Cashing Out the Benefits: The Spillover Impact of Cash Transfers on Household Educational Investment

Mitchell Garrett Ochse
Matheus Dias

Professor Xiao Yu Wang, Faculty Advisor
Professor Michelle Connolly, Faculty Advisor

Honors Thesis submitted in partial fulfillment of the requirements for Graduation with Distinction in Economics in Trinity College of Duke University.

Duke University
Durham, North Carolina
2019

Acknowledgements

We are extremely helpful for the endless guidance by our advisors, Professors Xiao Yu Wang and Michelle Connolly, in every part of this endeavor. We are confident this project would be far less enriching without their consistent advice, encouragement, and endless patience for our ramblings along the way. We also extend the warmest gratitude to our peers from our Econ 495 and 496 seminars. Their sources of constant feedback and advice was an undeniable source of support from the very beginning.

Abstract

This paper utilizes a Randomized Cash Transfer program to assess the intra-household spillover effects on the school enrollment rates of individuals who were not essential for transfer distribution (non-core respondents). We find that conditional exposure to transfer amounts higher than the first quartile of distributed transfer values increased school enrollment for younger (3-12-year-old) female non-core respondents by around 5% and modestly increased rates for younger male non-core respondents, while there was little effect on teenagers (13-22-year-old). However, these effects depended on the pre-intervention enrollment status of the core survey respondent. Finally, unconditional exposure had minimal effects on 3-22-year-old non-core respondents.

JEL classification: C93; I21; I24

Keywords: Conditional Cash Transfers; Unconditional Cash Transfers; Education; Intra-Household Resource Allocation; Intra-Household Spillovers

Introduction

Despite the almost universal findings of positive returns to education, child enrollment in school remains far from a salient investment for families around the world. World Bank estimates conducted in the 2010s find a range of primary school enrollment rates between 52.2% and 99.9% worldwide, with secondary school rates as low as 19% in some countries.[1] Such a trend appears paradoxical considering the positive pecuniary returns to education worldwide, with an even larger magnitude of returns in developing countries with lower levels of educational attainment (Psacharopoulos 2018). The motivating factor of this paper is to add to the body of research that investigates the question: What forms of policy intervention can adequately improve educational outcomes? More specifically, we extend existing analysis of the impact of randomized Unconditional (UCT) and Conditional (CCT) Cash Transfers on household educational outcomes.

While a wide range of factors play into household decisions to educate their children, credit constraints and the *perceived* returns to education of household decision are particularly relevant in the context of CCT and UCT programs. For one, credit constraints impose an upper bound on household educational investment due to financial limitations and potentially nonexistent credit markets to provide additional capital. Thus, households may not be able to invest the desired amount in their children's education if they lack sufficiently liquid assets to do so. Secondly, the perception of the returns to education – or the value each household believes the returns of sending their children to school to be – may be very different from the *actual* returns. A growing literature incorporates the role of parental expectations in child educational outcomes, and many studies indicate that this expectation can influence child education decisions, such as continuing postsecondary education (Hossler and Stage, 2012).

[1] Data obtained from: https://data.worldbank.org/indicator/se.prm.nenr

As an increasingly popular form of poverty alleviation, CCT and UCT programs provide households with an immediate increase in household income; CCT programs mandate certain household conditions to receive funds, such as enrollment in schools, while UCT transfers come as no-strings-attached. Comparing household responses to UCT and CCT transfer interventions provides an ideal setting to measure both the effects that an alleviation of credit constraints and a possible shift in the perception of educational returns have on school attendance of children in a household. More specifically, UCT responses expose the impact of an increase in household income on school enrollment, thus allowing us to partially infer the effect of financial constraints in our sample. On the other hand, CCT treatment responses might capture both the effect of increasing household income as well as the impact of transfer conditionality on household preferences for education. Therefore, comparing outcomes between CCT and UCT programs provides an opportunity to parse out the impact of these two household-level considerations.

Our analysis utilizes data from Malawi's Schooling, Income, and Health Risk Impact Household Survey (SIHR), which featured a bilateral randomized Cash Transfer program with both UCT and CCT branches. Conditionality of the CCT branch centered on school enrollment for a core group of females aged 13-22 years old at the onset of the study (henceforth core group). Baird et al. (2011) extensively analyzes the immediate effects of the SIHR study on educational and health-related outcomes on this core female group, but this existing analysis does not consider the extensive schooling data available for *other* school-aged (3-22 years old) individuals who were not part of this original core cohort (henceforth non-core group).

In this paper, we investigate the intra-household spillover impact of the SIHR survey on the educational outcomes of this non-core group, as well as how the impact of transfer exposure differs between households who received conditional and unconditional transfers. After replicating the original results on the core respondents of the study presented in Baird et al. (2011), we investigate the school enrollment rates of the non-core group. Controlling for potentially confounding covariates (and confirming our results with a Fixed Effects model), we estimate the effects of transfer exposure on the probability that these non-core individuals were at school at a given year of the survey. Additionally, variation in transfer amounts allows us to investigate the extent to which households may be credit constrained and measure how non-core enrollment patterns depended on different levels of capital inflow. We find significant evidence suggesting that CCT exposure improved school enrollment rates for non-core females and males younger than the original group identified in Baird et al. (2011), though only for transfer values larger than the first quartile of transfer values, while effects for individuals that are the same age as the core females are minimal. UCT transfers, however, appear to have little impact on school enrollment rates on the entire non-core group. We also find transfers had insignificant effects on treated households where the core female was not in school pre-intervention, suggesting the presence of both financial and unobserved factors that directly influence household decisions for education.

This analysis may yield significant insight for many policy issues surrounding poverty and education. First, the impact of these educationally targeted transfers (and their conditions) prove to be more or less effective for certain groups in our sample. Moreover, the perception of parents to their own educational returns or those of their older children is likely to affect how they allocate resources to their younger children's education, suggesting an entanglement of education preferences between generations that might contribute to a "poverty trap." This inquiry hopes to address these connections and determine how educational preferences and credit constraints can affect the results of Cash Transfers policies. Lastly, there is little research on the intra-household spillover effects of conditionality of cash transfer programs for those in the household whose educational status is not required to receive funds. Our insight provides a deeper understanding on the full impact of cash transfer programs and adds to a growing body of research that seeks to improve their effectiveness in addressing global education trends.

Literature Review

Educational Outcomes

Recognizing the important role of the *perceived* returns to education as opposed to empirical estimates is a critical distinction in education research, and many studies focus on students' perceptions as an integral force on education outcomes. Jensen (2007) emphasizes the importance of perceived returns to schooling as the main determinant in educational investment and enrollment, finding that students in the Dominican Republic completed more school on average after receiving information on the countrywide average return to schooling (Jensen, 2007). Menon (2008) comes to similar conclusions, finding that Cypriot students who pursued college-level education perceived higher returns to education than those who opted to work after high school (Menon, 2008).

Equally important is determining how the educational perceptions of the child are formed, and a significant body of literature has attempted to address the role of parental influence both in forming child expectations as well as directly dictating child education decisions (Stage and Hostler, 1989; Maertens, 2011). The importance of parental influence in child education outcomes can be addressed from both a sociological and economic framework. From a sociological perspective, Haveman and Wolfe (1995) define the importance of parents as role models in the child's life, emphasizing the intergenerational transmission of values—including the perceptions of education—to the child (Haveman and Wolfe, 1995). For example, by using survey data collected in Jamaica, Cook and Jennings (2016) found that both parents and their children believe their value for education is directly influenced by their own parents' valuations of education in an affirmative association (Cook and Jennings, 2016). For the purposes of our study, these findings emphasize the importance for how parents value and perceive education both in molding the child's *perception* of education as well as his or her realized educational outcomes.

Another body of research considers the education level of the parent as an important driving force in child educational outcomes. Child Trends surveys conducted throughout the 2000s in the US report that parents with lower education levels tend to have lower expectations for the education attainment levels of their children compared to parents with higher education levels, suggesting the importance of parental education in forming educational aspirations for their child (Child Trends, 2015). Many studies confirm this trend, describing a significant relationship between parental education levels and education attainment expectations in their children, with some extending heterogeneous effects based on immigration status of the parents—including their country of origin—and geographic location of residence (Hossler and Stage, 1992; Bauer and Riphahn, 2004; Davis-Kean, 2005; Oketch et al., 2012). Along similar lines, while Dubow et al. (2009) found that parental education attainment was not directly associated with the education level or occupational 'prestige' of their children once the child is beyond adolescence, they deduce an important indirect effect in that parental education strongly influences education attainment up to age 19 (Dubow et al., 2009).

As referenced above, some research also attempts to investigate if the parental impact on educational outcomes is influenced by demographic characteristics, such as gender. Investigating gender identity from the perspective of the child, Glick and Sahn (1999) emphasizes the importance of the child's gender for education investment decisions, showing that increases in West African household income led to increasing investment in the education of girls in the household, but not for boys (Glick and Sahn, 1999). Making similar note to the importance of gender in education decisions, a significant body of research qualifies the impact of the gender of the child on education outcomes to show that the gender of the parent also matters in educational attainment outcomes overall as well as *between* gender identities of children within the household (Daouli et al., 2010; Zhang et al., 2007; Schoon et al., 2007). However, the impact of gender on the parent-child relationship is mixed across studies and suggests the importance of cultural values in evaluating the intersection of gender and education.

Consistent with this sociological framework, economic perspectives consider the role of the income level of the family in explaining discrepancies in childhood education levels, with many studies finding an importance of family socioeconomic status for both education attainment and outcomes (Child Trends, 2015; Sirin, 2005). Theoretically, households with higher incomes are able to both devote higher amounts of income to educate their children and rely less on their children as potential sources of income, especially in older children. Given the significant correlation between education and income, however, higher income levels may also be indicative of generally positive perceptions of education. For example, Delaney et al. (2011) finds that college students of a lower socioeconomic status tended to estimate their earnings profiles lower than those of a higher socioeconomic status, signaling a clear difference in the perceived returns to education based on family income (Delaney et al., 2011).

Other analyses further qualify this income gap by arguing income is both a direct factor in child education and development as well as an indirect representation of other important household characteristics. In a direct sense, some studies argue income impacts the child-rearing ability of parents through access to resources and time allocation, which are significantly inhibited at low income levels (Weinberg, 2001). As an indirect representation of other factors, McEwen and

Steward (2014) argue that, although family income does have a statistically significant effect on childhood development and labor market outcomes, the magnitude of the effect is substantially diminished after controlling for a multitude of other socioeconomic variables, such as parental support within the household and involvement in education (McEwen and Steward, 2014). This growing literature demonstrates that household decisions to invest in a child's education are influenced by an array of both economic and sociological factors. Parental and child-level expectations play an important role in shaping the perceptions of education as well as educational outcomes, but the exact mechanisms through which these preferences and others are expressed are more difficult to pin down.

Cash Transfer Programs

Cash Transfer programs have recently gained significant popularity both as a policy tool in lesser developed economies and in economic literature, with a particular emphasis on Conditional Cash Transfer (CCT) programs throughout the developing world. In the most general terms, CCTs are welfare programs that require participants to complete certain actions to receive the monetary transfer, often mandating physician checkups, child enrollment in school, or completion of certain educational training program. This is contrasted by Unconditional Cash Transfer (UCT) programs, which stipulate no conditionality of participation for the enrollees.

