071)		(0.075)		(0.083)
Head is male		-0.026		-0.028		-0.019		-0.017		-0.005		-0.041
		(0.020)		(0.022)		(0.046)		(0.041)		(0.052)		(0.056)
Treat * Head is male		-0.012		-0.018		-0.030		-0.133**		-0.038		-0.042
		(0.028)		(0.033)		(0.059)		(0.057)		(0.070)		(0.082)
Head age		-0.005		-0.015**		-0.000		-0.004		-0.008		-0.020**
		(0.004)		(0.006)		(0.007)		(0.011)		(0.008)		(0.008)
Treat * Head age		-0.003		0.009		-0.013		-0.016		0.014		0.013
		(0.006)		(0.008)		(0.010)		(0.014)		(0.012)		(0.013)
Head age sq.		0.000		0.000***		0.000		0.000		0.000		0.000***
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Treat * Head age sq.		0.000		-0.000		0.000		0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Female						-0.024		0.001		0.089**		0.114**
						(0.031)		(0.033)		(0.042)		(0.043)
Treat * Female						0.012		0.007		0.052		0.066
						(0.043)		(0.045)		(0.060)		(0.061)
Ind. age						-0.186**		-0.223***		0.103		0.192
						(0.091)		(0.075)		(0.279)		(0.297)
Treat * Ind. age						0.043		-0.063		0.240		-0.028
						(0.116)		(0.115)		(0.383)		(0.410)
Ind. age sq.						0.010*		0.012***		-0.003		-0.006
						(0.005)		(0.004)		(0.009)		(0.010)
Treat * Ind. age sq.						-0.001		0.005		-0.007		0.002
						(0.006)		(0.006)		(0.012)		(0.013)
Constant	0.083***	0.154	0.138***	0.549***	0.163***	0.979**	0.250***	1.262***	0.292***	-0.486	0.449***	-0.744
	(0.012)	(0.118)	(0.015)	(0.202)	(0.016)	(0.476)	(0.021)	(0.458)	(0.026)	(2.223)	(0.034)	(2.405)
Observations	1757	1755	1757	1755	1255	1254	1255	1254	894	893	894	893
R-squared	0.002	0.024	0.004	0.028	0.001	0.025	0.007	0.064	0.019	0.072	0.004	0.069
F-stat. joint signif. of interactions		0.321		0.703		1.680		1.739*		2.303**		1.381

Note: Table presents results from an OLS regression of each treatment separately on covariates, including district dummies and standard errors clustered at the village. Household wealth is low if it is in the bottom 3 quintiles of asset index.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Table 4: Conditional Cash Transfers Impacts on Schooling Outcomes

	School Participation	Ever attended	Missed last week	Completed Primary (age 13-18)	Attended Secondary (age 13-18)
Treatment X 2011 (midline)	0.08***	0.08***	0.03	0.14***	0.04
	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)
Treatment X 2012 (endline)	0.10**	0.10***	0.02	0.16***	0.15
	(0.04)	(0.03)	(0.04)	(0.06)	(0.10)
2011 (midline)	0.05**	0.09***	-0.03*	0.33***	-0.06**
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
2012 (endline)	-0.01	0.10***	-0.05**	0.43***	0.09
	(0.02)	(0.02)	(0.03)	(0.04)	(0.06)
Baseline mean	0.74	0.76	0.12	0.34	0.14
Observations	3272	3273	2605	1763	1658

