UNITED NATIONS
UNIVERSITY
UNU-WIDER

WIDER Development Conference

Public economics for development

5-6 July 2017 | Maputo, Mozambique

Short-term Impacts of an Unconditional Cash Transfer Program on Child Schooling: Experimental Evidence from Malawi

*Kelly Kilburn[a1], Sudhanshu Handa[a], Gustavo Angeles[b], Peter Mvula[c], Maxton Tsoka[c]

[a] Department of Public Policy, University of North Carolina-Chapel Hill
Abernethy Hall
131 South Columbia Street
Chapel Hill, NC, USA 27599-3435

[b] Department of Maternal and Child Health, University of North Carolina-Chapel Hill
401 Rosenau Hall
Gillings School of Global Public Health
Chapel Hill, NC 27599-7445

[c] Centre for Social Research, University of Malawi
Political and Administrative Studies
P.O Box 278, Zomba, Malawi

*Kelly Kilburn is the corresponding author (kkilburn@unc.edu, +1.919.961.0245)
Sudhanshu Handa: shanda@email.unc.edu
Gustavo Angeles: gustavo_angeles@unc.edu
Peter Mvula: petermvula58@yahoo.com
Maxton Tsoka: tsokamax@gmail.com

Abstract
This study analyzes the impact of a positive income shock on child schooling outcomes using experimental data from an unconditional cash transfer program in Malawi. Since households receive the cash and parents are responsible for making spending decisions, we also examine the intervening pathways between cash transfers and child schooling. Data comes from a cluster-randomized study of Malawi's Social Cash Transfer Program (SCTP). After a baseline survey, households in village clusters were randomly assigned to treatment and control arms with treatment villages receiving transfers immediately and control villages assigned a later entry. We test for treatment impacts on a panel of school-aged children (6-17) using a differences-in-differences model. After a years' worth of transfers, we find the Malawi SCTP both improves enrollment rates and decreases dropouts. The main intervening pathway between the program and schooling is education expenditures, suggesting that the cash improves the demand for education by reducing financial constraints.

Keywords: demand for schooling, cash transfers, economic development, sub-Saharan Africa

JEL codes: I25, I38, O15, O12

Abbreviations:
SSA: Sub-Saharan Africa
SCTP: Social Cash Transfer Program

[1] Present address: Institute for Global Health and Infectious Diseases, UNC-Chapel Hill, Bioinformatics Building, 130 Mason Farm Rd, Chapel Hill, NC 27599-7030

1. Introduction

Over the past few decades, expanding and improving education has been a major agenda item for governments in low and middle-income countries and development organizations since education is a key factor in both individual well-being (Behrman, 2010; Psacharopoulos & Patrinos, 2004) and societal progress (World Bank, 2011). In particular, children's access and completion of primary education was named a top goal by the United Nations' (UN) Millennium Development Goals (MDGs), introduced in 2000 as a blueprint for the world's development agenda. Since the establishment of the MDGs, there has been considerable progress made in school enrollment rates across the developing world. Most children in low and middle-income countries now complete primary school and many also go on to obtain at least some secondary-level education (Glewwe & Muralidharan, 2015).

Nevertheless, sub-Saharan Africa (SSA), still lags behind other developing regions of the world. Indeed, the majority of the world's 124 million children that are not in school live in SSA. In 2013, half of the nearly 30 million primary school-age children out of school in the region had never been enrolled, and of these children, girls make up the disproportionate share (UIS & EFA, 2015). One of the biggest barriers to access is armed conflict with a third of out-of-school children living in conflict-affected countries (UNESCO, 2015). In addition to armed conflict, other regional challenges include high poverty and rapid population growth. Malawi, the location of this study, is one of the poorest, most rural countries in SSA. Poverty is the primary barrier to education for children in Malawi, limiting both supply and demand. The out of school rates for the poorest children (bottom quintile) in Malawi are 18 percent for primary school-ages (6-13) and 38 percent for secondary school-ages (14-17) (compared to 3% and 18% respectively for the top quintile) (EPDC, 2014).

Despite all the concurrent challenges in the region, there have been notable improvements in primary schooling in SSA following the MDGs including a 20 percent increase in the adjusted net primary school enrollments from 1999 to 2012 (UNESCO, 2015). Although this indicator signifies important progress, it does not tell the whole story. School incompletion rates are persistently high in

SSA—over 30 percent of children that enroll in primary school are not expected to finish. Additionally, there are large grade-for-age discrepancies due to children entering school late, repeating grades, or dropping in and out of school (UNESCO, 2015). This situation results in gross primary school enrollment rates (ratio of the number of children enrolled to the number of primary school-age children) that are often over 100 percent—Malawi's rate is 141 percent (EPDC, 2014). Correspondingly, net secondary school enrollment rates are quite low, 33 percent overall in Malawi and 17 percent in the poorest quintile (UNICEF Global Database, 2016).

The UN's Sustainable Development Goals (SDGs), introduced in 2015 (replacing the MDGs), now have a more ambitious goal of universal completion of primary and secondary school. Unlike universal access, which can be attained with heavy supply-side investments, universal completion goals may require interventions to ensure household demand for education (Bruns, Mingat, & Rakotomalala, 2003). In Malawi, this household demand is unlikely to be met without reducing cost barriers (both direct and indirect) parents face in sending their children to school (Glewwe & Kassouf, 2012). Even though Malawi provides free primary education, other obligatory expenses like uniforms and school supplies can make primary school too expensive for some families. Furthermore, even if children complete primary school, secondary education is usually cost prohibitive for poor families because of added costs from tuition and occasionally travel or board since schools tend to be far from rural areas (Baird, Ferreira, Ozler, & Woolcock, 2013b). Policy makers have therefore been concerned with finding ways to reduce out-of-pocket costs to increase household demand for education. Interventions that provide direct income support may increase household demand for schooling and lead to greater parental investment in their children if schooling is a normal good (Fiszbein, & Schady, 2009).

This paper analyzes the effect of a large, government-run unconditional cash transfer program on child schooling in rural Malawi. The program, targeted to ultra-poor, labor-constrained households, is primarily a poverty-alleviation intervention and distributes regular cash payments to eligible households. Transfers comprise a significant share (almost 20 percent) of pre-program per capita consumption for the

average household. We examine whether and how the cash may help to increase demand for schooling. In comparison to conditional programs that often require households to send their children to school, unconditional programs distribute payments regardless of behavior, and parents, who have the responsibility of making household spending decisions, may or may not prioritize children's schooling. Indeed, an important argument for justifying cash transfers conditioned on school enrollment and minimum attendance requirements is precisely because policy-makers feel that parents may under-invest in children's human capital, concentrating, for example, on short-term needs rather than longer-term benefits accruing far in the future.

Evidence has demonstrated the ability of both conditional and unconditional programs to improve schooling outcomes rates in the developing world (for examples see reviews: Fiszbein & Schady; Baird et al., 2013b), however, little is known about the mechanisms through which how unconditional programs like Malawi's work to impact child schooling. Therefore, this study helps address the gap in knowledge by investigating how unconditional cash transfer programs given to the household impact child schooling outcomes. We use causal mediation methods to examine several potential mechanisms through which the cash could work to support schooling including parental well-being and spending behavior.