The scale and implementation of Cash Transfer programs is incredibly diverse and can range from a few thousand households to over 46 million families, as is the case for the *Bolsa Familia* CCT program in Brazil (Sugiyama, 2014). Programs also vary based on the extensive margin of their transfer amounts: *Oportunidades* in Mexico is among the highest, with transfer amounts representing on average 27% of the recipient's monthly income, while other programs contribute as little as 2-4% to monthly household income (Benhassine et al., 2014).

In a short-term framework, CCTs have been shown to increase enrollment and decrease dropout rates for children in educationally-focused CCT programs across a significant body of literature (Glewwe and Kassouf, 2012; Filmer and Schady, 2011; Benhassine et al., 2014; Garcia and Saavedra, 2017). In contrast, the effects of UCT are less studied than their CCT counterparts, partially due to their relatively young status as well as their lesser popularity as a form of welfare.[2] Haushofer and Shapiro (2016) found no significant effect of a UCT program in Kenya on household educational expenditure, but positive effects on investment and other metrics (Haushofer and Shapiro, 2016). In Burkina Faso, Akresh et al. (2013) found both UCT and CCT programs to be effective in increasing child primary school enrollment, but CCT programs better encouraged investment in children who were initially less likely to attend school likely due to the conditionality requirement (Akresh et al., 2013). Kilburn et al. (2017) found increased expenditure on children's education in a Malawi program, resulting in higher enrollment rates and lower dropout rates among those receiving transfer funds (Kilburn et al., 2017).

Some studies have attempted to analyze the medium-run effects of such programs. Perhaps most importantly, Baird et al. (2016) investigates the impact of the SIHR survey three years after transfers ended and suggests that UCT recipients did not gain sustained improvements in household empowerment, marriage rates, or labor market outcomes two years after the program ended, as well as reporting lower enrollment rates compared to the CCT branch. In contrast, CCT recipients found sustained decreases in teen marriages, birth rates, and nutritional intake within the same timeframe (Baird et al., 2016). Due to the recent nature of many of these programs, however, it is difficult to robustly analyze the long-term effects at this time. In the context of our analysis,

[2] Indeed, most Randomized UCT programs have taken place in the past 15 years and are relatively small in number. For a review of some of the earlier UCT programs, see Hulme et al. (2010). For a meta-analysis of the largest UCT programs in recent years, see Ferreira et al. (2014).

we focus on the immediate impacts of the SIHR on our identified group. This short-term focus in an important factor in determining the extent to which transfer interventions can have spillover effects when responses to the transfers are arguably strongest.

Data Sources: An Overview

Our dataset comes from the "Schooling, Income, and Health Risk Impact Evaluation Household (SIHR) Survey" conducted in Malawi from 2007-2010. The SIHR study took place within the Zomba district of Malawi, a region in the central part of the country with a population of approximately 583,000 out of Malawi's total of 13 million. Household surveys were conducted three times during the study, with each survey occurring approximately one year apart in late 2007-early 2008, early 2009, and early 2010 (we henceforth refer to the first survey round as the 2008 survey round). The purpose of the SIHR survey was to determine the efficacy of Unconditional (UCT) and Conditional (CCT) cash transfer programs on child educational and health outcomes, such as enrollment rates in school, standardized test scores, marital status, and HIV infection rates. The study focuses on households with female children aged 13-22 who had not been married at the start of the survey, and the 2008 survey identifies a cohort of 3,796 girls fitting this description across 3,432 households (the 'core females' sample). Most importantly, the cash transfer aspect of the study was tailored for this core female group; the SIHR specifically followed households with these core females so that all control and treatment households had *at least* one core female residing there in a given survey round.

Treatment assignment for the UCT and CCT transfer programs was determined at a community level so that all households within a specific community were assigned to the same treatment (being control, CCT, or UCT classifications). Of the 3,796 core females satisfying the survey conditions in the initial 2008 survey, 2,907 (77%) were found to be in school in 2008, of which 2,284 (78%) opted to be part of the program. Within these 2,284 participants, 1,495 (66%) were randomly assigned to the control group, 506 (22%) to the conditional (CCT) branch, and the remaining 283 (12%) to the unconditional (UCT) branch. The households that did not opt to be part of the program in a designated UCT and CCT treatment area (623 core females) were classified as the Communal Spillover treatment group. Survey information was still collected on these households that allows us to include them in our analysis. The remaining 889 (23%) of the 3,796 surveyed girls were those who were not enrolled in school in 2008, which we shall refer to as the Core Dropout Group. Girls in the Core Dropout Group were also divided into control and treatment groups, though they were only assigned to CCT treatments if they were in a community with cash transfer eligibility (CCT or UCT assigned communities). Of these 889 schoolgirls, 436 were assigned to a CCT program.

Though the focus of the SIHR study was on this core female group, surveyors collected additional information on others within the household, including household member age, relationship to the head of the household, educational status, and marital status. We therefore utilize this information to consider the intra-household spillover impacts on the educational attainment of those not considered core females in these households (our non-core sample). Our dataset allows us to identify which individuals in the non-core group were in each household classification and treatment exposure branch. Most importantly, individuals in this non-core group were considered ineligible for transfers; therefore, transfer impacts on this non-core group are purely spillover effects from our core female sample. We consider the treatment status for non-core individuals to be the treatment classification of the survey girl within the same household.

Theoretical Framework

We present a theoretical framework to incorporate expected returns of education into household decisions to send their children to school. Suppose that heads of household j are deciding whether to send child i to school in period t. For a given child the realized return to education, R_{it}, is forward-looking, and head of households j therefore consider enrollment decisions from the expectation of R_{it}, denoted $E_j(R_{it})$. We describe this by:

$$\boldsymbol{Equation\ 1}: E_j(R_{it}) \coloneqq f(\overrightarrow{B_j}, \overrightarrow{C_{it}}),$$

For simplicity, we ignore intertemporal discount factors. We assume the returns to education for child i likely depends on their gender and level of schooling—among other considerations—and denote a vector of these characteristics of child i that directly impact the realized returns to the child's education at time t as $\overrightarrow{C_{it}}$. We consider R_{it} to be a function of $\overrightarrow{C_{it}}$ as a set of fundamental characteristics that directly determine *actual* returns to education. These characteristics include but are not limited to age, scholarly aptitude, gender and marital status.

Secondly, head of households have revealed information as to the potential returns to education exogenous to their background, such as realized educational outcomes for previous children or others in their community. These are likely to vary with respect to the head's own experience – their educational level, income, and gender and to their region of residence. We denote these factors $\overrightarrow{B_j}$. This provides a mechanism through which parents receive information on the value of education, which influences their perceptions of the returns of education. Although we do not specify the functional form of $f(.)$, we would expect it to be monotonically increasing and marginally decreasing in all inputs.

Head of households would likely send child i to school if their expected returns from sending her to school in period $t, E(R_{it})$, outweighs K_{ij}, the pecuniary costs of sending the person to school, which we assume is time invariant. These are intrinsic household factors that influence educational perceptions independent of child-specific and temporal factors. Parents similarly consider these factors in the intensive margin for the amount they invest in their child's education. Define school enrollment for child i's education at time t as I_{it} - a binary variable that takes the value of 1 if the child is enrolled at the time of the survey and 0 otherwise. Then Equation 2 describes the parental decision model:

$$\boldsymbol{Equation\ 2}: I_{it} \coloneqq \begin{cases} 1 \;\; if\; E(R_{it}) \geq \; K_{ij} \\ 0 \quad otherwise \end{cases}$$

Since the returns to education are likely to be discontinuous (they depend on completing discrete grade levels or categorizations of school), head of households decide to send their children to school in order to receive more or less levels of education according to their optimal allocation generated by Equations 1 and 2, and their ability to pay for education in the short-term. This last factor could prevent parents from allocating optimally, as discussed in our introduction.

The impact of the conditional and unconditional transfers can now be seen more clearly in our model. If head of households prefer to send their children to school but are unable due to credit constraints, both transfers should reduce the impact of the financial constraints and allow for households to allocate closer to their unconstrained optimal choice– thus sending their children to school. While CCT directly decreases K_{ij} for the core females group, we still expect to see an additional effect of CCT relative to UCT in the non-core student's enrollment rate. This would come from a change in the preferences for education of the head of households, $\overrightarrow{B_j}$, and could vary in intensity and direction given different values of children's characteristics ($\overrightarrow{C_{it}}$). Finally, we have to reasons to believe K_{ij} would be especially higher for specific groups in our sample, such as the children who had already left school before intervention.

Empirical Specification

We specify a reduced form approach to find the relationship between parents' education and the decision to enroll the child in school, employing ***Equation* 1** as a generalized framework. Consider In_School_{ijt} to be an indicator variable for person i from household j being enrolled in school at time t. Due to the randomized nature of the transfer program, we employ a random effects time series Probit model to predict In_School_{ijt} as follows:

$\boldsymbol{Regression}\ \mathbf{1}$: $In\_School_{ijt} = \beta_0 + \beta_1 InTreatedHousehold_{jt} + \beta_2 MarginalImpactofUCT_{jt} + \beta_3 CommunalSpilloverHH_{jt} + \beta_4 CoreDropoutHH_{jt} + \beta_5 (CoreDropoutHH_{jt} * InTreatedHousehold_{jt}) + \vec{\lambda} * \overrightarrow{X_{ijt}} + \delta_t + \epsilon_{it}$

$InTreatedHousehold_{jt}$ is an indicator for individual i being in a CCT or UCT household in period t. $MarginalImpactofUCT_{jt}$ is a similar indicator but only takes the value 1 for UCT households and measures the marginal differences between CCT and UCT transfer exposure. Since the transfer intervention took place after the 2008 survey round, all treatment indicators take a value of 0 in the first period. This allows us to employ a difference-in-difference approach to investigate the impact of exposure to the program on the decisions to send children to school. We also control for the different household groups Communal Spillover and Core Dropout Group from the original study as a way to account for potential differences in these groups. We also interact treatment status with the Core Dropout group (who only received CCT treatment) to test for differences in program response between these households. $\vec{X}$ is a vector of individual time variant controls for age, Age_{it}, and marital status, $Married_{it}$, as well as the asset index measure of wealth for household j in baseline, $Asset_j$, the education level of the head of household, $HeadofHousholdEdu_{jt}$, and an indicator variable for the household be in an urban area in a given survey round, $Urban_{jt}$. Since a substantial portion of our literature discusses the relationship between parent and child educational attainment, we consider it prudent to control for this factor as a potentially significant variable in predicting child enrollment. Controlling for geographic factors through an urban and rural distinction may absorb factors relating to proximity to school/educational resources, as well as quality of schooling in influencing enrollment decisions. Lastly, since we are also concerned with potential correlations in enrollment patterns due to trends over time, δ_t is a vector of time series controls.

We also construct this specification and replace our $InTreatedHousehold_{ijt}$ and $MarginalImpactofUCT_{ijt}$ terms with our analogous measures for total transfer values to treated households and in a given period, $TransferAmount_{jt}$ and $MarginalUCTTransferAmount_{jt}$, respectively. To parse out gender differences, one further specification includes interactions with a male indicator variable for the gender of individual i, $Male_i$, with our $InTreatedHousehold_{ijt}$ and $MarginalImpactofUCT_{ijt}$ indicators. We consider this specification with gender interactions to be our preferred specification. This family of regressions indicates the extent to which household j's decision to enrolling child i in school at time t varies due to the treatment exposure, holding the covariates constant. Our literature review shows that presence of parents in the household, relationship to the head of household, and other factors may influence educational outcomes, and we therefore run a sensitivity analysis including these variables in Appendix D.