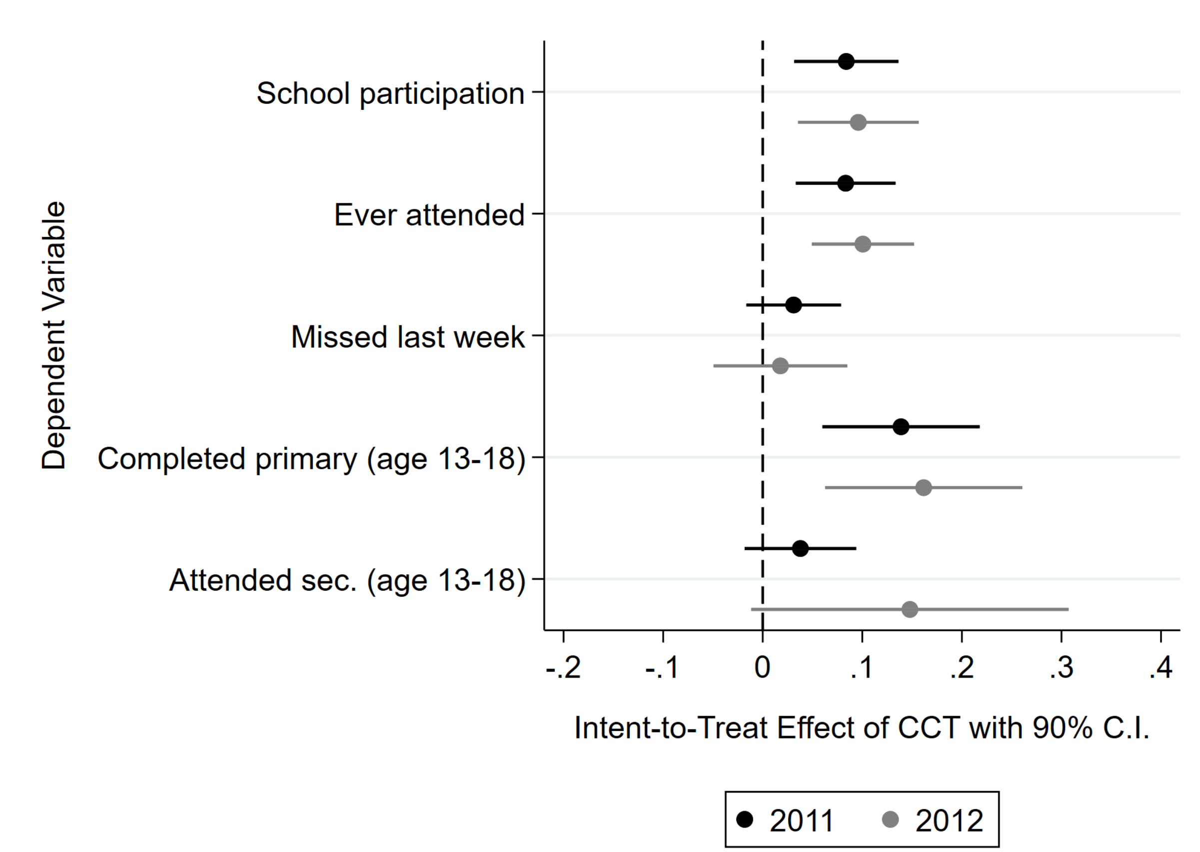
Note: Table presents intent-to-treat effects of each village treatment at midline and endline. Each column is a separate regression. The analytic sample is children who were 6 to 12 (or 13 to 18, as indicated) at baseline. Dependent variables are a dummy to indicate whether child is currently participating in school, whether child has ever attended, whether child missed school in last schooling week for a reason other than public holiday, school closure or teacher absence, whether child completed primary, and whether child attended secondary. Standard errors are clustered by village.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Figure 1: Conditional Cash Transfer Impacts on Schooling Outcomes



NOTE: Figure shows main effects of CCT on schooling outcomes. Figure conveys same information as Table 3.

Table 5: Heterogeneous Effects of CCT by Gender

	School Participation	Ever attended	Missed last week	Completed Primary (age 13-18)	Attended Secondary (age 13-18)
T X 2011 X Female	0.12***	0.10***	0.03	0.18**	-0.02
	(0.04)	(0.04)	(0.04)	(0.07)	(0.05)
T X 2011 X Male	0.04	0.06	0.03	0.11*	0.08
	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)
T X 2012 X Female	0.10**	0.10**	0.04	0.20**	0.10
	(0.05)	(0.04)	(0.05)	(0.08)	(0.13)
T X 2012 X Male	0.09*	0.10**	0.00	0.14*	0.18
	(0.05)	(0.04)	(0.05)	(0.07)	(0.11)
Observations	3272	3273	2605	1763	1658
Baseline mean female	0.76	0.77	0.11	0.34	0.11
Baseline mean male	0.73	0.75	0.13	0.34	0.17
2011 p-value of difference	0.17	0.40	1.00	0.44	0.20
2012 p-value of difference	0.87	0.85	0.51	0.53	0.61