Households for this study were randomly assigned to either the treatment or control group after an initial baseline survey, and a second round of data collection was conducted on these same households after approximately 12 months of payments to treatment households. This strong research design allows us to examine the casual impact of the program on schooling outcomes for children 6-17 years of age and to interpret whether examined mechanisms explain observed impacts. Our findings indicate that the Malawi SCT program has strong, short-term impacts on schooling, increasing enrollments and decreasing dropouts after about one year's worth of transfers. The key mechanism for this effect is through increase spending on education, particularly uniforms and supplies. These results are confirmed by in-depth interviews with caregivers who describe how the cash enables them to meet out-of-pocket schooling expenses.

2. Background

2.1 Schooling Interventions and Cash Transfers

Existing evidence on schooling policies in the developing world is primarily focused on the impacts of traditional supply-side interventions such as the allocation of buildings, teachers, or learning materials. Less is known about demand-side interventions that focus on reducing costs and other barriers to children's educational access and attainment. Using the traditional model of parental investment in children's human capital, a household's decision to invest in an additional year of schooling for their child occurs when the expected benefits exceed the costs with respect to the present discounted value (Becker, 1962; Ben-Porath, 1967). Policies that attempt to increase schooling attainment through enrollment or attendance target this household decision either by increasing the immediate benefits or reducing the costs of sending the child to school.

Lately, there has been increasing attention to the growth and efficacy of these demand-side interventions including those that offer direct support (e.g. scholarship programs or the elimination of school fees) and indirect programs (e.g. increasing maternal literacy or subsidizing transportation)[2]. In a recent review of demand-side interventions in developing countries, Glewwe and Muralidharan (2015) find that these interventions have been generally effective at increasing enrollments and learning outcomes but are disparately cost-effective. Cash transfer programs, in particular, stand out for their cost-effectiveness and widespread use in developing countries (Glewwe & Muralidharan, 2015). These programs, whether conditional or unconditional, distribute cash typically to the poorest households to help alleviate poverty but also often aim to increase child schooling attainment and human capital through increasing service utilization. Conditional cash transfer programs (CCTs), such as those found in Latin America (e.g., Mexico's *Oportunidades* or Brazil's *Bolsa Familia*), do this by conditioning cash receipt

[2] In the United States, one widely discussed demand-side intervention is a school voucher, which enables school choice. In countries like Malawi where overall enrollment is low, and where the supply of secondary schools is low, the key policy objective is moving children from out-of-school to school, rather than facilitating choice among those that are already in school.

on households enrolling their children in school. Unconditional programs (UCTs), typically found in SSA, distribute cash payments regardless of recipient behavior (Baird et al., 2013b).

The guiding rationale for UCTs is that poor people are rational economic actors but merely lack the resources (money) to realize preferred investment levels. In other words, they possess the knowledge to make the wisest spending decisions that would improve their livelihoods, but they cannot do so because of financial constraints (Hanlon, Barrientos, & Hulme, 2010). By providing additional income in the form of cash transfers, households' credit constraints are freed up, allowing them invest in things like education. Those arguing in favor of attaching conditions to transfer programs, however, do not take households' economic rationality at face value. Instead, market failures such as incomplete information and high discount rates reduce decision-makers' ability to make the best investment choices, leading to privately sub-optimal education levels (Fiszbein & Schady, 2009). Schooling conditions are therefore designed to increase schooling levels by 'nudging' people to make decisions that better align with their own self-interest (Hanlon, Barrientos, & Hulme, 2010). Moreover, CCTs are defended for boosting education to socially optimal levels since additional positive externalities from an educated populace are not factored into private decision-making (Fiszbein & Schady, 2009; Psacharopoulos & Patrinos, 2004).

Separate from theoretical arguments, an important reason for UCTs in SSA is that there are significant barriers to attaching conditions for both targeted populations and implementers. Schooling systems, for one, are stressed (i.e., low supply and quality) in poor countries such as Malawi (World Bank, 2010). A low supply of schools means they are often out-of-the-way for households, and even if not, low quality means that the opportunity cost of sending children to school may be too high given the returns. This is especially true for the neediest families that would benefit most from the extra income. Moreover, these same governments often lack the capacity and resources to enforce conditions, making UCTs more practical and cost-effective than other interventions (Schubert & Slater, 2006).

According to a recent review, both CCTs and UCTs have increased education enrollment rates in large part because they reduce the financial constraints of schooling (Baird et al., 2013b), but given the

differences between these programs, it is unclear if they work in the same ways. The authors' proposed theory of change emphasizes how CCTs have two available mechanisms to affect household demand for schooling—the cash has an 'income effect' that helps alleviate credit constraints and the conditionality produces a 'substitution effect' that lowers the opportunity cost of schooling. UCTs solely work through the income effect. The review compares treatment effects and costs of interventions but does not attempt to empirically model the specific channels through which this income effect works in UCTs.

Our study contributes to this line of literature by empirically testing for mechanisms that explain schooling outcomes with data collected on parental well-being and spending decisions. Modeling the relationship between UCTs and child outcomes is of interest both in the academic community to reveal the mechanisms underlying behavior change and for policy-makers to design more effective and complementary interventions (Keele, Tingly, & Yamamoto, 2015). While several studies have examined pathways through which UCTs work to affect other outcomes including early pregnancy (Handa et al., 2015), adolescent mental health (Baird, McIntosh & Özler, 2013a; Kilburn, Thirumurthy, Halpern, Pettifor, & Handa, 2016), and infant/toddler child development (Fernald & Hidrobo, 2011), there is a need for evidence linking UCTs to child schooling. By explicating these pathways from income increases to schooling, this paper contributes to the evidence base surrounding unconditional cash programs and how they mitigate the consequences of poverty for children.

2.2 Mechanisms

Since unconditional cash transfer programs provide an income supplement and let households decide how to spend the money, they should only indirectly affect child well-being. The impact on child schooling thus depends upon the household response to the income, which makes it important to understand the internal allocation of resources within households (Barrientos & DeJong, 2006). The assumption is that the income affects children initially through increased household consumption resulting in a greater standard of living for the whole household, but in time, households may also reallocate resources leading to increased child investment. Only a few studies have examined how cash

transfers indirectly work through parental decision-making to impact child outcomes. In one experimental study of Ecuador's conditional cash program, *Atención a Crisis*, authors find improvements in young children's cognitive development are associated with increased parental investment behaviors that extend beyond the direct cash effect (Macours, Schady, & Vakis, 2012).

The literature on child development has offered a number of parental behavior channels through which income may work to influence child schooling and human capital accumulation. The most traditional pathway, parental investment, highlights the economic component of income and argues that family income affects child development through its impact on parental decisions to allocate resources such as money and time (Becker & Tomes, 1994). Poverty limits parent's ability to provide these resources meaning poor children have less exposure to materials and experiences that could benefit their development (Mayer, 1997; Haveman & Wolfe, 1995). Income from Malawi's cash transfer would then primarily have an economic effect that enables children to stay in school and build their human capital.