We extend this model to test for differential impacts on treated households based on transfer values. We achieve this by stratifying our transfer amounts into four quartiles to determine their impact on a coarser level than per dollar increases. We consider the following model:

$$\boldsymbol{Regression\ 2}: In\_School_{ijt} = \beta_0 + \overrightarrow{\beta_1} * \overrightarrow{TransferQuartile}_{jt} + \overrightarrow{\beta_2}(Male_i * \overrightarrow{TransferQuartile}_{jt}) + \beta_3 CommunalSpilloverHH_{jt} + \beta_4 CoreDropoutHH_{jt} + \beta_5(CoreDropoutHH_{jt} * \overrightarrow{TransferQuartile}_{jt}) + \vec{\lambda} * \overrightarrow{X_{ijt}} + \delta_t + \epsilon_{it}$$

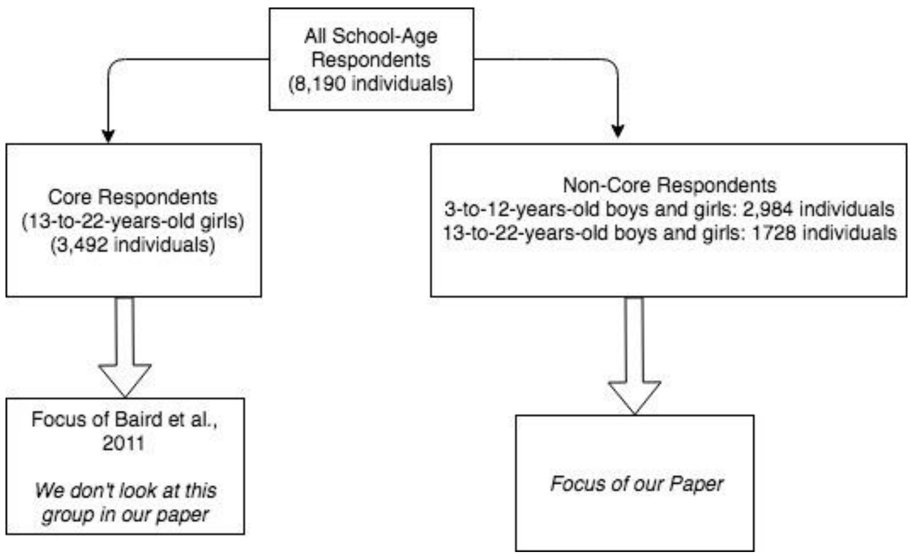
Here, $\overrightarrow{TransferQuartile}_{ijt}$ is a vector of 4 indicators determining if individual i in household j is in a given transfer quartile in period t, where Quartile 1 received the smallest transfer values, and Quartile 4 received the largest. We then interact our transfer quartiles with our male indicator to test for differential effects between males and females. We also control for Communal Spillover and Core Dropout households, with an interaction between Core Dropout households and our Transfer Quartile. For legibility in our analysis, we split these both into our Child and Teenage age groups, as well as UCT and CCT households for 4 total groups. In each specification, we exclude the households that received one of the transfer types (UCT or CCT) to directly compare the effects of the *non-excluded* transfer type to houses not receiving transfers.

Data

<u>Non-Core Sample Construction</u>

We construct our non-core sample to be the individuals who were not core females and were between the ages of 3 and 22 years old in the baseline 2008 survey. The purpose of this age group is to identify the cohort of individuals most likely to be in school during our survey timeline. This age range therefore allows us to determine the effects of the program on individuals who are 'school-aged' at some point during the survey as well as directly comparing results to the core female cohort. Figure 1 depicts the general breakdown of our study design. To achieve this, we construct our data set to include individuals that were able to be tracked across all three rounds of the SIHR survey. Of the 7,156 non-core children (aged 3-22) observed in the first round of the survey, we were able to match 4,698 respondents across all the three surveys (34.34% attrition). If we consider the attrition rate for the core females, this rate gets smaller: we successfully matched 3,492 of the original 3,796 (8% attrition). Within this successfully tracked non-core cohort of 4,698 individuals, 2,705 of these children are males, with the remaining 1,993 being females. Of these, 1,186 of the males and 528 females are in the baseline 13-22-year-old age range (same age as the original group of girls), with the remaining 1,519 males and 1,465 females being in the baseline 3-12-year-old group (younger than the original group of girls).[3]

Figure 1: Breakdown of Successfully Tracked Core and Non-Core Samples

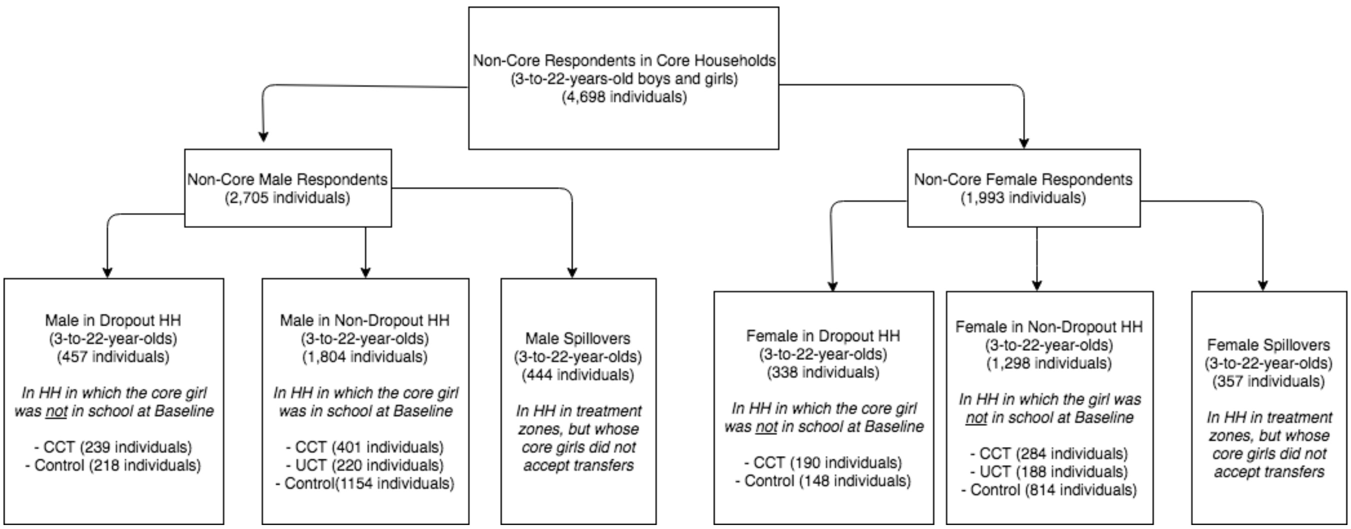


By considering the cohort of survey girls tracked across all three survey rounds, our sample size of 3,492 core respondents results in 480 girls in the CCT branch, 264 in the UCT, and 383 in the treated (CCT) Core Dropout group. Figure 2 presents a full breakdown of the spillover treatment exposure for the non-core males and females to these tracked core females. This information allows us to determine the effect of exposure to household cash influxes on our entire

[3] The exact reason the non-core females in the 13-22 age range were ineligible for transfers remains unclear. The study design suggests the only reason these girls would be considered ineligible is if they were previously pregnant or marred. However, we find a marriage rate of 2.27% in the 2008 study among this group. This data set does not provide pregnancy information for this group, so we cannot determine if this was the primary factor. We consider this to be a large limitation for our analysis in terms of comparing outcomes for this group to other non-core and core groups. Nevertheless, it is still a worthwhile endeavor to measure the impact of the program on this group.

non-core sample. Overall, we find a consistent distribution of treatment exposure in each round compared to the original core girl breakdown, with ~25% of the non-core sample in CCT households, ~9% in UCT households, ~50% in Control households, and ~18% in Spillover households.[4]

Figure 2: Treatment Assignment for Non-Core Group (Tracked all 3 Survey Rounds)



As the main variable of interest for our study, Table 1 details the enrollment statistics of our non-core sample over the three rounds of the survey, stratified by our main groups of non-core males and females. The striking relationship between the two groups is the relative similar trends of enrollment for males and females. When further stratifying by the baseline age ranges of 3-12 and 13-22 (non-core Teenagers), we still find similar enrollment percentages across genders and enrollment trends across these age groups. We also find that marriage rates in our sample are quite

[4] We note that there is some movement of our core and non-core group between survey rounds 2 and 3, although the presented percentages are robust across all rounds and as a whole treatment exposure is rather rigid over time. This includes a small number of households in round 3 that share both UCT and CCT transfers. However, this group accounts for < 1% of our non-core individuals (n = 41), and we therefore do consider this group of transfer contamination to be necessary to control for.

low, at an overall rate of only ~2% in our non-core sample at the first survey.[5] This may be a result of bias in our non-core group that we successfully track if those who marry leave the surveyed households. We discuss the implications of this source of bias in our findings section.

Table 1: School Enrollment Rates of Non-Core Sample per Survey Round

Enrolled in School during Survey Round	(1) Survey Round 1 (2008)	(2) Survey Round 2 (2009)	(3) Survey Round 3 (2010)
Non-Core Males			
In School	1980	2238	2224
Not in School	725	467	481
Percent in School	73.20%	82.74%	82.22%
Percent in School (By Age Range in Survey Round 1)	***3-12:*** *62.93%* ***12-22:*** *86.34%*	***3-12:*** *81.04%* ***12-22:*** *84.91%*	***3-12:*** *90.06%* ***12-22:*** *72.17%*
Non-Core Females			
In School	1377	1658	1732
Not in School	616	335	261
Percent in School	69.09%	83.19%	86.90%
Percent in School (By Age Range in Survey Round 1)	***3-12:*** *63.50%* ***12-22:*** *84.66%*	***3-12:*** *82.93%* ***12-22:*** *83.90%*	***3-12:*** *91.67%* ***12-22:*** *73.67%*

Transfer Margins

Cash transfers to program participants began in the second round of the survey (2009) and lasted until the end of the program in 2010. Within this structure, transfer amounts were randomly assigned on two levels. At the first level, households were assigned different transfer amounts within the cash transfer branches of the study. Transfers were provided in units of Malawian Kwacha of $MK 560, $MK 840, $MK 1120, or $MK 1400 per month ($4, $6, $8, $10 USD, respectively) at the household level per core female. These values were given directly to the identified head of household, and the amount was randomized at the community level. Since transfer amounts were given per core female in the household, it is possible for households to receive total transfer amounts greater than these individual values.