Note: Table presents intent-to-treat effects of each village treatment at midline and endline. Estimation for midline and endline coefficients are not shown. Each column is a separate regression. The analytic sample is children who were 6 to 12 (or 13 to 18, as indicated) at baseline. Dependent variables are a dummy to indicate whether child is currently participating in school, whether child has ever attended, whether child missed school in last schooling week for a reason other than public holiday, school closure or teacher absence, whether child completed primary, and whether child attended secondary. Standard errors are clustered by village.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Table 6: Heterogeneous Effects of CCT by Household Wealth

	School Participation	Ever attended	Missed last week	Completed Primary (age 13-18)	Attended Secondary (age 13-18)
T X 2011 X Poorest	0.11**	0.09**	-0.00	0.11*	0.03
	(0.05)	(0.04)	(0.05)	(0.07)	(0.04)
T X 2011 X Less poor	0.04	0.06	0.07	0.17**	0.04
	(0.04)	(0.04)	(0.04)	(0.07)	(0.05)
T X 2012 X Poorest	0.11**	0.09**	-0.00	0.10	0.14
	(0.05)	(0.04)	(0.06)	(0.07)	(0.14)
T X 2012 X Less poor	0.06	0.10**	0.02	0.19**	0.11
	(0.05)	(0.04)	(0.04)	(0.08)	(0.12)
Observations	3272	3273	2605	1763	1658
Baseline mean poorest	0.70	0.71	0.11	0.30	0.11
Baseline mean less poor	0.81	0.83	0.12	0.39	0.18
2011 p-value of difference	0.32	0.53	0.31	0.52	0.89
2012 p-value of difference	0.46	0.84	0.67	0.35	0.89

Note: Table presents intent-to-treat effects of each village treatment at midline and endline. Estimation for midline and endline coefficients are not shown. Each column is a separate regression. The analytic sample is children who were 6 to 12 (or 13 to 18, as indicated) at baseline. A household is considered poor if it is in the bottom 3 quintiles of an index of 12 common household asset, constructed using principal components analysis. Dependent variables are a dummy to indicate whether child is currently participating in school, whether child has ever attended, whether child missed school in last schooling week for a reason other than public holiday, school closure or teacher absence, whether child completed primary, and whether child attended secondary. Standard errors are clustered by village.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Table 7: Heterogeneous Effects of CCT by Orphan Status

	School Participation	Ever attended	Missed last week	Completed Primary (age 13-18)	Attended Secondary (age 13-18)
T X 2011 X Orphan	0.01	0.04	0.03	0.14	0.01
	(0.06)	(0.05)	(0.06)	(0.09)	(0.07)
T X 2011 X Non-orphan	0.11***	0.10***	0.03	0.14**	0.04
	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)
T X 2012 X Orphan	0.05	0.09	0.02	0.22**	0.10
	(0.07)	(0.06)	(0.06)	(0.09)	(0.14)
T X 2012 X Non-orphan	0.11***	0.10***	0.02	0.13**	0.16
	(0.04)	(0.03)	(0.04)	(0.06)	(0.11)
Observations	3272	3273	2605	1763	1658
Baseline mean orphan	0.77	0.79	0.10	0.38	0.16
Baseline mean non-orphan	0.73	0.75	0.12	0.32	0.14
2011 p-value of difference	0.09	0.20	0.95	0.97	0.71
2012 p-value of difference	0.47	0.79	0.94	0.37	0.74

Note: Table presents intent-to-treat effects of each village treatment at midline and endline. Estimation for midline and endline coefficients are not shown. Each column is a separate regression. The analytic sample is children who were 6 to 12 (or 13 to 18, as indicated) at baseline. A child is an orphan if either parent is deceased. Dependent variables are a dummy to indicate whether child is currently participating in school, whether child has ever attended, whether child missed school in last schooling week for a reason other than public holiday, school closure or teacher absence, whether child completed primary, and whether child attended secondary. Standard errors are clustered by village.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Table 8: Heterogeneous Effects of CCT by Baseline Exam Performance