More recent research adds to this model by differentiating how human capital formation is a dynamic process and parental investments in earlier developmental time periods can be amplified over time as they interact with schooling inputs (Cunha, Heckman & Schennach, 2010; Cunha & Heckman, 2009; Heckman, 2006). The two act as 'dynamic complementarities' whereby parental investments of time and resources help develop child cognitive and non-cognitive skills, allowing for enhanced proficiency and performance in school, which in turn leads to greater efficiency in subsequent skill attainment. Investments in later periods would therefore have larger returns for the more skilled, higher-ability children. In settings like Malawi where education is non-compulsory, economic efficiency suggests that parents might allocate more resources to higher ability children so that they can stay in school rather than leave to start working because the family can expect a relatively higher return to education. It is possible then that income from the cash transfer may have differential impacts even for children in the same household.

Another pathway, the family stress model, focuses on the role of low income and other economic hardships to inhibit child development through their effect on parental stress and emotional instability (Conger & Elder, 1994). Evidence shows that these states can lead to destructive consequences for children because they are associated with weakened relationships and harsher parenting behaviors **(**Guo & Harris, 2000; Yeung, Linver, & Brooks–Gunn, 2002; Gershoff, Aber, Raver, & Lennon, 2007). Income from a cash transfer could work through this pathway by decreasing financial stress and improving parental psychological well-being, which in turn improves familial relationships and parental support of their children's education.

Additionally, there may be other indirect mechanisms that could help explain cash transfer impacts on child schooling. Communities in rural SSA tend to be small and well connected such that other households are often aware of the beneficiaries. Consequently, shaming could be a factor involved in the cash transfer effect on schooling if community members observe household behaviors and think that households are not using the money appropriately. In a similar manner, households may initially believe (or be pressured from the community into believing) that there are actually rules attached to cash transfer receipt like enrolling their children in school (Bastagli et al., 2016).

Moreover, we may observe differential impacts on schooling due to a range of contextual factors such as characteristics of children and families or components of the program. Some evidence has shown that programs can have different schooling effects by gender (Bastagli et al., 2016). In South Africa, for instance, unconditional cash payments from the Old Age Pension had the largest impact on enrollment for girls and female-headed households were associated with higher enrollment rates (Duflo, 2003). It is also common to see different impacts by age. Older children are less likely to be enrolled in part because free schooling typically only applies to primary school. Additionally, time spent in school (or on school work at home) decreases the availability of children to work and older children have a higher opportunity cost because they are more productive workers either in or out of the household. Moreover, for women, this opportunity cost includes marriage and child rearing (Glewwe & Kassouf, 2012). Another important

factor may include baseline enrollment rates since impacts tend to be strongest among households least likely to use education resources before receiving the transfer. Indeed, some programs with larger schooling effects come from populations that have lower baseline enrollments (Fiszbein & Schady, 2009), but the review by Baird and coauthors (2013b) does not find that an analogous measure—mean follow-up enrollment rates of the control group—explains variation across programs. Lastly, the extent of household poverty and the size of cash supplement may also be important since the cash must be enough for a household to meet its immediate consumption needs before it can free up resources for further investments. The size of the cash transfer relative to baseline consumption is generally an important indicator for program success. Transfer amounts that comprise around 20 percent of pre-program household consumption have resulted in larger program impacts across household measures in SSA (Handa & Davis, 2015). In Latin America, Nicaragua's CCT, Red de Protección Social, had both the largest transfer share (27 percent of per capita expenditure) and the largest impact on enrollments (Maluccio & Flores, 2005). Overall though, there is limited evidence for a significant relationship between schooling impacts and transfer size for either CCTs or UCTs (Fiszbein & Schady, 2009; Baird et al, 2013b). Furthermore, Baird and coauthors' (2013b) review find that no other programmatic design element except for the strength of conditionality explains the variance in schooling effects across programs.

2.3 The Malawi SCT Program

The Government of Malawi's (GoM's) Social Cash Transfer Program (SCTP) is an unconditional cash transfer program that aims to alleviate household hunger and poverty and also improve children's well-being and human capital. The program is targeted to ultra-poor, labor constrained households. Ultra-poor households have trouble meeting their most basic needs for both food and non-food essentials. Labor constrained households have a large dependency ratio, meaning that there are fewer wage earners or able-bodied workers to dependent members including the young, the elderly, and the disabled. These targeted beneficiaries are selected through a community-based approach with oversight provided by local and

national government. If they meet these two targeting conditions, they are automatically enrolled in the program and thus take up is effectively universal.

The Malawi SCTP began in 2006 as a pilot program in Mchinji and an early evaluation confirms that beneficiaries are both extremely poor and vulnerable even compared to other poor households (Miller, Tsoka, & Reichert, 2010). Additionally, households have higher dependency ratios with few able-bodied household members. They are particularly missing prime-age adults, which is thought to be due to high prime-age adult mortality in contexts (such as Malawi) where there is generalized prevalence of HIV (Handa et al., 2013).

The SCTP provides a monthly unconditional cash transfer to eligible households, which varies according to the demographic composition of the household. Table 1 shows transfer amounts in Malawi Kwacha (MWK) that were in use at time of follow-up data collection (transfer levels increased in May 2015). According to policy experts, the size of the transfer should amount to at least 20 percent of baseline consumption in order to have measurable impacts (Handa & Davis, 2015). During the time period covered by this study, the majority of households' transfer was below this 20 percent share (an average of 18 percent). Beneficiaries in our study received transfers that accounted for two months of payments at each collection. Over half of households reported receiving transfers between 6,000 to 10,000 MWK with most of the remaining households receiving lower payments (Malawi SCTP Evaluation Team, 2015).

Table 1. Structure and level of transfers (MWK)

	Prior to May 2015
1 Member	1,000
2 Members	1,500
3 Members	1,950
4+ Members	2,400
Each member under age 21	300
Each member between ages 21-30	600

Source: Malawi Social Cash Transfer Program Midline Impact Evaluation Report (2015)

The payment size depends upon the total number of household members and the number of members of applicable school ages (regardless of enrollment status of the child) according to household composition at baseline. As shown in Table 1, a household receives a 'top-up' of 300 MWK for a child

under age 21 and 600 MWK for household members ages 21-30. Top-ups are meant to assist with expenses for schooling and so amounts are lower for younger children since school fees are only required for secondary and technical/ vocational schools. At baseline, approximately 73 percent of primary school-age children (ages 6-13) in our sample were attending primary school and 68 percent of secondary school age children (ages 14-17). Although primary school starts at age 6 and standard grade-for-age progression would have children transitioning to secondary school at age 14, only 3 percent of adolescents ages 14-17 that were enrolled in school were attending secondary school at baseline. In the rural, poor Malawi context, this finding makes sense as children are starting school starting late, repeating grades, dropping in and out of school, and waiting a few years after primary to go back to secondary school. Therefore, targeting the larger top-up for children over 21 is sensible in rural areas where a typical schooling trajectory for a child that goes beyond primary school could be finishing primary at age 13 (or 15-16 after starting late and/or repeating grade), finishing lower secondary school (2 years), leaving school to work for a while, then attending technical/vocational school.