Secondly, additional transfers were given directly to the core females in the CCT or UCT branches. Transfer amounts to the core females were either $MK 140, $MK 280, $MK 420, $MK 560, or $MK 700 Malawian Kwacha per month ($1, $2, $3, $4, $5 USD, respectively) and were

[5] By the third round, all of these marriages except for two female instances occurred from individuals in the 13-22-year-old age range.

randomized at the household level so that girls within the same household would receive the same transfer amount. Appendix A presents a breakdown of household transfer amounts per core female, and total transfers to each household. Since we focus on school enrollment on a yearly basis, we consider the total transfer value a household receives *each year*. To do this, we assume each household receives all transfer payments in a year based on the core females present in the household roster during the start of a given survey round. This result may present an inaccurate picture if a core female were to move households *in between* survey rounds by overstating the transfer amount to the original household and understanding the transfers to the new household. Ideally, this effect would balance out due to the symmetry of the transfer bias, but we do note this is a potentially strong assumption to make and a limitation of our analysis.

Due to the conditionality requirements of sending children to school, participants in the CCT had their school tuition costs paid for. However, those in the UCT branch eligible for secondary school also received a transfer premium equal to the average cost of secondary school paid by the CCT branch.[6] We construct an estimation of the 'wealth effect' to the household, which measures the program cash transfer value for the UCT group and the sum of the transfer plus the average tuition (for the CCT branch). Table 2 presents the average yearly transfer and tuition amounts given to households as well as the conditional recipient's tuition by transfer group. Comparing this to our average household monthly expenditures in Appendix 2 Table 2B, we see that the average yearly wealth effect for those receiving transfers is equivalent to nearly 2-3 months of household total expenditures.

[6] Since public primary school is free in Malawi, CCT-assigned girls enrolled in primary school were not considered for tuition reimbursement. Similarly, UCT-assigned girls enrolled in primary school were not given a transfer premium compared to CCT transfer amounts. Our estimations reflect this distinction.

Additionally, the average wealth effect to each household is close in magnitude between the CCT and UCT households, though slightly higher within the UCT program. The overall similar size of the wealth effect between programs allows us to better test the importance of conditionality within the household given similar transfer values to the households. One distinction is that those in the UCT report an increase in average tuition levels between rounds 2 and 3, while those in CCT programs report a decrease in tuition between rounds. This may reflect a response to attend more expensive schools in response to the transfers by those in UCT households. However, since we do not have specific school identifying information or tuition values for the non-core children and incomplete school identifying information, addressing this potential discrepancy is difficult to achieve for our non-core group. We therefore consider this a limitation of our study.

Table 2: Average Tuition and Transfer Amounts for Treated Households -$MK(2008)

	(1)	(2)	(3)
Wealth Effect per Survey Round	Tuition	Transfer	Wealth Effect
Unconditional Transfers			
Round 2 (2009)	$6,150.95	$26,040.81	$26,040.81
Round 3 (2010)	$7,569.66	$24,505.02	$24,505.02
Conditional Transfers			
Round 2 (2009)	$6,042.13	$17,977.59	$24,019.72
Round 3 (2010)	$5,020.32	$18,245.38	$23,265.70

Estimating Household Wealth

Without direct data on household-level income, our dataset allows for two methods as proxies. The first uses total monthly household expenditures to represent household income flows as reported by the head of household. The second approach utilizes surveys conducted at all stages of the intervention on household ownership of 25 assets, as well as access to electricity and materials of the household. We use household ownership of these assets to construct an asset index through Principle Component Analysis (PCA) as defined by Filmer and Pritchett (2001).[7] We construct an indicator variable for materials of the household, differentiating between low- and high-quality materials (mud, grass vs. brick, concrete, steel). We follow a generalized approach that utilizes the resulting first principle component vector to serve as weights for determining the impact ownership of each individual asset on the overall household asset. Construction of the index, weights of all items in the index, and resulting summary statistics are shown in Appendix B. The resulting statistics for the household asset index and monthly household expenditures distribution can be found in Table 7 below. The asset index shares a correlation of .536 with baseline monthly household expenditures.[8] The signage of the resulting weights lies largely in-line with expected correlation between household material wealth and ownership of specific items.

Constructing an asset index provides a richer picture of household wealth that serves as a less volatile representation of wealth compared to monthly expenditure. Ownership of assets is generally less prone to change in response to economic shocks and therefore represents a longer-term interpretation of household wealth compared to total household expenditure data. The asset index, however, comes at the cost of being more resilient to economic shocks compared to total expenditures, which better mirrors household responses to the shock. Our investigation benefits from the rigidity of the asset index to capture initial material wealth of the household before the program intervention. Allowing expenditures to vary over time captures behavioral responses within the household to the transfer program so that holding per-round expenditures constant as a measure of household wealth may be biased.[9] This approach is limited in that we do not have

[7] See Filmer and Pritchett, 2001; Booysen et al., 2007; Vyas and Kumaranayake (2006) for an overview on the use of PCA Asset Index Constructions to estimate household wealth.

[8] Within our model, the first principle component accounts for 20.6% of the variation in our list of assets. Given our resulting correlation level with monthly total expenditures, we consider this level of explanatory power to be sufficient enough to use for our analysis.

[9] Indeed, we find a significant increase in monthly expenditures between survey rounds 1 and 2 in treated households relative to the control trend; while control households found an increase in yearly expenditures of ~ $MK800, all CCT

household information on the children before the survey, and thus may not fully capture the material wealth of the child's upbringing. However, this approach still allows us to gauge wealth at the time of the survey intervention and its subsequent effects in investing patterns.

Household Characteristics

Our primary household variable of interests are our constructed asset index values as well as level of education attainment of the household and Geographic location as defined by the SIHR survey (Urban vs. Rural location). This level of education for the households of survey girls can be found in Appendix C Table 1C. Most of the heterogeneity in head of household educational attainment occurs in four groups, being no formal education, primary education, and secondary education, and a small amount in post-secondary (university and training school). Therefore, we focus our analysis on these four main levels of education.

The relative increase in household heads with a level of Secondary School education seems to be driven in-part by the increased instances of marriage in the core female sample between survey rounds 2 and 3. 36% of households with married core females have a head of household secondary school education level, compared to 24% for households where the core female was not married. More importantly, 67% of the core females married in 2010 were the spouse of the identified head of household, which indicates a likely movement of core females into households where their spouse is now the head. The increased education level of these newly married heads of households may represent a general trend of increasing education levels for younger generations of Malawians compared to the older individuals in our sample. As for our geographic control, we

households had an increase of ~$MK1200, and UCT of ~$MK2500. This suggests total expenditures are at least partially capturing responses to the program, so that controlling for total expenditures in our model may be confounding the impact of transfers. Still, we specify our model using total expenditure values, asset index values, and both. Our results are robust across all specifications.

note that the number of households in an Urban setting remain strongly constant between 16.08% and 16.22% across all survey rounds.

We also consider whether our main household groups were significantly different on several important variables before program intervention in Survey Round 1, which we present in Appendix C Table 2C. The results suggest some differences across groups. Since those households in the Core Dropout Group appear to have lower asset index values, expenditure levels, and lower head of household education, we consider it prudent to control for this group in our analysis for potentially unobserved confounding factors. Similarly, the decision by those in Communal Spillover households to not participate in the transfer program lends some concern for unobserved differences between Communal Spillover and non-Communal Spillover households. However, Table 4 alleviates these concerns in the relatively similarity between Communal Spillover households and our other (non-Core Dropout) groups. We argue that those in Communal Spillover households still provide an important avenue to investigate if transfer programs yield significant gains for those that do not directly receive transfers, and we control for these households across our specifications. We also note that UCT households appear to be slightly wealthier, more educated, and have a higher percentage in urban areas at baseline. While we cannot directly address the possibility these UCT households are different from our other groups in some unobserved manner, we hope to capture these effects by controlling for these factors in our analysis.

Findings

Replication of Baird et al. (2011) on Core Females

In this preliminary model, we emulate the main regressions from the paper by Baird et al., (2011). We do this to compare our empirical specification to the results of previous research using this dataset. Specifically, we cannot control for school grades and sexual activity as Baird et al.,

(2011) does, since this information is not available in our dataset. Instead, we add controls for marriage and pregnancy. The results are presented in Table 3 below, where the outcome is the teacher-reported school enrollment.[10] In column (2), we show a specification similar to the primary model in Baird et al. (2011) and add additional controls in column (3). We keep the CCT and UCT variables separate as is done in Baird et al. (2011) without measuring marginal effects.

Table 3: Likelihood of Core Female School Enrollment (Replication)

VARIABLES	(1) In School (Terms) (Baird et al. (2011))	(2) In School (Year) (Partial Controls)	(3) In School (Year) (Full Controls)
CCT Treatment	0.602***	0.743***	0.830***
	(0.142)	(0.0589)	(0.0615)
UCT Treatment	0.199	0.342***	0.137
	(0.195)	(0.0876)	(0.0863)
Asset Index		0.173***	0.0959***
		(0.0190)	(0.0190)
HH Education		-0.0513*	0.0137
		(0.0290)	(0.0294)
Core Dropout at Baseline Group		-1.973***	-1.856***
		(0.0617)	(0.0592)
Communal Spillover		0.160***	0.164***
		(0.0596)	(0.0583)
Survey Round 2		-0.224***	0.152***
		(0.0498)	(0.0506)
Survey Round 3		-1.282***	-0.801***
		(0.0479)	(0.0510)
Age		-0.190***	-0.132***
		(0.0111)	(0.0100)
Urban			0.0253
			(0.0587)
Married			-1.987***
			(0.0834)
Pregnancy			-1.075***
			(0.0620)
Constant		4.768***	3.588***
		(0.207)	(0.186)
Observations		10,143	10,143
Number of unique observations		3,381	3,381

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[10] In this specification, Baird et al. (2011) assumes that the missing values are zeros if the school reported does not exist and ones otherwise. Since this information was not made publicly available, we were not able to replicate it.

In general, our analysis both confirms and qualifies the results of Baird et al. (2011) in showing that CCT exposure for the core female sample increased likelihood of school enrollment during the survey period, but our results differ in that the effects of UCT exposure depending on the specification. Column (1) finds strong effects for CCT exposure and weak effects for UCT exposure, but we do, find very similar results in column (2) to Baird et al. (2011) when we similarly control for age and household wealth (asset index), including the fact that the impact of UCT exposure is ~46% smaller than CCT exposure compared to ~43% in the original paper. Marriage and pregnancy status is an important factor in educational decisions, and Baird et al. (2011) finds that UCT exposure helped decrease instances of these factors significantly compared to CCT exposure. This may explain the difference in UCT impact between columns (2) and (3) when we control for these factors as well as why UCT exposure is no longer significant, as the main specification in Baird et al. (2011) does not control for marriage or pregnancy. Additionally, the significance of our asset index, marital status, age, and Core Dropout and Communal Spillover households strengthen the validity of our index construction and model specification.

Non-Core Sample

We now consider the Non-Core cohort group, and Table 4 shows the results for $\boldsymbol{Regression\ 1}$. Beginning with a discussion for CCT results, column (1) finds that non-core students in CCT households are significantly more likely to be enrolled in school than those in households without transfers. In column (2), we supplement treatment exposure for transfer value to the household (in thousands of Kwacha), and find strong effects for transfer amounts that could suggest higher transfer values result in higher enrollment rates. We will return to further analysis of transfer values, but for now we focus on basic treatment exposure. Finally, in column (3) we further stratify the impact of treatment exposure between and non-core males and females. We find that boys are significantly less likely to benefit from transfers than non-core females; in fact, the effect of program exposure on non-core males is insignificant to a 10% size of test and suggests the program had small effects on male enrollment rates overall.