	Completed Primary (age 13-18)	Attended Secondary (age 13-18)
T X 2011 X Passed	0.19***	0.06
	(0.07)	(0.05)
T X 2011 X Not passed	0.13	-0.08
	(0.12)	(0.06)
T X 2012 X Passed	0.21***	0.23*
	(0.07)	(0.12)
T X 2012 X Not passed	0.07	-0.08
	(0.12)	(0.24)
Observations	1462	1284
Baseline mean passed	0.26	0.22
Baseline mean not passed	0.77	0.04
2011 p-value of difference	0.70	0.08
2012 p-value of difference	0.35	0.17

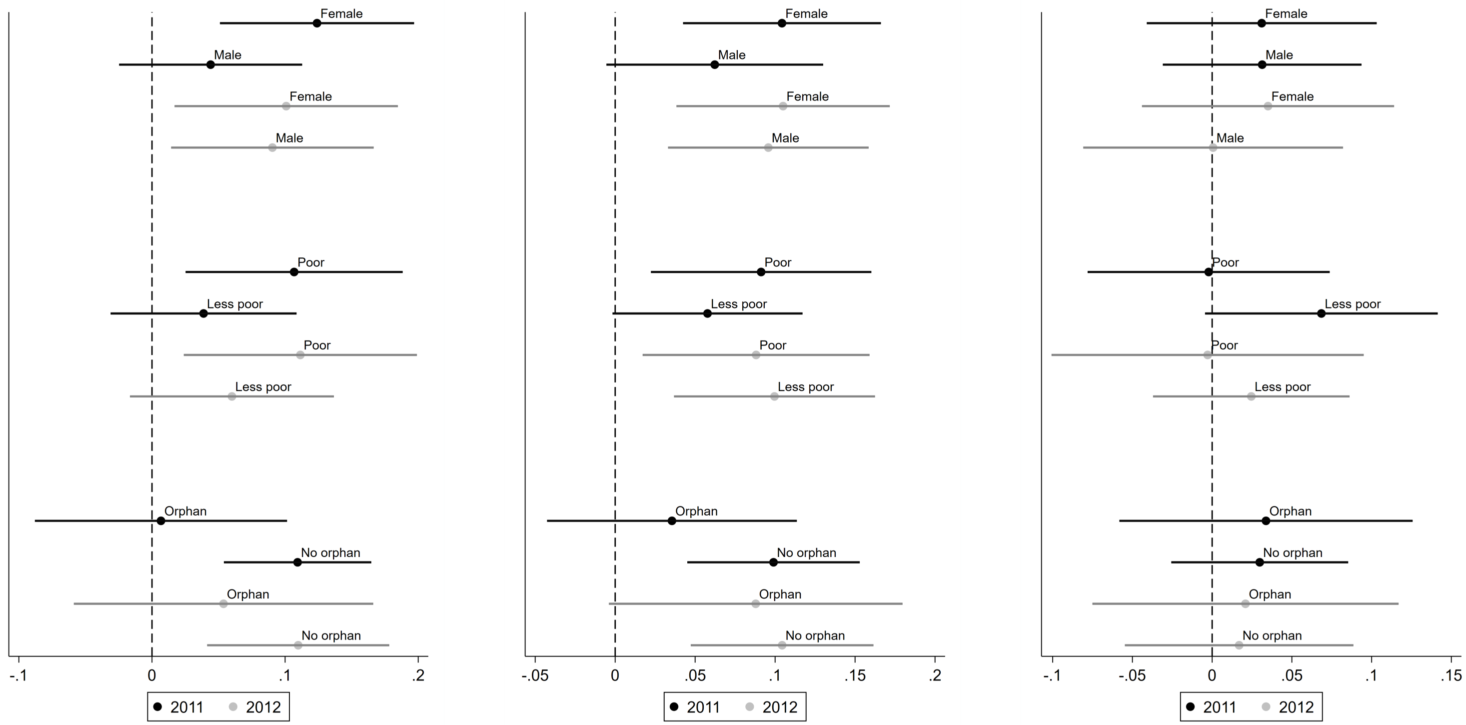
Note: Table presents intent-to-treat effects of each village treatment at midline and endline. Estimation for midline and endline coefficients are not shown. Each column is a separate regression. The analytic sample is children who were 6 to 12 (or 13 to 18, as indicated) at baseline. A child passed if they had sat and passed the Standard IV exam at baseline. Dependent variables are a dummy to indicate whether child completed primary, and whether child attended secondary. Standard errors are clustered by village.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