Among study participants, average baseline education expenditures such as tuition, fees, school books, uniforms, etc., from the previous school year are 705 MWK for each enrolled child 6-17 years of age. Therefore, 300 MWK top-ups would hypothetically be enough to cover the education expenses for each child after a couple months of transfers. While parents are not required to use the money for education purposes, by providing these small payment additions, the Malawi SCTP aims to encourage school attendance by reducing families' financial barriers. Similar to a study in Morocco that found that an unconditional but 'labelled cash transfer' (Benhassine, Devoto, Duflo, Dupas, & Pouliquen, 2015) was successful in improving schooling outcomes, this addition might induce parents to send their child to school if households believe that the money is intended to be used for educational purposes. To the extent that this aspect of the Malawi program serves as a 'nudge' for parents, this could help explain why the cash transfer could improve the demand for schooling.

3. Data

3.1 Study Design

We use data collected from an impact evaluation of Malawi's SCTP that includes both quantitative and qualitative components and was designed by UNC-Chapel Hill's Carolina Population Center and University of Malawi's Center for Social Research. The quantitative data comes from a household survey, a comprehensive instrument covering household composition, consumption, economic activity, education, and health, among others. The qualitative component includes in-depth individual interviews with the caregiver and one youth from 16 treatment households selected using a stratified sampling approach. IRB approval from was obtained from both the University of North Carolina (IRB Study No. 14-1933) and Malawi's National Commission for Science and Technology (IRB Study No. RTT/2/20).

Randomization: The impact evaluation consists of a cluster-randomized longitudinal study with a baseline and two follow-up surveys. The study was designed around the GoM's plans to extend and expand coverage of the SCTP within Malawi over three years starting in 2013. In order to integrate the impact evaluation with these expansion plans, two districts were chosen for this study, Salima and Mangochi. After establishing the study districts, random selection was carried out at two smaller levels within these districts, Traditional Authorities (TAs) and Village Clusters (VCs). In the first stage, four TAs (two in each district) were randomly selected to participate in the evaluation study and then eligible beneficiary lists were generated for all VCs within these four TAs. In the second stage, in each TA beneficiary lists were randomly ordered and then half were randomly selected to enter the program immediately with the remaining VCs to enter the program a later date. In the end, 29 VCs were selected for inclusion in the study with 14 assigned to treatment and 15 to the control arm.

Sampling and power: The study team computed power for the three key program outcomes of consumption, school enrollment and child nutritional status using intra-class correlation estimates from the most recent Malawi Demographic and Health Survey for nutrition, and the latest Malawi Integrated Household Survey for consumption and schooling. These calculations led to a sample size of 3,500

households in 29 VCs for an average of 121 households per cluster. Eligible households in each VC were randomly sorted and the first 122 households were selected for inclusion in the study. The final sample for the study was 3,531 households, approximately 47 percent of all eligible households from the four TAs.

The quantitative baseline survey was administered over several months from June to September 2013. Households were not assigned to treatment (T) and control (C) status until after the baseline survey in order to maintain objectivity during data collection. Half of the VCs in each TA were randomly assigned the treatment arm (1,678 households) to start receiving the cash transfer right away. The other half (1,853 households) was assigned to the delayed-entry control group and entered the program in late 2015. This cluster randomization approach is preferable to household randomization in this study because it reduces concerns that treatment effects could become contaminated due to households living in close proximity with other study participants (Malawi SCTP Evaluation Team, 2013). The design is also more administratively and ethically feasible because the program did not have the financial resources to reach all households immediately.

For our purposes, we use quantitative data from the baseline and the first follow-up household surveys. At follow-up, beneficiary households had received five or six cash payments. Each payment accounted for two months so results can be interpreted as one-year impacts of the program (Malawi SCTP Evaluation Team, 2015).

3.2 Attrition and Baseline Balance

To confirm that randomization was successful in creating balance between the study arms at baseline, we tested for statistical differences in means between the two treatment arms using OLS regression with standard errors adjusted for clustering at the VC level. We find evidence for successful randomization, mean household characteristics measures are all balanced between the treatment and control groups (no significant differences at the 10 percent level, see Appendix Table 1).

From the 3,531 households interviewed at baseline, 3,365 households (1,605 treatment and 1,760 control) were interviewed at follow-up. The follow-up occurred at the end of 2014 and concluded in February 2015. Overall attrition was low; 95 percent of the baseline sample was retained and detailed attrition analysis in the Impact Evaluation Midline Report finds no evidence of differential attrition (Malawi SCTP Evaluation Team, 2015). The evaluation examined 162 individual and household measures for statistical differences between remaining T and C households and find less than one percent are different at the 5 percent significance level. We also find no evidence of differential attrition from a smaller attrition analysis (Appendix Table 2) for the subset of key program indicators in Table 2 and all variables used in this analysis.

The main unit of analysis for this study is the individual child. Our study population includes all children of primary and secondary school age at baseline (between 6 and 17) with enrollment data from the panel of 3,365 households. The resulting sample size is 12,771 (6,324 children at baseline and 6,447 children at follow-up).

3.3 Measures

3.3.1 Education

Schooling outcomes[3] are defined for primary and secondary school aged children (ages 6 to 17) and include: school enrollment, temporary withdrawal, and dropout. School enrollment is defined as whether the child was enrolled in the current school year (2013-2014 at baseline and 2014-2015 at follow-up). Temporary withdrawal is an indicator for whether an enrolled child left school for two weeks or more during the current school year. Dropout is defined for children who were enrolled in the previous school year but not in the current school year. All measures are self-reported by the household.

[3] All survey items are available at the project website: https://transfer.cpc.unc.edu

3.3.2 Mechanisms

Parental stress is measured using the four-item shortened version of the Perceived Stress Scale (Cohen, Kamarack, & Mermelstein, 1983; Cohen & Williamson, 1988). The Perceived Stress Scale (PSS) is the most widely used psychological instrument for measuring the perception of stress. The scale intends to measure the degree to which situations in one's life are considered stressful. The PSS asks respondents to rate how often over the past month they had certain feelings that tap into how uncontrollable and overloaded respondents find their lives. The child's caregiver most often answers these subjective questions, but when this individual is not the caregiver, it is assumed that they are involved in making household decisions that impact these children. Responses are given on a one to five Likert scale from whether they "never" or "always" feel that way. Items are summed to develop a scale with a range of 4-20. In the follow-up survey, the full 10-item PSS scale was included. To test the 4-item scale for robustness, we compare the scores for the control group across these two scales. The alpha score of the shortened PSS is 0.63 across both rounds and the full scale is 0.74, the correlation between the two scales is 0.78.

Investment is measured with indicators for child specific investment. The household survey included a number of items that capture household investment in children including whether the child owns certain material items (shoes, two sets of clothes, and blanket), household expenditures on child clothing, and individual expenditures on education and health. We create index measures of investment with these child specific material items and spending measures. One index sums the number of material items (shoes, two sets of clothes, blanket) giving it a range of 0-3. The other investment index is a summation of whether the child has more than one of the three material well-being items, and whether parents spent any money on child education, health, and clothing. Therefore, the range is for this index is 0-4 with higher scores representing greater child investment.