Across all specifications, the impact of UCT transfers is found to be insignificant for both non-core males and females, as evidenced by the negative marginal effect of UCT exposure; further significance tests confirm those in UCT households were not significantly more likely to be enrolled compared to those in untreated households. As a robustness check, Appendix D Table 1D includes a version of this regression with additional controls for other variables inspired from our literature review (summary stats in Appendix C Table 3C), including the presence of parents, relationship to head of household, and the number of children in each household. These results largely confirm our model specified above, and only the status of the non-core individual being a child of the head of household yields a significant impact on enrollment likelihood. This confirms a modest importance in the relationship of the child to the head of household in education-related decisions, but we choose not to focus on this for our subsequent analysis.

Non-Core Age Groups

From this generalized framework, these results suggest that CCT programs significantly improve enrollment rates for non-core females, though the effect on non-core males and of UCT programs in general were not highly significant. However, the wide age range of our sample consists of different stages in the potential education of a child and teenager. More specifically, households deciding to send older students to school may have fundamentally different responses to transfers than enrolling a younger child in school. The age range specified by Baird et al. (2011) provides a natural point of differentiation in our sample, first as a way to separate outcomes for younger and older students, and also as a way to compare results for non-core students in the same

Table 4: Likelihood of Non-Core School Enrollment

VARIABLES	(1) In School Treatment Exposure (All Rounds) (Non-Core Sample)	(2) In School Transfer Amounts (All Rounds) (Non-Core Sample)	(3) In School ((1) +Gender Controls) (All Rounds) (Non-Core Sample)
In Treatment Household	0.262***		0.433***
	(0.0847)		(0.107)
Marginal Effect of UCT HH	-0.194		-0.316*
	(0.118)		(0.168)
Total HH Transfer Size Amount		0.00844***	
		(0.00298)	
Marginal Effect of UCT Transfer Amount		-0.00779**	
		(0.00337)	
Male			-0.120**
			(0.0587)
Marginal Effect of Male in Treatment HH			-0.283**
			(0.117)
Marginal Effect of Male in UCT HH			0.178
			(0.216)
Core Dropout HH	-0.578***	-0.552***	-0.537***
	(0.0786)	(0.0741)	(0.0745)
Marginal Effect of Treated Core Dropout HH	-0.0662	-0.0138	-0.0896
	(0.119)	(0.106)	(0.115)
Communal Spillover HH	0.108	0.0614	0.0846
	(0.0750)	(0.0707)	(0.0712)
Asset Index	0.104***	0.0821***	0.0849***
	(0.0278)	(0.0264)	(0.0264)
Education Level of H.o.H	0.0478	0.0384	0.0368
	(0.0496)	(0.0471)	(0.0472)
Urban	0.164**	0.158**	0.159**
	(0.0834)	(0.0792)	(0.0794)
Age	0.0431***	0.0585***	0.0620***
	(0.00535)	(0.00518)	(0.00529)
Married	-3.228***	-2.588***	-2.626***
	(0.147)	(0.218)	(0.219)
Survey Round 2	0.430***	0.643***	0.627***
	(0.0461)	(0.0431)	(0.0446)
Survey Round 3	0.466***	0.652***	0.631***
	(0.0468)	(0.0435)	(0.0451)
Constant	0.568***	0.210*	0.236**
	(0.119)	(0.112)	(0.115)
Observations	13,971	13,971	13,971
Number of Individuals	4,657	4,657	4,657

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

age range as the primary core survey girl group. We therefore use the specification from column (3) and stratify our main sample by two age ranges: those who were younger than the core female group (3-12 years old) in the first round of the survey, called non-core Children, and those who were the same age (13-22), called non-core Teenagers. The results are shown in Table 5 below.

Table 5: Likelihood of Non-Core School Enrollment, by Age Group

VARIABLES	(1) In School Non-Core Children (3-12 at Baseline) (All Rounds)	(2) In School Non-Core Teenagers (13-22 at Baseline) (All Rounds)
In Treatment Household	0.464***	-0.116
	(0.132)	(0.238)
Marginal Effect of UCT HH	-0.397*	0.0966
	(0.209)	(0.357)
Male	-0.184***	0.246*
	(0.0643)	(0.126)
Marginal Effect of Male in Treatment HH	-0.169	0.278
	(0.150)	(0.253)
Marginal Effect of Male in UCT HH	0.266	0.0188
	(0.292)	(0.419)
Core Dropout HH	-0.565***	-0.689***
	(0.0835)	(0.149)
Marginal Effect of Treated Core Dropout HH	-0.294*	0.142
	(0.152)	(0.222)
Communal Spillover HH	0.150*	0.0450
	(0.0825)	(0.134)
Asset Index	0.245***	0.159***
	(0.0299)	(0.0505)
Education Level of H.o.H	0.0773	0.0275
	(0.0536)	(0.0868)
Urban	0.252***	0.102
	(0.0947)	(0.141)
Age	0.519***	-0.447***
	(0.0186)	(0.0257)
Married	-1.461**	-2.109***
	(0.693)	(0.278)
Survey Round 2	1.171***	-0.142
	(0.0671)	(0.0884)
Survey Round 3	1.416***	-0.631***
	(0.0732)	(0.0870)
Constant	-3.710***	9.280***
	(0.178)	(0.518)
Observations	8,877	5,094
Number of Individuals	2,959	1,698

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

These specifications indicate that the primary benefactors of treatment exposure are non-core female Children in the conditionally treated households, with no significant effect for non-core female Teenagers nor for the unconditionally treated female Children. Similarly, results remain insignificant for non-core males of all age ranges and treatment types. The lack of significant results for our Teenager group starkly contrasts the significant findings of both the CCT and UCT transfer programs on the core survey girl group in Baird et al. (2011). We believe this difference is the consequence of two different shifts in parental perception of the returns to education. First, parents may have a higher perception of educational returns for younger children relative to older ones, which is potentially the product of a decreasing marginal return to education or of a higher fixed cost of sending a child back to school at an older age. If parents believe a higher return would exist for the education of younger over older children, this would provide a cohesive explanation for the significance of the transfers effect in the outcome of the younger children, and the insignificance for the older non-core group. Secondly, if the conditional transfers induce a higher parental perception of the returns of education to girls, head of households would allocate more transfer inflows and enforce school attendance stricter for girls relative to boys. This would explain the significance of the conditional transfer in the outcome for girls relative to the outcome for boys, and the insignificance of these results for the unconditional transfer.

A less theoretical justification could be the relatively small sample size of our non-Core Teenage female group, of which consists of only 528 total individuals that prevents us from having sufficient power to parse out effects of the program. Nevertheless, the impacts of the transfers on non-core female Teenagers is still important spillover effect of the program to consider.

Discussion of Controls

Across all specifications, we find that age, asset index value, marital status, and being in a Core Dropout household are strong predictors of school enrollment status. The signage for our asset index and marriage coefficients confirms our original hypothesis that houses with higher material wealth are more likely to send their children to schools, and married individuals are less likely to be in school. The lack of significance for the head of household education comes as somewhat surprising, but due to the fact the majority of our observations are concentrated on the primary and secondary level, this rather coarse description of education may not be sufficient to truly parse out any effect education may have in enrollment decisions. Similarly, the effect of household education may be captured in other variables, such as our asset index measure.

Perhaps the control most worthy of discussion is the strong negative significance of the Core Dropout indicator. This outcome suggests that these households are significantly less likely to send their children to school in general terms. Additionally, when we consider our interaction term of the Core Dropout dummy and treatment (i.e. CCT), the differential impact of transfer exposure appears directionally negative, but insignificant. However, in testing the significance of the program with our preferred specification in Column 3 in Table 3, we find that significance levels for the treated Core Dropout households are attenuated and only significant to a 10% size of test. Further stratifying our sample into our two age groups finds the overall effect of the treatment for both non-core males and females in Core Dropout households is insignificant.

This suggests weaker responses to treatment exposure for non-core child enrollment within Core Dropout households, which is likely the product of both credit constraints and household preferences. Since Core Dropout households are significantly less wealthy before program intervention, this insignificant response to transfers could represent the presence of financial constraints within the household that still prohibit households from sending school-aged individuals to school. However, the fact these households are similar in the unenrolled status of the core female and the lower average education attainment of the household head in the first round of the survey suggests these households may also have lower evaluations of education for all school-aged individuals in the household compared to the non-Core Dropout cohort. Due to the fact Core Dropout group treatment is exclusively conditional, however, we are unable to compare UCT and CCT outcomes as a cursory investigation to parse out the importance of credit constraints and perceptions of education on these households. Still, the lack of a strong response to conditional treatment in these households suggests these households are less responsive to transfer interventions compared to non-Core Dropout households.

Robustness Check: OLS Fixed Effect Specification

The discussion for a difference in outcomes for our Dropout and non-Dropout households raises the concern for unobserved individual-level factors driving our results. As an attempt to address this possibility, we consider an OLS fixed effects model as a robustness test on our results above. We include a basic specification of treatment exposure with our age and marriage variables, as these are typically highly predictive factors in education decisions. We also include a time fixed effect in the form of an indicator for being in a treatment period (survey rounds 2 or 3). Because the results above indicate some potential for different effects of transfers on males and females, we further stratify by gender. Our results are in Table 6 below.

The results confirm our previous findings, both in our total Child-aged sample and in our stratified age and gender groups. We find strong effects of CCT exposure on non-core female Children, with insignificant effects on non-core male students. Similarly, all models also find an insignificant effect of UCT exposure on enrollment rates. As a departure from the models above, the fixed effect specification finds a slightly negative effect on CCT exposure to non-core female Teenagers to a 10% significance level, but due to the relatively small level of significance and our previous results, we do not claim this to be a robust result of significant effects on this group. Additionally, the marriage variable is significantly only for the Teenagers - a consequence of the low marriage rate in the Child group. While this OLS specification is no longer interpreted probabilistically, the similarities in significance and directionality to our Probit specification provides a secondary perspective that yields credibility to the robustness of our findings.

Table 6: OLS Fixed Effect Specification for Non-Core School Enrollment

VARIABLES	(1) In School - Non-Core Females 3-12 Years Old at Baseline (All Rounds) (Fixed Effect)	(2) In School - Non-Core Females 13-22 Years Old at Baseline (All Rounds) (Fixed Effect)	(3) In School – Non-Core Males 3-12 Years Old at Baseline (All Rounds) (Fixed Effect)	(4) In School – Non-Core Males 13-22 Years Old at Baseline (All Rounds) (Fixed Effect)
Treatment Household	0.0656***	-0.0602*	0.0295	0.00918
	(0.0226)	(0.0347)	(0.0223)	(0.0228)
Marginal Effect of UCT HH	-0.0887**	0.0494	-0.0548	0.0418
	(0.0361)	(0.0531)	(0.0367)	(0.0362)
Married	-0.204	-0.359***	0.00716	-0.196***
	(0.167)	(0.0547)	(0.166)	(0.0551)
Age	0.0881***	-0.0926***	0.0905***	-0.126***
	(0.0113)	(0.0159)	(0.0110)	(0.0111)
Treatment Period	0.180***	0.0116	0.176***	-0.0192
	(0.0134)	(0.0180)	(0.0128)	(0.0128)
Constant	-0.0468	2.311***	-0.0869	2.960***
	(0.0876)	(0.250)	(0.0871)	(0.184)
Observations	4,395	1,584	4,557	3,558
R-squared	0.186	0.092	0.174	0.083
Number of Individuals	1,465	528	1,519	1,186

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

<u>Transfer Margins</u>

While our results suggest a significant impact for exposure to the program *as a whole* for non-core female Children, we also suspect there are significant differences in enrollment decisions based on values of the transfer to the households. Due to the randomized values of transfers to CCT and UCT households, we now investigate the extent to which transfer amounts influenced household propensity to send their children to school. The regressions are included in Appendix C Table 3C, and we conduct joint significance tests to determine if exposure to each transfer quartile yielded significant impacts on enrollment status compared to our control group. These significance levels are present in Table 7 below, stratified by gender and our two main age groups. P-values for these significance levels are presented in Appendix D Table 3D.