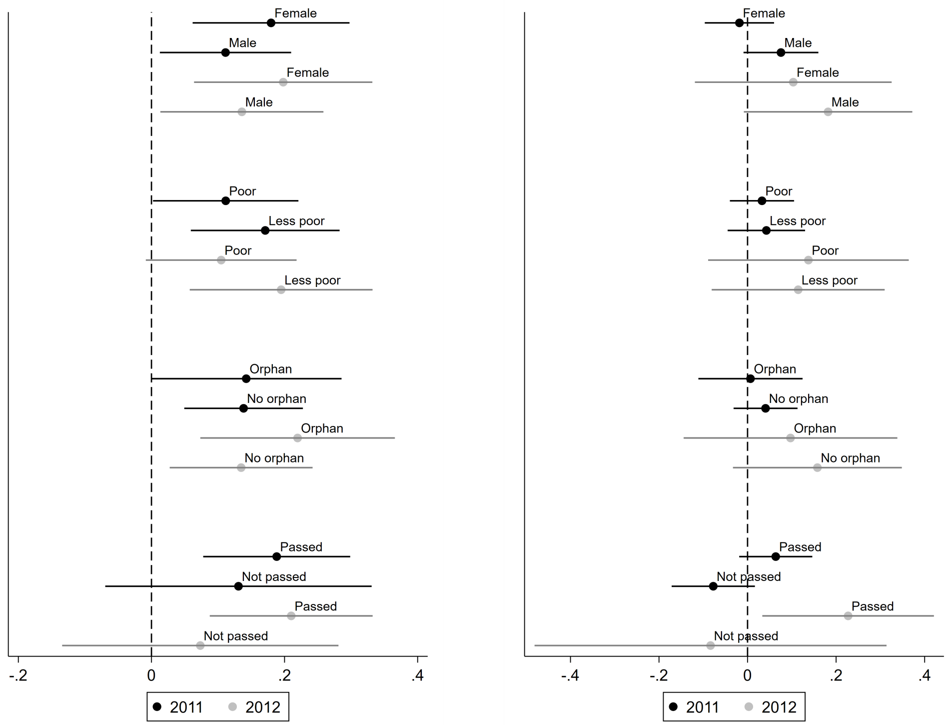
Figure 2: Heterogeneous Effects on Participation



(a) Participation (b) Ever attended (c) Missed last week

NOTE: Figures show heterogeneous effects of CCT on schooling outcomes. Figures convey same information as Tables 4-6.

Figure 3: Heterogeneous Effects on Progression and Completion



(a) Primary complete (age 13-18) (b) Attended secondary (age 13-18)

NOTE: Figures show heterogeneous effects of CCT on schooling outcomes. Figures convey same information as Tables 4-6.

Table 9: Characteristics of 101 Earlier Studies of Cash Transfers on Educational Outcomes

Proportion of studies that report results separately by...	
Gender	65%
Relative poverty	25%
Orphanhood	0%
Foster status	0%
Baseline school performance	5%
Baseline school access	13%
Child age	50%
Urban / rural	15%
Other vulnerabilities	13%

Note: The 101 studies include all studies in English referenced in Baird et al. 2014b and Bastagli et al. 2016. (Three studies were omitted because they were in Spanish or Portuguese.) Other vulnerabilities include households with a single parent, households in indigenous areas, parent education or employment, race, etc.

A Appendix

Table A1: Correlation Matrix for Dimensions of Vulnerability

	Panel A: Ages 6 to 12					
	Female	Poorest	Orphan	Not passed	No ed.	Foster
Female	1					
Poorest	0.0385	1				
Orphan	-0.013	0.0672	1			
Not passed	0.0645	-0.0303	0.0017	1		
No ed.	0.0444	0.1696	0.0307	0.3018	1	
Foster	0.0356	0.0361	0.1847	0.1629	0.2004	1
	Panel B: Ages 13 to 18					
	Female	Poorest	Orphan	Not passed	No ed.	Foster
Female	1					
Poorest	-0.0348	1				
Orphan	-0.026	0.0132	1			
Not passed	0.0033	0.0123	-0.0308	1		
No ed.	0.0919	0.213	0.0402	0.0646	1	
Foster	-0.0326	0.0583	0.3231	-0.0702	0.1985	1

Note: Table presents pairwise correlations for dimensions of vulnerability at baseline. Poorest indicates child is from a household in the bottom 3 quintiles of an index of 12 common household assets, constructed using principal components analysis. Orphan indicates either child's parent is deceased. Not passed indicates child did not pass the Standard IV exam. No education indicates a child lives in a household where the head has no education. Foster indicates neither parent resides in the household.