3.4. Baseline Data and Analysis

Although initial randomization was successful, to ensure our sample of school-aged children are

balanced at baseline, we test for difference in means between treatment arms, adjusting standard errors for clustering at the VC level. Table 2 displays baseline mean characteristics (including all outcome and control variables) for both treatment and control group as well as the p-value for the difference in means test. We find that balance holds, there are no significant differences at the 10 percent significance level between arms for any variable used in this paper.

Table 2. Baseline characteristics of school age children (ages 6-17) by treatment status

	Treatment	Control	P-value (T-C)
	Mean (SD) or %		
Male	52.1	51.2	0.44
Age	10.8 (3.1)	10.6 (3.2)	0.22
Primary school age (6-13)	77.5	78.5	0.42
Past 2 weeks, suffered from illness or injury	18.6	17.0	0.43
Orphan	42.3	38.4	0.35
Schooling outcomes			
Enrolled in school	70.9	73.2	0.43
Dropout (if enrolled at start of year)	7.7	6.1	0.21
Withdrew for at least 2 weeks (if enrolled)	13.7	13.4	0.88
Household Characteristics			
Head went to school	37.3	35.8	0.81
Head can read	21.8	23.1	0.76
Head female	85.8	86.4	0.84
Head age	53.1 (18.5)	51.2 (17.8)	0.37
Head widow	38.0	35.3	0.52
Total members 6 to 11	1.8 (1.1)	1.9 (1.1)	0.23
Total members 12 to 17	1.5 (1.0)	1.4 (1.0)	0.35
Total members 18 to 64	1.4 (1.0)	1.4 (1.0)	0.91
Total members 65+	0.5 (0.6)	0.4 (0.6)	0.30
Household size	5.9 (2.0)	5.9 (2.0)	0.97
Per capita expenditure	32,920 (20,517)	32,133 (19,317)	0.71
Log per capita expenditure	10.4 (0.6)	10.4 (0.6)	0.86
Salima-Mangana	23.6	27.9	0.80
Salima-Ndindi	28.5	27.9	0.98
Mangochi-Jalasi	20.7	20.7	1.00
Mangochi-Mbwana Nyambi	27.2	23.5	0.82
Observations	3,022	3,292	
Clusters	14	15	

Notes: No significant differences found between T and C groups. T-tests based on standard errors clustered at the VC level.

Additionally, because randomization was at the household level but our unit of analysis is the individual child-level, we tested for differences in schooling outcomes between study arms based on the number of school-age children living in the household. The vast majority (93%) of households have 5 or

fewer school-age children at baseline (median of 3), and for each sized group 1 thru 5+, we find no significant differences for schooling outcomes between T and C arms. Since we would not expect all individual outcomes to be balanced in our study, this finding provides even stronger evidence that our control group is a valid counterfactual at the individual child-level.

Table 3. Baseline determinants of schooling outcomes

	Enrolled	Dropout	Withdraw
Age	0.16***	-0.01	0.02**
	(0.01)	(0.01)	(0.01)
Age squared	-0.01***	0.00**	-0.00
	(0.00)	(0.00)	(0.00)
Male	0.00	0.00	0.03***
	(0.01)	(0.01)	(0.01)
Past 2 weeks, suffered from illness or injury	0.02	0.01	0.04***
	(0.02)	(0.01)	(0.01)
Orphan	0.00	0.02*	-0.01
	(0.01)	(0.01)	(0.01)
Head went to school	0.04**	0.01	0.05**
	(0.02)	(0.01)	(0.02)
Head can read	0.08***	-0.03	-0.07***
	(0.03)	(0.02)	(0.02)
Head female	0.06***	-0.01	-0.04*
	(0.02)	(0.02)	(0.02)
Head age	0.00**	-0.00	-0.00***
	(0.00)	(0.00)	(0.00)
Head widow	-0.01	-0.00	0.02
	(0.02)	(0.01)	(0.02)
Total members 6 to 11	0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)
Total members 12 to 17	0.01	-0.01	-0.03***
	(0.01)	(0.01)	(0.01)
Total members 18 to 64	0.02	-0.02**	-0.03**
	(0.01)	(0.01)	(0.01)
Total members 65+	0.03	-0.01	0.01
	(0.02)	(0.01)	(0.02)
Household size	-0.01	0.01	0.01*
	(0.01)	(0.01)*	(0.01)
Baseline log per capita expenditure	0.08***	-0.04	-0.03**
	(0.01)	(0.01)***	(0.01)
Randomization Variables			
Salima-Ndindi	0.02	-0.02	-0.06
	(0.03)	(0.01)	(0.03)**
Mangochi-Jalasi	-0.09	-0.02	0.00
	(0.04)**	(0.03)	(0.02)
Mangochi-Mbwana Nyambi	-0.02	-0.02	-0.08
	(0.03)	(0.01)	(0.04)*
Treatment	-0.03	0.02	0.01
	(0.02)	(0.01)	(0.02)
Observations	6,303	4,070	4,543

Notes: Robust standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

At baseline, our sample is equally male and female, has an average age of 11, and more than a third are orphans (Table 2). Examining baseline levels of our dependent schooling outcomes, we find that over 70 percent of children 6 to 17 were enrolled in school during the 2012-2013 school year. Out of those children, about 14 percent had withdrawn for at least 2 weeks at some point during the school year across both treatment arms. Dropout levels (those that left school since the previous school year) are at 8 percent for treatment children and 6 percent for control children. Additionally, household heads are overwhelmingly female (86%), only a third ever attended school, and less than a quarter can read.

We next examine the individual and household determinants of schooling at baseline using OLS models with standard errors clustered at the VC level (Table 3). Results show that age and age squared are both strongly significant individual predicators of all schooling outcomes, and particularly for enrollment. The coefficient on age for enrollment is large and positive but negative for age squared, which signifies that enrollments are large for younger ages and start to decline as children get older.

Other individual determinants include orphan status for dropouts, and male gender and morbidity for withdrawals. In particular, morbidity (suffering from illness or injury in the past 2 weeks) increases withdrawals by 4 percentage-points (p-value<0.01) suggesting that withdrawals are associated with illness. Some characteristics of the household head are also determinants of schooling outcomes. Parental education backgrounds are an important factor in child schooling, particularly whether a household head ever attended school or can read. Children living with a household head that ever attended school are 4 percentage-points more likely to be enrolled (p-value<0.01), however, unaccountably, they are also more likely to withdraw. Children living with literate heads are also more likely to be enrolled (8 pp) and less likely to withdraw from school (-7 pp). Household financial resources are also important; log per capita expenditure is a significant predictor of enrollment (8 pp) and dropout (-4 pp).

4. Methods

To assess the impact of the Malawi SCTP on schooling, the main estimation strategy of this paper is a Differences-in-Differences model (DD), which uses both data from both pre (baseline) and post

(follow-up) periods to account for group-level differences across the two study arms and across time. Equation (1a) shows the basic empirical specification where Y_{it} is a binary outcome measure for schooling, T_i is an indicator for treatment status, P_t is an indicator for the post period, the DD estimate of treatment effect is the interaction of these indicators (T_iP_t).