We first note that the impact of these transfers on our Teenage group confirm our results from above, with little effect besides slight increases and decreases to enrollment rates. While Teenage females did find a slight decrease to enrollment rates in the UCT program, these effects are modest, and we do not argue for these to be compelling results for a strong negative effect on this group. This shows that households receiving transfers were not significantly likely to increase school enrollment for Teenagers regardless of transfer values, which supports our discussion that educational outcomes for our non-core Teenagers are much more resilient to exogenous factors.

Results for UCT transfers in the Child-age group remain starkly different than those for CCT transfers and holistically confirm our findings from our specifications above. While we find no significant effect on boys, Child-aged females also find a modest increase to enrollment rates for higher UCT transfer values in the first quartile, with a strong negative effect in the third quartile. This raises concern for a negative effect on school enrollment for large unconditional transfer values, but this fear can be mitigated by the lack of a strongly negative trend in enrollment as a response to the 2nd and 4th UCT transfer quartiles--in fact, these quartiles have insignificant effects on enrollment. While the randomization of the transfer program ideally makes these results resilient to unobserved differences in quartile transfer groups, this rather isolated result within the third quartile group raises some questions as to the underlying factors for this apparent spurious finding. We recognize this potentially heterogeneous outcome as a question for further research.

The most striking contrast of these findings compared to our analysis above is that the strong positive effects of CCT exposure are concentrated in households receiving larger transfer values. We find that households in these quartiles for CCT transfers were significantly more likely to send children to school, while there does not appear to be a significant effect for households receiving transfers in the lower quartiles. These results were strongest in the non-core females, although there is a slight increase to enrollment rate for boys in the CCT households receiving transfers values in the 2nd and 4th quartiles (significant at a 10% and 5% level, respectively). Similarly, these findings further show that treated Core Dropout households were not significantly more likely to send any non-core children to school compared to control households. In fact, while previous analysis showed some weak significance for this group, this specification shows almost no distinguishing effect within these households over those that did not receive transfers.

Table 7: Significance for Transfer Quartiles on Non-Core Enrollment, By Gender and Age

	Child Group (3-12 Years Old in First Survey)		Teenage Group (13-22 Years Old in First Survey)	
Effect of Transfer Exposure by Transfer Quartile	Female	Male	Female	Male
CCT				
CCT First Quartile				
CCT Second Quartile	(+)**	(+)*		(+)*
CCT Third Quartile	(+)**			
CCT Fourth Quartile	(+)***	(+)**		
Core Dropout Group Results				
CCT First Quartile				
CCT Second Quartile				
CCT Third Quartile	(+)*			
CCT Fourth Quartile				
UCT				
UCT First Quartile	(+)**		(-)*	
UCT Second Quartile			(-)*	(+)*
UCT Third Quartile	(-)***			
UCT Fourth Quartile				

(+) and (-) are the inferred direction of the transfer effect as being positive (+) or negative (-)

*** p<0.01, ** p<0.05, * p<0.1

Overall, these transfer results add a layer to nuance to our original exposure findings. Most notably, the strong effect on non-core females in the Child age range in the upper half of transfer values also overstate the significance of CCT exposure for small transfer values, which appear to have little actual effect on enrollment outcomes. Similarly, there is some evidence that larger CCT transfers may also help non-core male Children, though these results are less strong than for similarly aged females. We ultimately find that CCT transfer values seem to matter for the enrollment of our non-core cohort, which works to qualify the findings in Baird et al. (2011) in understanding the impact of transfer values to households. For one, Baird et al. (2011) finds that CCT effects are strong even in the smallest transfer households, and that higher transfer values did not spur a larger increase in enrollment for their core survey girl sample. However, our investigation finds that these small transfers may not benefit the enrollment of other children in the household, while larger transfers can (with exception to Core Dropout households). This presents a clear tradeoff in balancing capital constraints with direct and spillover impacts when designing Cash Transfer programs. This is a particularly relevant concern for the findings for the female-centric findings of Baird et al. (2011), since the larger CCT cash transfers appeared to benefit non-core females strongest in our sample.

Still, it is difficult to interpret the difference in outcomes for core females in Baird et al. (2011)—as well as our replication—and the lack of significance for non-core female Teenagers found by our research. As previously discussed, this could be the product of unobservable differences between these two groups of same age girls, biases in our sample, or a true unbiased representation of household responses to the treatment. The differences between the core and the non-core Teenage girls could be of educational aptitude, personal preferences or of external factors that prevented some of these girls from being part of the core group of the research. Similarly, our low instances of marriage across our non-core sample raises the possibility that the individuals in the non-core age range who marry in our sample might move households and therefore not be able to be successfully tracked. Since we find that marriage is a strong negative predictor in school enrollment, our data set might not include those who were most likely to leave school in the first place; in these terms, we might be making an unfair comparison between core females and our tracked non-core female Teenagers. However, if we make the relatively fair argument that transfer exposure does not result in the movement of school-aged individuals away from treated households, the insignificance of the results for our non-core Teenage group as a whole suggests we are not necessarily biasing our results upward in the effect of the program on this cohort. Similarly, we do not suspect marriage to be a large cause of attenuation in our Child aged cohort, and we therefore don't believe this strongly biases our results.

Conclusion

Changes in household education decisions as a response to income shocks reflect both the relaxation of financial constraints as well as household preferences for education. Considering the complexities in the formation of household education perceptions, poverty alleviation is a clear contender to improve educational attainment for future generations. As one of the most direct ways to increase household wealth, cash transfer programs provide a crucial insight of the impact of relaxing financial constraints on educational outcomes. By utilizing a four-year panel from a randomized bilateral (Conditional and Unconditional) cash transfer program in Malawi, we investigate the intra-household spillover effects on educational outcomes for school-aged individuals who were not a core individual in the main study design.

We find that young females (3-12 years old) were significantly more likely to be enrolled in school during the survey if they were in conditionally treated households. These results depended on the transfer amount to the house, and conditional transfer values in the lowest quartile had little effect on enrollment rates. We also find modest improvements in school enrollment for boys in these same age and quartile ranges of conditionally treated households. However, transfers had insignificant effects on conditionally treated households where the conditionally targeted core female was not in school pre-intervention, which suggests the presence of deeper unobserved factors influencing household education decisions. As for non-core individuals in the same age range as the core females (13-22 years old), we found no evidence for increased school enrollment likelihood due to conditional transfer exposure. Similarly, unconditional exposure did not saliently improve enrollment rates for non-core males or females in our entire sample (3-22 years old).

This analysis shows there can be significant spillover impacts from conditional transfer interventions to improve educational attainment with minimal negative effects. There also appears to be value in the conditional nature of these transfers, as they had the strongest effect for non-core females as well as modest effects on non-core males in the 3-12-year-old range. Distributing cash transfers with the pretense of requiring the targeted core female to go to school may encourage households to use these funds for the educational purposes even on for those whose education is not "required" for the funds. In these terms, conditional interventions may have the beneficial externality of improving household perceptions of education and could encourage a continued increase in intergenerational educational investment. This provides an additional argument for the implementation of conditional over unconditional interventions.

The central question of the ability for cash transfer programs to yield sustained improvements in educational and developmental outcomes post-intervention remains at stake. If the lasting impacts of the SIHR transfers on the core female group are any indication, cash transfer programs are far from a magic bullet intervention that ubiquitously improves educational outcomes (Baird et al., 2016). More research on the effectiveness of Cash Transfer interventions to reduce income inequality in a long-term framework remains essential to robust policy. However, our analysis demonstrates these interventions can help induce a crucial first step to increase school enrollment rates for young children, even when targeted at others in the household.

Appendix

Appendix A: Distribution of Transfer Values for Treated Households

Appendix Figure 1A: Value of Transfers to Household and Schoolgirls

Monthly HH Transfer Value per Core Female

Assigned Household Transfer Value per Core Female (Malawian Kwacha)	(1) N
560	245
840	276
1120	193
1400	288
1560 (UCT w/ Secondary Tuition)	21
1840 (UCT w/ Secondary Tuition)	43
2120 (UCT w/ Secondary Tuition)	41
2400 (UCT w/ Secondary Tuition)	21
Total	1128

Monthly Transfer Amounts to Each Core Female

Individual Transfer Value to Core Female (Malawian Kwacha)	(1) N
140	246
280	208
420	241
560	195
700	238
Total	1128

*Total Monthly HH Transfer Amounts (Round 2)***

Household Transfer Amount (Malawian Kwacha)	(1) N
560	213
840	236
1120	177
1400	221
1560	14
1680	20
1840	27
2120	35
2240	14
2400	13
2520	1
2680	3
2800	29
3120	1
3240	5
3680	5
3800	1
4200	2
4240	2
4520	1
4800	2
Total	1022

*****Round 2 and 3 Transfer amounts are extremely similar, so we show only Round 2 values for simplicity.***

Note: At the start of the program, the exchange rate between United States Dollars and Malawian Kwacha was 140 Kwacha per Dollar, resulting in the provided transfer amounts above. We also restrict the monthly household transfer amounts to houses that were successfully tracked across all rounds, so that only successfully paired transfer amounts are analyzed.

Appendix B: Asset Index Construction

Asset Index Construction Methodology

Consider an asset a_i within a household h such that a_i^h is an indicator variable denoting household ownership of the asset. Conducting a PCA using n asset indicator variables results in an nxn component matrix M, where column vector l represents principle component l. Consider also asset a_i's associated weight w_i, which comes from the ith entry in the transposed column vector from the first principle component of the PCA. The first principle component vector accounts for the largest share of explained variation in the assets. Then, we consider the asset index valuable for a household, A_h, as:

$$A_h = \sum_{i=1}^{n} w_i a_i^h$$

Where n denotes the number of assets utilized in the asset index. The underlying logic is that, although PCA construction responds purely to variation in data values, this variation is best explained by differences in household wealth. We construct this index utilizing information for all households in the baseline survey as a better representation of household wealth compared to all peer households. The results of our PCA and determined weights are shown in Figure 1A below. Note that higher values in the asset index correspond to a higher estimate of household wealth such that positive weights generally indicate the asset correlates with higher material wealth.