Table A2: Robustness Check on HH Measured School Attendance

	HH: Missed school last week (1)	Share of days attended (2)	School: 80% (3)	School: 90% (4)
Village treated	0.00	0.05	0.04	0.08*
	(0.03)	(0.04)	(0.04)	(0.04)
Constant	0.06	0.54	0.49	0.35*
	(0.02)	(0.03)	(0.03)	(0.03)
Mean	0.06	0.57	0.51	0.39
Observations	1233	1488	1488	1488
R-squared	0.04	0.02	0.01	0.02

Note: Table presents OLS coefficient of school attendance on CCT treatment, for 7 to 15 year olds at endline. Attendance is measured both through the household (HH) and school registers. School-measured attendance is calculated as the proportion of open school days a child was recorded as attending in May 2012, during endline data collection. Children not attending are coded as zero. Each regression controls for district fixed effects. Standard errors clustered by village.

Table A3: Heterogeneous Effects of CCT by Household Head Education

	School Participation	Ever attended	Missed last week	Completed Primary (age 13-18)	Attended Secondary (age 13-18)
T X 2011 X No Ed.	0.09**	0.07	0.06	0.08	0.05
	(0.05)	(0.05)	(0.04)	(0.07)	(0.04)
T X 2011 X Some Ed.	0.05	0.08**	0.01	0.17**	0.02
	(0.04)	(0.04)	(0.04)	(0.07)	(0.06)
T X 2012 X No Ed.	0.10*	0.10**	0.09**	0.23***	0.20*
	(0.05)	(0.04)	(0.04)	(0.08)	(0.11)
T X 2012 X Some Ed.	0.08	0.08**	-0.06	0.11	0.12
	(0.05)	(0.04)	(0.06)	(0.08)	(0.13)
Observations	3269	3270	2602	1761	1656
Baseline mean no ed.	0.69	0.71	0.10	0.36	0.11
Baseline mean some ed.	0.80	0.82	0.13	0.33	0.18
2011 p-value of difference	0.51	0.89	0.35	0.31	0.65
2012 p-value of difference	0.78	0.66	0.04	0.26	0.58

Note: Table presents intent-to-treat effects of each village treatment at midline and endline. Estimation for midline and endline coefficients are not shown. Each column is a separate regression. The analytic sample is children who were 6 to 12 (or 13 to 18, as indicated) at baseline. A household is considered low education if the head reports having no education. Dependent variables are a dummy to indicate whether child is currently participating in school, whether child has ever attended, whether child missed school in last schooling week for a reason other than public holiday, school closure or teacher absence, whether child completed primary, and whether child attended secondary. Standard errors are clustered by village.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Table A4: Heterogeneous Effects of CCT by Foster Status

	School Participation	Ever attended	Missed last week	Completed Primary (age 13-18)	Attended Secondary (age 13-18)
T X 2011 X Foster	0.06	0.07*	0.01	0.08	0.01
	(0.04)	(0.04)	(0.05)	(0.09)	(0.05)
T X 2011 X Non-foster	0.11**	0.10**	0.04	0.16**	0.03
	(0.05)	(0.04)	(0.05)	(0.07)	(0.04)
T X 2012 X Foster	0.06	0.10***	0.00	0.18*	0.18*
	(0.05)	(0.04)	(0.05)	(0.09)	(0.10)
T X 2012 X Non-foster	0.13**	0.10**	0.02	0.16**	0.14
	(0.05)	(0.04)	(0.06)	(0.08)	(0.11)
Observations	3185	3186	2531	1539	1442
Baseline mean foster	0.75	0.77	0.09	0.28	0.14
Baseline mean non-foster	0.73	0.75	0.14	0.30	0.15
2011 p-value of difference	0.38	0.65	0.61	0.47	0.79
2012 p-value of difference	0.31	0.91	0.86	0.88	0.77