(1a) $Y_{it} = \beta(T_iP_t) + \lambda T_i + \delta P_t + e_{it}$

(1b) $Y_{it} = \beta(T_iP_t) + \lambda T_i + \delta P_t + \phi X_{it} + e_{it}$

We add on to the unadjusted model in Equation (1b) with a set of individual and household covariates (X_{it}). Individual controls include a child's age, age squared, male gender, a baseline indicator for being an orphan (single or double), and a baseline indicator for morbidity (suffering from illness or injury in the past 2 weeks). These variables were chosen because they are known to affect schooling and can thus improve the precision of the impact estimates. We also control for household-level variables that could affect parenting behaviors and decision-making. All measures are defined at baseline and include the household head's sex, age, and education, as well as household characteristics including household size, total members in different age groups, consumption, and dummies for Traditional Authority residence to account for stratification in the randomization process

In the first step of analysis, we estimate the average treatment effect of the cash transfer on schooling outcomes using both the adjusted and unadjusted DD models (equation 1a and 1b). We also test for differential treatment effects for groups that might benefit most from such programs including females, older children, and those not enrolled at baseline by creating an interaction term between the DD measure (T_iP_t) and each subgroup (G_i). Specifically, we estimate equation (1c) where β_1 corresponds to the treatment effect for those not defined by G_i (i.e., males, younger children, and those enrolled at baseline) and β_1+β_2 corresponds to the treatment effect for the subgroup being tested. Thus, a differential impact with respect to each subgroup is defined by the significance of β_2.

(1c) $Y_{it} = \beta_1(T_iP_t) + \beta_2(T_iP_t * G_i) + \sigma G_i + \lambda T_i + \delta P_t + \phi X_{it} + e_{it}$

For all models, we use OLS regression and cluster standard errors at the level of randomization, the village cluster (VC). According to the literature though, our cluster robust standard errors (CRSE) might still be too small since we have a relatively small number of clusters (29 VCs). As a robustness check, we follow the advice of Cameron, Gelbach, and Miller (2008) and use the wild bootstrap method to test for downward bias in our SEs. In each table, we present our impact parameters with both the CRSEs in parentheses and the p-value from the wild bootstrap test in the row underneath. For subgroup analysis, the wild bootstrap p-value is provided for the joint test of $\beta_1$and β_2.

In the next step, we consider how the program works to affect schooling outcomes. Earlier we proposed and defined potential pathways through which a cash transfer program might work to affect child-schooling outcomes. We identified two main parental channels—increased investment in children and reductions in parental stress. We operationalize these channels with the PSS score for parental stress and index measures for child material items and child investment spending. Our approach is as follows: we estimate average treatment effects on these mediation measures and then evaluate whether the program works through these intermediary pathways to impact on schooling. To do this, we employ the 'causal steps' first proposed by Baron and Kenny (1986) to establish the necessary conditions for mediation. For each outcome-mediator pair, we separately estimate the two equations below which are modified from Baron and Kenny's original equations for longitudinal analysis (MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002). We simplify notation in Equations 2-3, but implicit in each model are indicators for treatment, time, and the set of X_{it} covariates.

(2) $M_{it} = \propto_{(2)} + \delta(T_i P_t) + e_{it\,(2)}$

(3) $Y_{it} = \propto_{(3)} + \beta'(T_i P_t) + \varphi M_{it} + e_{it\,(3)}$

In this mediation framework, significance is found through testing each step. Three conclusions are necessary (Baron & Kenny, 1986):

1) β is significant (treatment significantly affects the outcome variable in equation 1b)

2) δ is significant (treatment significantly affects the mediator in equation 2)

3) β' loses significance (after including the mediator in equation 3, the previous significant treatment effect is partially or completely diminished)

In equation (3), we also include baseline values of the mediator to control for any confounding between treatment and the mediator (Keele et al., 2015). In order to identify causal mediation effects, this step is important to satisfy the sequential ignorability assumption since the mediator was not randomzied. According to the counterfactual mediation framework proposed by Imai, Keele, & Tingely (2010), for sequential ignorability to hold, there must be no confounding between 1) treatment and the outcome (randomization to treatment takes care of this) and 2) between the mediator and treatment. To satisfy this second part of the sequential ignorability assumption, the mediator can be regarded as "as-if" randomized between treatment arms once all potential values of the outcome are conditioned on both the observed treatment and pretreatment confounders (Imai et al., 2010, Keele et al., 2015).

While the Baron and Kenny method has been used widely in psychology and the social sciences, this traditional approach relies on strong exogeneity and linearity assumptions. Recent advances in mediation analysis apply the counterfactual framework to articulate conditions needed for identification of causal mediation and to allow for greater flexibility in model choice (Pearl, 2001; VanderWeele and Vansteelandt, 2009; Imai et al., 2010; Flores & Flores-Lagunes, 2009). James Heckman and coauthors have also made recent contributions that highlights how to identify sources of the treatment effects on human capital formation by leveraging experiments. Heckman and Pinto (2015), for example, propose an 'econometric mediation analysis' and use experimental variation to decompose the causal direct and indirect effects of schooling interventions on production functions. Heckman, Pinto, and Savelyev (2013) employ these methods to decompose the treatment effects of a randomized early childhood intervention, the Perry Preschool Program, on adult outcomes while Conti, Heckman, Pinto (2015) decompose treatment effects on long-term health outcomes using both the Perry Preschool Program and the experimental Abecedarian Project.

The statistical advances made in mediation analysis since Baron and Kenny (1986) provide valuable extensions to many situations where the 'causal steps' are unsuitable; however, we believe this traditional approach is appropriate for identifying mechanisms in our study. For one, although this method does not directly quantify an indirect effect, estimating and testing each 'causal step' is useful for showing all relationships along the causal mediation pathway and also helps simplify discussion. Nevertheless, we confirmed that our conclusions about mediation from our logical tests are valid by employing two other decomposition methods: the Sobel-Goodman mediation tests that test for an indirect effect with the product of ($\delta\varphi$) from the Baron and Kenny steps and the counterfactual approach proposed by Imai et al. (2010). We find no differences in either the size or significance of indirect and direct effects as compared to the $\beta'$and φ coefficients we report later in Tables 8 (results available upon request). Moreover, because our study has a strong experimental design and we assume linear relationships between mediators and schooling outcomes, the equations (1-3) both provide causal effects of treatment on outcomes and solid identifiability conditions for causal mediation (Heckman & Pinto, 2015; Flores & Flores-Lagunes, 2009). Note that this approach is similar to that taken by other studies that examine the pathways through which cash transfers impact adolescent and child outcomes (Handa et al., 2015; Baird et al. 2013a; Kilburn, et al., 2016)

5. Results

5.1 Effect of SCTP on Schooling

Table 4 shows the main impacts of the cash transfer program on schooling outcomes for children ages 6 to 17 using the DD model from Equations (1a) and (1b).[4] We find that the SCT program has a strongly significant effect on school enrollment and dropout (columns 1-4) and that these effects are robust to the addition of individual and household covariates. Children in treatment households are 12 likely to have