Appendix Table 1B: Asset Index Variables and Weights from First Principle Component

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max	(6) Weight in Asset Index
Mortar/pestle (mtondo)	3,432	0.106	0.308	0	1	0.0637
Bed	3,432	0.763	0.425	0	1	0.2381
Mattress	3,432	0.645	0.479	0	1	0.2578
Table (dining)	3,432	0.520	0.500	0	1	0.2131
Coffee table (for sitting room)	3,432	0.509	0.500	0	1	0.1867
Chair (un-upholstered)	3,432	0.421	0.494	0	1	0.1511
Upholstered chair or sofa	3,432	0.596	0.491	0	1	0.2588
Cupboard, drawers, wardrobe	3,432	0.285	0.451	0	1	0.2025
Lantern (paraffin)	3,432	0.269	0.444	0	1	0.1301
Clock	3,432	0.480	0.500	0	1	0.2408
Iron (for pressing clothes)	3,432	0.343	0.475	0	1	0.2111
Radio ('wireless')	3,432	0.454	0.498	0	1	0.0748
Tape or CD player; HiFi	3,432	0.577	0.494	0	1	0.1487
Television	3,432	0.303	0.460	0	1	0.2197
Landline phone	3,432	0.158	0.365	0	1	0.147
Mobile phone	3,432	0.0478	0.213	0	1	0.2125
Bicycle	3,432	0.279	0.449	0	1	0.094
Motorcycle	3,432	0.499	0.500	0	1	-0.011
Car	3,432	0.0297	0.170	0	1	0.0899
Beer brewing drum	3,432	0.0192	0.137	0	1	0.0506
Panga	3,432	0.0752	0.264	0	1	0.0953
Hoe	3,432	0.704	0.457	0	1	-0.0332
Axe	3,432	0.965	0.183	0	1	0.0987
Sickle	3,432	0.737	0.440	0	1	-0.0099
Solar panels	3,432	0.593	0.491	0	1	-0.0206
Owns place of residence	3,432	0.0117	0.107	0	1	-0.0857
Low quality roof in dwelling	3,432	0.866	0.341	0	1	-0.2766
Low quality floor in dwelling	3,432	0	0	0	0	-0.2838
High quality roof in dwelling	3,432	0.584	0.493	0	1	0.2766
High quality floor in dwelling	3,432	0.588	0.492	0	1	0.2838
Electricity in household	3,432	0.412	0.492	0	1	0.1948

Table 2B: Asset Index and Monthly Total Expenditure Summary Statistics

VARIABLES	(1) N	(2) Mean	(3) SD	(4) Min	(5) 25%	(6) 50%	(7) 75%	(8) Max
Asset Index	3027	1.298	1.216	-0.427	0.214	1.088	2.288	3.914
Total Expenditure (2008 Survey Round)	3026	9296.8	10417.34	0	3500	6000	20000	130000
Total Expenditure (2009 Survey Round)	3100	12969.7	14020.25	0	5150	8805	15000	230000
Total Expenditure (2010 Survey Round)	3234	12378.2	17219.93	0	4000	7000	14000	300000

Appendix C: Household Summary Statistics

Table 1C: Education Level of Head of Household

Head of Household Education	(1) Survey Round 1 (2008)	(2) Survey Round 2 (2009)	(3) Survey Round 3 (2010)
None	403 (13.2%)	400 (12.8%)	413 (12.8%)
Pre-School	4 (0.1%)	13 (0.4%)	3 (0.1%)
Primary	1884 (61.8%)	1845 (60.5%)	1742 (57.2%)
Secondary	621 (20.4%)	697 (22.9%)	906 (29.7%)
University	33 (1.1%)	33 (1.1%)	49 (1.6%)
Training School	78 (2.6%)	120 (3.9%)	106 (3.5%)
Don't Know/Missing	25 (0.8%)	16 (0.5%)	17 (0.6%)
Total	3048	3124	3236

Table 2C: Household Characteristics from Initial 2008 Survey

Mean Values of Baseline Variables for Each Treatment Group	(1) CCT Household (Non-Core Dropout Group)	(2) CCT Household (Core Dropout Group)	(3) UCT Household	(4) Spillover Household	(5) Control Household (Non-Core Dropout Group)	(6) Control Household (Core Dropout Group)
Asset Index	1.35	0.98	1.66	1.39	1.38	0.94
Natural Log of Total Expenditures	8.7	8.54	8.86	8.8	8.68	8.42
Household in Urban Setting	0.15	0.16	0.23	0.18	0.14	0.19
Number of Children in Household	4.3	4.45	4.46	4.25	4.37	4.45
Education Level of Head of Household	2.17	2.04	2.32	2.24	2.12	1.98
Total Households (3048)	*366*	*416*	*233*	*473*	*380*	*1180*

Note that H.o.H Education Level takes the value (1) if educational attainment is less than Primary, (2) if Primary level, (3) if Secondary level, and (4) if Post-Secondary level

Table 3C: Additional Summary Statistics for Sensitivity Variables

		(1)	(2)	(3)	(4)	(5)	(6)
		Survey Round 1 (2008)		Survey Round 2 (2009)		Survey Round 3 (2010)	
Sensitivity Variable Summary Stats	N	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Individual-Level Variables							
Child of Head of Household	4698	0.752	0.432	0.747	0.435	0.732	0.443
Mother In House, Father Not	4698	0.252	0.434	0.255	0.436	0.262	0.440
Father In House, Mother Not	4698	0.019	0.135	0.019	0.137	0.016	0.125
Both Parents in House	4698	0.567	0.496	0.547	0.498	0.534	0.499
Household-Level Variables							
Number of Children in Household	Varies	4.367	2.084	4.863	2.155	3.929	2.020
Head of Household is Female	Varies	0.34	0.474	0.374	0.484	0.303	0.46
Total Survey Households:3048		N = 3048		N = 3124		N = 3236	

Appendix D: Sensitivity and Full Transfer Regressions

Table 1D: Non-Core Sensitivity Regression

VARIABLES	(1) In School Treatment Exposure (All Rounds) (Full Non-Core Sample)	(2) In School Transfer Amounts (All Rounds) (Full Non-Core Sample)	(3) In School ((1) +Gender Controls) (All Rounds) (Full Non-Core Sample)
In Treatment HH	0.242***		0.424***
	(0.0910)		(0.107)
Marginal Effect of UCT HH	-0.192*		-0.303*
	(0.115)		(0.169)
Total HH Transfer Size Amount		0.00839***	
		(0.00299)	
Marginal Effect of UCT Transfer Size Amount		-0.00704**	
		(0.00340)	
Male			-0.126**
			(0.0588)
Marginal Effect of Male in Treatment HH			-0.284**
			(0.117)
Marginal Effect of Male in UCT HH			0.190
			(0.216)
Core Dropout Group	-0.531***	-0.539***	-0.524***
	(0.0757)	(0.0746)	(0.0749)
Marginal Effect of Treated Core Dropout HH	-0.0778	-0.00962	-0.0754
	(0.115)	(0.106)	(0.115)
Communal Spillover HH	0.0892	0.0546	0.0792
	(0.0730)	(0.0710)	(0.0714)
Asset Index	0.0888***	0.0848***	0.0879***
	(0.0269)	(0.0269)	(0.0269)
Education Level of H.o.H	0.0131	0.0153	0.0140
	(0.0481)	(0.0480)	(0.0481)
Married	-2.580***	-2.574***	-2.614***
	(0.219)	(0.219)	(0.220)
Number of Children in HH	0.00406	0.00431	0.00386
	(0.0120)	(0.0121)	(0.0120)
Female Head of Household	-0.0949	-0.101	-0.0960
	(0.0731)	(0.0734)	(0.0732)
Natural Mother in HH	0.0287	0.0305	0.0313
	(0.0782)	(0.0785)	(0.0783)
Natural Father in HH	0.0720	0.0716	0.0757
	(0.187)	(0.187)	(0.187)
Both Natural Parents in HH	0.0624	0.0651	0.0678
	(0.0780)	(0.0781)	(0.0781)
Child of H.o.H	0.120**	0.119*	0.121**
	(0.0612)	(0.0612)	(0.0612)
Urban	0.152*	0.152*	0.153*
	(0.0796)	(0.0796)	(0.0797)
Age	0.0587***	0.0584***	0.0619***
	(0.00521)	(0.00521)	(0.00532)
Survey Round 2	0.636***	0.645***	0.631***
	(0.0466)	(0.0432)	(0.0447)
Survey Round 3	0.649***	0.660***	0.641***
	(0.0470)	(0.0436)	(0.0453)
Constant	0.102	0.125	0.151
	(0.150)	(0.149)	(0.151)
Observations	13,971	13,971	13,971
Number of Individuals	4,657	4,657	4,657

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2D: Full Transfer Quartile Regression

VARIABLES	(1) CCT 3-12 Years Old at Baseline (All Rounds)	(2) UCT 3-12 Years Old at Baseline (All Rounds)	(3) CCT 13-22 Years Old at Baseline (All Rounds)	(4) UCT 13-22 Years Old at Baseline (All Rounds)
First Transfer Quartile	0.168	0.966**	-0.506	-1.266*
	(0.214)	(0.482)	(0.430)	(0.764)
Second Transfer Quartile	0.527**	-0.185	0.135	-1.065*
	(0.237)	(0.446)	(0.439)	(0.569)
Third Transfer Quartile	0.583**	-0.790***	-0.174	0.470
	(0.242)	(0.300)	(0.397)	(0.612)
Fourth Transfer Quartile	0.796***	0.492	0.156	0.563
	(0.310)	(0.343)	(0.499)	(0.515)
Male	-0.182***	-0.187***	0.247*	0.255*
	(0.0669)	(0.0714)	(0.131)	(0.135)
Male* First Transfer Quartile	-0.259	-0.0675	0.518	1.020
	(0.244)	(0.735)	(0.454)	(0.861)
Male* Second Transfer Quartile	-0.000411	1.582	0.458	1.909**
	(0.287)	(0.969)	(0.481)	(0.783)
Male* Third Transfer Quartile	-0.218	1.346**	0.301	-0.206
	(0.285)	(0.525)	(0.432)	(0.780)
Male* Fourth Transfer Quartile	-0.135	-0.591	-0.0893	-0.186
	(0.355)	(0.441)	(0.515)	(0.609)
Spillover HH	0.181**	0.145*	0.0327	0.0839
	(0.0862)	(0.0872)	(0.142)	(0.145)
Dropout Group	-0.556***	-0.604***	-0.729***	-0.771***
	(0.0856)	(0.104)	(0.153)	(0.180)
First Transfer Quartile*Dropout Group	-0.145		0.115	
	(0.247)		(0.389)	
Second Transfer Quartile*Dropout Group	-0.492*		-0.0448	
	(0.290)		(0.432)	
Third Transfer Quartile*Dropout Group	-0.0619		0.215	
	(0.288)		(0.425)	
Fourth Transfer Quartile*Dropout Group	-0.504		0.225	
	(0.357)		(0.409)	
Asset Index	0.228***	0.233***	0.188***	0.117**
	(0.0311)	(0.0349)	(0.0512)	(0.0543)
Head of Household Education	0.109**	0.111*	0.0826	0.146*
	(0.0508)	(0.0567)	(0.0747)	(0.0781)
Married	-1.466**	-1.608**	-2.137***	-2.073***
	(0.697)	(0.717)	(0.295)	(0.308)
Urban	0.268***	0.252**	0.126	0.130
	(0.101)	(0.112)	(0.150)	(0.155)
Age in 2008	0.526***	0.541***	-0.455***	-0.457***
	(0.0197)	(0.0226)	(0.0274)	(0.0296)
Survey Round 2	1.178***	1.180***	-0.161*	-0.0909
	(0.0693)	(0.0746)	(0.0915)	(0.0944)
Survey Round 3	1.979***	1.946***	-1.112***	-1.011***
	(0.0856)	(0.0928)	(0.0951)	(0.0980)
Constant	-3.823***	-3.896***	9.297***	9.153***
	(0.184)	(0.210)	(0.538)	(0.574)
Observations	8,089	6,594	4,647	3,922
Number of Individuals	2,703	2,211	1,557	1,313