Note: Table presents intent-to-treat effects of each village treatment at midline and endline. Estimation for midline and endline coefficients are not shown. Each column is a separate regression. The analytic sample is children who were 6 to 12 (or 13 to 18, as indicated) at baseline. A child is fostered if neither parent is in the household. Dependent variables are a dummy to indicate whether child is currently participating in school, whether child has ever attended, whether child missed school in last schooling week for a reason other than public holiday, school closure or teacher absence, whether child completed primary, and whether child attended secondary. Standard errors are clustered by village.

*** significant at the 1% level

** significant at the 5% level

* significant at the 10% level.

Table A5: Multiple Hypothesis Testing Results

Estimate	Ages	Outcome	Table	Column	P-value	BH	BKY
Panel A: Main effects							
T X 2011	6 to 12	Participation	4	1	.009	.017	.017
T X 2012	6 to 12	Participation	4	1	.01	.017	.017
T X 2011	6 to 12	Ever attended	4	2	.007	.017	.017
T X 2012	6 to 12	Ever attended	4	2	.002	.017	.017
T X 2011	6 to 12	Missed last week	4	3	.28	.312	.143
T X 2012	6 to 12	Missed last week	4	3	.661	.661	.248
T X 2011	13 to 18	Primary comp.	4	4	.004	.017	.017
T X 2012	13 to 18	Primary comp.	4	4	.008	.017	.017
T X 2011	13 to 18	Attended sec.	4	5	.264	.312	.143
T X 2012	13 to 18	Attended sec.	4	5	.127	.182	.079
Panel B: Heterogeneous effects							
T X 2011 - Female	6 to 12	Participation	5	1	.006	.062	.066
T X 2011 - Male	6 to 12	Participation	5	1	.289	.469	.33
T X 2012 - Female	6 to 12	Participation	5	1	.049	.145	.122
T X 2012 - Male	6 to 12	Participation	5	1	.051	.146	.123
T X 2011 - Poorest	6 to 12	Participation	6	1	.032	.116	.095
T X 2011 - Less poor	6 to 12	Participation	6	1	.358	.53	.347
T X 2012 - Poorest	6 to 12	Participation	6	1	.037	.127	.106
T X 2012 - Less poor	6 to 12	Participation	6	1	.196	.342	.26
T X 2011 - Orphan	6 to 12	Participation	7	1	.906	.963	.657
T X 2011 - Non-orphan	6 to 12	Participation	7	1	.002	.062	.066
T X 2012 - Orphan	6 to 12	Participation	7	1	.429	.583	.378
T X 2012 - Non-orphan	6 to 12	Participation	7	1	.009	.064	.066
T X 2011 - Female	6 to 12	Ever attended	5	2	.006	.062	.066
T X 2011 - Male	6 to 12	Ever attended	5	2	.13	.253	.191
T X 2012 - Female	6 to 12	Ever attended	5	2	.011	.064	.066
T X 2012 - Male	6 to 12	Ever attended	5	2	.013	.064	.066
T X 2011 - Poorest	6 to 12	Ever attended	6	2	.03	.115	.095
T X 2011 - Less poor	6 to 12	Ever attended	6	2	.11	.246	.184
T X 2012 - Poorest	6 to 12	Ever attended	6	2	.042	.13	.108
T X 2012 - Less poor	6 to 12	Ever attended	6	2	.01	.064	.066
T X 2011 - Orphan	6 to 12	Ever attended	7	2	.452	.583	.378
T X 2011 - Non-orphan	6 to 12	Ever attended	7	2	.003	.062	.066
T X 2012 - Orphan	6 to 12	Ever attended	7	2	.115	.246	.184
T X 2012 - Non-orphan	6 to 12	Ever attended	7	2	.003	.062	.066
T X 2011 - Female	6 to 12	Missed last week	5	3	.475	.588	.382
T X 2011 - Male	6 to 12	Missed last week	5	3	.404	.573	.369
T X 2012 - Female	6 to 12	Missed last week	5	3	.463	.583	.378
T X 2012 - Male	6 to 12	Missed last week	5	3	.99	.991	.689
T X 2011 - Poorest	6 to 12	Missed last week	6	3	.962	.977	.673
T X 2011 - Less poor	6 to 12	Missed last week	6	3	.122	.252	.19
T X 2012 - Poorest	6 to 12	Missed last week	6	3	.962	.977	.673