[4] Single difference OLS results provided in Appendix Table 3. Estimates are similar to those in Table 4, but accounting for group differences over time is important in reducing bias. We also offer a visual representation of the change over time in enrolments by age in Appendix Figure 1.

dropped out at follow-up (columns 3 and 4). Additionally, these treatment effects are significant at the 1 percent level by means of either CRSEs or the wild bootstrap method. This consistency implies that the regression-based CRSEs do not suffer bias because of our limited number of sample clusters.[5]

Table 4. Adjusted and unadjusted estimates of average treatment effects of the SCTP on schooling outcomes for children ages 6 to 17

	(1)	(2)	(3)	(4)	(5)	(6)
	Enrolled in school		Dropout		Withdrawal for at least 2 weeks	
Treatment Effect (DD)	0.12***	0.12***	-0.04**	-0.04***	-0.03	-0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Wild bootstrap p-value	0.000	0.000	0.010	0.006	0.321	0.297
Treatment dummy	-0.02	-0.03	0.02	0.02	0.00	0.00
	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Time (Post period)	0.05***	0.05***	-0.01	-0.01	-0.06**	-0.06**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Individual and Household Controls	No	Yes	No	Yes	No	Yes
Observations	12,771	12,722	9,001	8,968	9,922	9,885
R-squared	0.025	0.067	0.007	0.023	0.020	0.032

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are for DD effect (H_0=0). Individual controls: age, age squared, male, baseline morbidity past 2 weeks, and baseline orphan status / Household controls (all defined at baseline): household head (female, age, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

In addition to examining treatment impacts for the whole sample, we looked at a few subgroups since individual characteristics like sex and age can moderate schooling impacts for reasons such as household preferences, cultural norms, or the returns to schooling for these groups. To test for differential effects by subgroups we estimate Equation (1c) for females and secondary school-aged children (14-17). Furthermore, we also examine whether treatment effects differ for those not enrolled at baseline in order to discern if the program is helping to bring children to school (either for the first time[6] or returning) as opposed to primarily keeping children in school. Baseline enrollment is consequently defined as zero for everyone in this sample so we only use follow-up data and report single-difference treatment impacts. In

[5] We find no evidence that CRSEs and p-values from the wild bootstrap method differ throughout the analysis and so henceforth, we discuss one significance of our impact parameters.

[6] At baseline, 16 percent of 6-17 year olds in the sample had never attended school (no significant difference between arms).

Table 5, we show both the coefficients on the treatment effect (β_1—DD for column 1 and 2, single difference for column 3) and the interaction term (β_2) to gauge both the total and differential impacts.

The first panel of Table 5 confirms the strong program impact on enrollment across all subgroups (significant at the 1 percent level), however, there is only a significant interaction effect for those not enrolled at baseline. The treatment effect (the sum of the DD and interaction coefficient) for females and older children (11 and 10 pp respectively) are slightly lower than for males and younger children (both 13 pp), however, the differences are not significant. However, there is a differential treatment impact for those not enrolled at baseline. Enrollments increase by 20 percentage-points for this group, and the joint significance of treatment and interaction effects (using the wild bootstrap method) in the last row is also significant at the 1 percent level.

Table 5. Estimates of average SCTP treatment effects on schooling by subsample

	(1) Female	(2) Secondary school age (14-17)	(3) Not enrolled at baseline (single differences)
Enrolled in school			
Observations	12,722	12,722	6,403
Treatment effect	0.13***	0.13***	0.07***
	(0.02)	(0.02)	(0.01)
Interaction (DD*subsample)	-0.02	-0.03	0.13***
	(0.01)	(0.02)	(0.03)
Wild bootstrap p-value	0.000	0.000	0.000
Dropout			
Observations	8,968	8,968	4,891
Treatment effect	-0.05**	-0.04**	-0.01*
	(0.02)	(0.02)	(0.01)
Interaction (DD*subsample)	0.01	0.00	-0.09***
	(0.01)	(0.01)	(0.02)
Wild bootstrap p-value	0.039	0.032	0.002
Withdrawal for at least 2 weeks			
Observations	9,885	9,885	5,330
Treatment effect	-0.05	-0.02	-0.04***
	(0.03)	(0.03)	(0.01)
Interaction (DD*subsample)	0.02	-0.02	-0.00
	(0.01)	(0.01)	(0.02)
Wild bootstrap p-value	0.204	0.131	0.011

Notes: OLS coefficient estimates for each subsample-dependent variable group come from a separate regression. Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are for the joint effect test of DD and DD*subsample (H_0=0). Individual controls: age, age squared, male, baseline morbidity past 2 weeks, and baseline orphan status / Household controls (all defined at baseline): household head (female, age, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

For other schooling outcomes, dropout and withdrawal, we also find that results do not differ by gender or age and lineup with the main effects from Table 4. As with enrollment, for those not enrolled at baseline, there is a differential impact on dropouts. The program reduces dropouts by a total of 10 percentage-points for this group (p-value<0.01). Additionally, there is a significant treatment impact (but no differential impact) on withdrawals (-4 pp) for this group. The withdrawal effect, however, is a consequence of the model being limited to follow-up data (we find a significant effect for the full sample in the single-difference model in Appendix Table 3 but this wipes out after controlling for baseline data).

Given the differential effects for those not enrolled at baseline, the program appears to have an important impact of not only keeping children in school but also helping them attend for the first time or return to school. Overall though, Table 5 does not provide strong evidence that treatment works differently on subgroups, effects are similar in size to the main effects in Table 4.

5.3 Mediation Analysis

Our results thus far provide strong evidence that cash transfers from the Malawi SCTP improve schooling outcomes for children living in treatment households. Still, it is not clear why this occurs since there is no schooling conditionality. Therefore, we continue our analysis with an examination of pathways through which the program may impact these outcomes.

We first show mean baseline values for the potential mediation channels we defined earlier (increased investment in children and reductions in parental stress) and test for balance between treatment arms (Table 6). We find no significant differences at the 10 percent level (p-value shown in last column) between treatment and control groups. At baseline, summary data show that child ownership of material items varies by item such that while less than 20 percent of the sample own shoes, over two-thirds own an extra set of clothing. Children in our sample are also about four times more likely to have expenditures on education over health or clothing. Moreover, parental stress is towards the upper end of the PSS scale (around 15 out of a scale of 4-20) indicating high stress among child caregivers.

Table 6. Mean values of mediator pathways by treatment arm at baseline

	Treatment	Control	P-value (T-C)
	Mean (SD) or %		
Material well-being items (blanket, shoes, two sets clothing) (0-3)	1.3 (0.9)	1.2 (0.9)	0.44
Two sets clothes	73.8	69.5	0.34
Shoes	18.8	17.0	0.57
Blanket	35.0	32.8	0.66
Investment spending (0-3)	0.9 (0.7)	0.9 (0.7))	0.93
Any education expenditure	64.9	65.9	0.79
Any child clothing expenditure	15.8	14.2	0.50
Any health expenditure	13.7	13.8	0.96
Stress Scale (4-20)	14.9 (3.3)	15.0 (3.4)	0.87
Education spending	528.6 (1072)	621.4 (1408)	0.37
Logged education spending	3.9 (0.1)	4.1 (0.1)	0.64
Observations	3,032	3,292	

Notes: No significant differences between T and C groups. T-tests based on standard errors clustered at the VC level.