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3D: P-Values for Transfer Quartiles on Non-Core Enrollment, By Gender and Age

	Child Group (3-12 Years Old in First Survey)		Teenage Group (13-22 Years Old in First Survey)	
P-Values for Transfer Quartile Significance	Female	Male	Female	Male
CCT				
CCT First Quartile	0.4340	0.6693	0.2380	0.9610
CCT Second Quartile	0.0261**	0.0559*	0.7560	0.0654*
CCT Third Quartile	0.0165**	0.1442	0.6590	0.6745
CCT Fourth Quartile	0.010***	0.0331**	0.7570	0.8045
Core Dropout Group Results				
CCT First Quartile	0.9207	0.2616	0.3978	0.6830
CCT Second Quartile	0.8882	0.8893	0.8697	0.1158
CCT Third Quartile	0.0501*	0.2164	0.8984	0.3579
CCT Fourth Quartile	0.3139	0.6370	0.4748	0.3638
UCT				
UCT First Quartile	0.042**	0.1006	0.0880*	0.5604
UCT Second Quartile	0.6960	0.1049	0.0630*	0.0989*
UCT Third Quartile	0.009***	0.1965	0.4580	0.5121
UCT Fourth Quartile	0.1550	0.7022	0.2620	0.3118

Significant P-Values suggest treatment exposure yielded higher/lower enrollment compared to untreated households

Resulting Test Statistics are Chi-Squared

*** p<0.01, ** p<0.05, * p<0.1

References

Akresh, R., Walque, D. De, & Kazianga, H. (2013). Cash transfers and child schooling: Evidence from a randomized evaluation of the role of conditionality. *World Bank Policy Research*, (January), 1–49. https://doi.org/10.1037/e547172013-001

Baird, S., McIntosh, C., & Özler, B. (2011). Cash or condition? Evidence from a cash transfer experiment. *Quarterly Journal of Economics*, *126*(4), 1709–1753. https://doi.org/10.1093/qje/qjr032

Baird, S., Mcintosh, C., & Özler, B. (2016). When the Money Runs Out: Do Cash Transfers Have Sustained Effects? *Development Workshop, Otago International Health Research Network*, (December), 1–2.

Bauer, P., & Riphahn, R. T. (2007). Heterogeneity in the intergenerational transmission of educational attainment: Evidence from Switzerland on natives and second-generation immigrants. *Journal of Population Economics*, *20*(1), 121–148. https://doi.org/10.1007/s00148-005-0056-5

Benhassine, N., Devoto, F., Duflo, E., Dupas, P., & Pouliquen, V. (2015). Turning A Shove into A Nudge? A "Labeled Cash Transfer" For Education. *American Economic Journal: Economic Policy*, *7*.

Booysen, F., van der Berg, S., Burger, R., Maltitz, M. von, & Rand, G. du. (2008). Using an Asset Index to Assess Trends in Poverty in Seven Sub-Saharan African Countries. *World Development*, *36*(6), 1113–1130. https://doi.org/10.1016/j.worlddev.2007.10.008

Child Trends (2015). Parental Expectations for Their Children's Academic Attainment.

Cook, L. D., & Jennings, Z. (2016). Perspectives of Jamaican parents and their secondary school children on the value of education: Effects of selected variables on parents' perspectives. *International Journal of Educational Development*, *50*, 90–99. https://doi.org/10.1016/j.ijedudev.2016.05.005

Daouli, J., Demoussis, M., & Giannakopoulos, N. (2010). Mothers, fathers and daughters: Intergenerational transmission of education in Greece. *Economics of Education Review*, *29*(1), 83–93. https://doi.org/10.1016/j.econedurev.2009.02.006

Davis-Kean, P. E. (2005). The influence of parent education and family income on child achievement: The indirect role of parental expectations and the home environment. *Journal of Family Psychology*, *19*(2), 294–304. https://doi.org/10.1037/0893-3200.19.2.294

Delaney, L., Harmon, C., & Redmond, C. (2011). Parental Education, Grade Attainment and Earnings Expectations Among University Students. *Economics of Education Review*, *30*(6), 1136–1152. https://doi.org/10.1016/j.econedurev.2011.04.004

Dubow, E. F.; Boxer, P.; Huesmann, L. R. (2009). Long-term Effects of Parents' Education on Children ' s Educational and Occupational Success Mediation by Family Interactions , Child Aggression ,. *Merrill-Palmer Quarterly*, *55*(3), 224–249. https://doi.org/10.1353/mpq.0.0030.Long-term

Ferreira, F. H. G., Özler, B., & Woolcock, M. (2014). Conditional, unconditional and everything in between: a systematic review of the effects of cash transfer programmes on schooling outcomes AU - Baird, Sarah. *Journal of Development Effectiveness*, *6*(1), 1–43. https://doi.org/10.1080/19439342.2014.890362

Filmer, D., & Pritchett, L. (2011). Estimating Wealth Effects without Expenditure Data-or Tears: An Application to Educational Enrollments in States of India Author (s): Deon Filmer and

Lant H . Pritchett Published by: Springer on behalf of the Population Association of America Stable U. *Demography*, *38*(1), 115–132. https://doi.org/10.1080/00141844.2012.679288
Filmer, D., & Schady, N. (2011). Does More Cash in Conditional Cash Transfer Programs Always Lead to Larger Impacts on School Attendance? *Journal of Development Economics*, *96*(1), 150–157. https://doi.org/10.1016/j.jdeveco.2010.05.006
García, S., & Saavedra, J. E. (2017). Educational Impacts and Cost-Effectiveness of Conditional Cash Transfer Programs in Developing Countries: A Meta-Analysis. *Review of Educational Research*, *87*(5), 921–965. https://doi.org/10.3102/0034654317723008
Glewwe, P., & Kassouf, A. L. (2012). The Impact of the Bolsa Escola/Familia Conditional Cash Transfer program on Enrollment, Dropout rates and Grade Promotion in Brazil. *Journal of Development Economics*, *97*(2), 505–517. https://doi.org/10.1016/j.jdeveco.2011.05.008
Glick, P., & Sahn, D. E. (1999). Schooling of girls and boys in a West African country: The effects of parental education, income, and household structure. *Economics of Education Review*, *19*(1), 63–87. https://doi.org/10.1016/S0272-7757(99)00029-1
Haushofer, J., & Shapiro, J. (2016). The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimiental Evidence from Kenya. *The Quarterly Journal of Economics*, *131*(4), 1973–2042. https://doi.org/10.1093/qje/qjw025
Haveman, Robert, & Wolfe, B. (1995). The Determinants of Children's Attainments: Findings and Review of Methods. *Journal of Economic Literature*, *33*(December), 1829–1878. https://doi.org/10.2307/2729315
Hossler, D., & Stage, F. K. (1992). Family and High School Experience Influences on the Postsecondary Educational Plans of Ninth-Grade Students. *American Educational Research Journal*, *29*(2), 425–451. https://doi.org/10.3102/00028312029002425
Hulme, D., Hanlon, J., & Barrientos, A. (2010). Just Give Money to the Poor: The Development Revolution from the Global South. Retrieved from https://ebookcentral.proquest.com
Jensen, R. (2010). The (Perceived) Returns to Education and the Demand for Schooling. *The Quarterly Journal of Economics*, (May).
Kilburn, K., Handa, S., Angeles, G., Mvula, P., & Tsoka, M. (2017). Short-Term Impacts of an Unconditional Cash Transfer Program on Child Schooling: Experimental Evidence from Malawi. *Economics of Education Review*, *59*, 63–80. https://doi.org/10.1016/j.econedurev.2017.06.002
Maertens, A. (2011). Does Education Pay Off? Subjective Expectations on Education in Rural India. *Economic and Political Weekly*, *xlvi*(9), 58–63.
McEwen, A., & Stewart, J. M. (2014). The Relationship between Income and Children's Outcomes : A Synthesis of Canadian Evidence The Relationship between Income and Children ' s Outcomes : A Synthesis of Canadian Evidence. *Canadian Public Policy / Analyse de Politiques*, *40*(1), 99–109. https://doi.org/10.3138/cpp.40.1-SYN
Menon, M. E. (2008). Perceived Rates of Return to Higher Education: Further Evidence from Cyprus. *Economics of Education Review*, *27*(1), 39–47. https://doi.org/10.1016/j.econedurev.2006.07.009
Oketch, M., Mutisya, M., & Sagwe, J. (2012). Parental aspirations for their children's educational attainment and the realisation of universal primary education (UPE) in Kenya: Evidence from slum and non-slum residences. *International Journal of Educational Development*, *32*(6), 764–772. https://doi.org/10.1016/j.ijedudev.2011.04.002

Psacharopoulos, G., & Patrinos, H. A. (2018). Returns to investment in education: a decennial review of the global literature. *Education Economics*, *26*(5), 445–458. https://doi.org/10.1080/09645292.2018.1484426

Sabates, R., Hossain, A., & Lewin, K. M. (2013). School drop out in Bangladesh: Insights using panel data. *International Journal of Educational Development*, *33*(3), 225–232. https://doi.org/10.1016/j.ijedudev.2012.09.007

Schoon, I., Martin, P., & Ross, A. (2007). Career transitions in times of social change. His and her story. *Journal of Vocational Behavior*, *70*(1), 78–96. https://doi.org/10.1016/j.jvb.2006.04.009

Sirin, S. R. (2005). Socioeconomic Status and Academic Achievement: A Meta-Analytic Review of Research. *Review of Educational Research*, *75*(3), 417–453. https://doi.org/10.3102/00346543075003417

Spera, C., Wentzel, K. R., & Matto, H. C. (2009). Parental aspirations for their children's educational attainment: Relations to ethnicity, parental education, children's academic performance, and parental perceptions of school climate. *Journal of Youth and Adolescence*, *38*(8), 1140–1152. https://doi.org/10.1007/s10964-008-9314-7

Stage, F. K., & Hossler, D. (1989). Differences in Family Influences on College Attendance Plans for Male and Female Ninth Graders. *Research in Higher Education*, *30*(3).

Vyas, S., & Kumaranayake, L. (2006). Constructing socio-economic status indices: How to use principal components analysis. *Health Policy and Planning*, *21*(6), 459–468. https://doi.org/10.1093/heapol/czl029

World Bank. (2010). The Education System in Malawi, (182).

Weinberg, B. A. (2001). An Incentive Model of the Effect of Parental Income on Children. *Journal of Political Economy*, *109*(2), 266–280. https://doi.org/10.1086/319556

Zhang, Y., Kao, G., & Hannum, E. (2007). Do Mothers in Rural China Practice Gender Equality in Educational Aspirations for Their Children? *Comparative Education Review*, *51*(2), 131–157. https://doi.org/10.1086/512023