Continuation of Table A5							
Estimate	Ages	Outcome	Table	Column	P-value	BH	BKY
T X 2012 - Less poor	6 to 12	Missed last week	6	3	.509	.608	.4
T X 2011 - Orphan	6 to 12	Missed last week	7	3	.543	.637	.428
T X 2011 - Non-orphan	6 to 12	Missed last week	7	3	.373	.54	.355
T X 2012 - Orphan	6 to 12	Missed last week	7	3	.718	.786	.518
T X 2012 - Non-orphan	6 to 12	Missed last week	7	3	.695	.777	.518
T X 2011 - Female	13 to 18	Primary comp.	5	4	.013	.064	.066
T X 2011 - Male	13 to 18	Primary comp.	5	4	.064	.168	.138
T X 2012 - Female	13 to 18	Primary comp.	5	4	.016	.069	.066
T X 2012 - Male	13 to 18	Primary comp.	5	4	.069	.173	.143
T X 2011 - Poorest	13 to 18	Primary comp.	6	4	.093	.226	.171
T X 2011 - Less poor	13 to 18	Primary comp.	6	4	.013	.064	.066
T X 2012 - Poorest	13 to 18	Primary comp.	6	4	.128	.253	.191
T X 2012 - Less poor	13 to 18	Primary comp.	6	4	.021	.083	.071
T X 2011 - Orphan	13 to 18	Primary comp.	7	4	.102	.239	.179
T X 2011 - Non-orphan	13 to 18	Primary comp.	7	4	.012	.064	.066
T X 2012 - Orphan	13 to 18	Primary comp.	7	4	.014	.066	.066
T X 2012 - Non-orphan	13 to 18	Primary comp.	7	4	.04	.13	.108
T X 2011 - Passed	13 to 18	Primary comp.	8	1	.006	.062	.066
T X 2011 - Not passed	13 to 18	Primary comp.	8	1	.28	.465	.327
T X 2012 - Passed	13 to 18	Primary comp.	8	1	.005	.062	.066
T X 2012 - Not passed	13 to 18	Primary comp.	8	1	.558	.643	.434
T X 2011 - Female	13 to 18	Attended sec.	5	5	.697	.777	.518
T X 2011 - Male	13 to 18	Attended sec.	5	5	.141	.267	.201
T X 2012 - Female	13 to 18	Attended sec.	5	5	.442	.583	.378
T X 2012 - Male	13 to 18	Attended sec.	5	5	.115	.246	.184
T X 2011 - Poorest	13 to 18	Attended sec.	6	5	.456	.583	.378
T X 2011 - Less poor	13 to 18	Attended sec.	6	5	.421	.583	.378
T X 2012 - Poorest	13 to 18	Attended sec.	6	5	.315	.499	.339
T X 2012 - Less poor	13 to 18	Attended sec.	6	5	.333	.515	.339
T X 2011 - Orphan	13 to 18	Attended sec.	7	5	.926	.969	.664
T X 2011 - Non-orphan	13 to 18	Attended sec.	7	5	.351	.53	.347
T X 2012 - Orphan	13 to 18	Attended sec.	7	5	.505	.608	.4
T X 2012 - Non-orphan	13 to 18	Attended sec.	7	5	.172	.312	.232
T X 2011 - Passed	13 to 18	Attended sec.	8	2	.203	.345	.263
T X 2011 - Not passed	13 to 18	Attended sec.	8	2	.174	.312	.232
T X 2012 - Passed	13 to 18	Attended sec.	8	2	.055	.149	.126
T X 2012 - Not passed	13 to 18	Attended sec.	8	2	.727	.786	.518

Note: All treatment effect estimates for tables 4-8 displayed. Midline and endline treatment effects are abbreviated T*2011 and T*2012, respectively BKY stands for Benjamini, Krieger, and Yekutieli q-values. BH stands for Benjamini and Hochberg q-values. Q-values represent the smallest level at which the hypothesis is rejected.