To test for causal mediation, we first estimate the average treatment effect on these proposed parental mediation channels using equation (3). The top panel in Table 7 shows the program impact on the indicator variables that comprise our indices while the bottom panel shows the program impact on the indices, which we use to test for mediation in the next step. These index measures include child material well-being items (blanket, shoes, two sets clothing), child investment (more than one material well-being item, any education spending, any health spending, any clothing spending), and the Perceived Stress Scale for the household caregiver. We also test a child investment index without education spending because expenditures are only non-zero for enrolled students, making it a strong predictor. Thus, we can compare these two indices to identify if other aspects of child investment also account for mediation.

We find that the program has strong, significant impacts on almost all of these measures within our school-age sample. The program increases the likelihood of household expenditures on both child education (13 pp) and clothing (32 pp). Children in treatment households are also more likely to own two of the three material items: shoes (20 pp) and a blanket (16 pp). Since index measures are composed of these indicators, we also find that large, positive treatment effects on child investment index. Children in treatment households have more material items and child-specific investment spending is greater whether or not we include education as a category, (p-values<0.01). Additionally, caregivers are less stressed, they score 1.5 points lower (-0.43 SD) on the Perceived Stress Scale (p-value<0.05).

Table 7. Effect of SCTP on mediator channels

	Treatment effect (DD)	Wild bootstrap p-value	Observations
Binary Measures			
Two sets clothes	0.03	0.461	12,611
	(0.04)		
Shoes	0.20***	0.000	12,613
	(0.05)		
Blanket	0.16***	0.009	12,609
	(0.06)		
More than one item (clothes, shoes, or blanket)	0.19***	0.003	
	(0.06)		
Any education	0.13***	0.000	12,722
	(0.03)		
Any health	0.02	0.543	12,722
	(0.02)		
Any clothing	0.32***	0.000	12,722
	(0.04)		
Index Measures			
Child material well-being items (0-3)	0.40***	0.003	12,614
	(0.12)		
Child investment with education (0-4)	0.66***	0.000	12,606
	(0.10)		
Child investment without education (0-3)	0.53***	0.000	12,606
	(0.09)		
Perceived Stress Scale (4-20)	-1.46**	0.026	12,721
	(0.59)		

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are for DD effect (H_0=0). Individual controls: age, age squared, male, baseline morbidity past 2 weeks, and baseline orphan status / Household controls (all defined at baseline): household head (female, age, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

Next, we estimate mediation effects using equation (4) to test whether the direct treatment effect on schooling outcomes can be explained in part by these channels. Each mediator index measure is included separately into enrollment and dropout outcome models. Withdrawal is excluded because there was no observed treatment effect in Table 4, the first condition of the casual steps.

Compared to estimates of average treatment effects from Table 4, treatment effects in Table 8 are mostly unchanged after adding in mediator index measures. The only measure with a mediating effect on the direct treatment impact (for both enrollment and dropout) is the investment index that includes education spending (Columns 2 and 6). Compared to the index measure without education (Columns 3 and 7), including any education spending within the index results in complete mediation of the direct treatment effect since the DD coefficient is now effectively zero for enrollment (column 2) and dropout

(column 6). To understand this relationship more fully, we examine education spending in greater detail in the subsequent section.

Table 8. Effects of the SCTP on schooling outcomes accounting for mediator pathways

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Enrolled in school (ages 6 to 17)				Dropout (6 to 17)			
Treatment effect (DD)	0.12***	0.00	0.12***	0.12***	-0.04**	-0.01	-0.04***	-0.04**
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Wild bootstrap p-value	0.000	0.922	0.000	0.000	0.005	0.716	0.006	0.013
Material well-being items	0.01**				-0.01**			
	(0.01)				(0.00)			
Investment with education		0.18***				-0.07***		
		(0.01)				(0.01)		
Investment without education			0.01				-0.00	
			(0.01)				(0.00)	
Stress scale				0.00				0.00
				(0.00)				(0.00)
Wild bootstrap p-value	0.020	0.000	0.173	0.014	0.038	0.000	0.541	0.373
Observations	12,418	12,410	12,410	12,721	8,842	8,834	8,834	8,967

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are given independently for DD effect and the mediator effect (H_0=0). Baseline values for the mediators are included in each regression to control for confounding between the mediator and treatment. Individual controls: age, age squared, male, baseline morbidity past 2 weeks, and baseline orphan status / Household controls (all defined at baseline): household head (female, age, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

5.4 Effect of Education Expenditures

Given the importance of education spending, we further examine what aspects of this spending most affects schooling outcomes using the same steps used in the previous section. In the causal mediation framework, the mediator should plausibly precede the outcomes. Schooling expenses, however, might operate slightly differently since, while spending precedes school attendance, only the decision to enroll would trigger schooling expenses. Indeed, expenditures for dropouts and children not enrolled are only defined as zeros, and so education spending measures do not vary for these groups. Instead of a mediator, education spending is more appropriately understood as a 'mechanism' or 'explanation' for the effect of the SCTP on schooling as it moves somewhat concurrently with enrollment decisions.

We use two measures, total expenditure in Malawi Kwacha (MWK)[7] and an indicator for any education expenditures, to examine the extent to which education spending explains the treatment impact on schooling. The top panel shows the treatment effect on each measure and expectedly, the program strongly impacts both measures.

Table 9. Analysis of education expenditure measures as the mechanism (M) for SCTP impacts (T) on schooling outcomes (Y)

		(1) Education Expenditures (MWK)	(2) Any education expenditures
Control mean		473.1 (722.1 SD)	70.6 %
T on M			
	Treatment effect (DD)	345.61***	0.13***
		(66.05)	(0.03)
	Wild boot p-value	0.000	0.000
	Observations	12,599	12,722
T on Y plus M		**Enrollment** (original effect: 0.12***)	
	Treatment effect (DD)	0.04**	0.01
		(0.02)	(0.01)
	Wild boot p-value	0.001	0.366
	Mediator	0.02***	0.82***
		(0.00)	(0.02)
	Wild boot p-value	0.000	0.000
	Observations	12,172	12,172
		Dropout (original effect: -0.04***)	
	Treatment effect (DD)	-0.02	-0.02
		(0.01)	(0.01)
	Wild boot p-value	0.010	0.112
	Mediator	-0.01***	-0.54***
		(0.00)	(0.03)
	Wild boot p-value	0.000	0.000
	Observations	8,829	8,865

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are given independently for DD effect and the mediator effect (H_0=0). Baseline values for the mediators are included in each regression to control for confounding between the mediator and treatment. Individual controls: age, age squared, male, baseline morbidity past 2 weeks, and baseline orphan status / Household controls (all defined at baseline): household head (female, age, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

Treatment results in an increase in education spending of 346 MWK (column 1) or a 13 percentage-point increase in having any expenditure (column 2). To place these treatment effects in

[7] We drop expenditure outlier data from the top 1 percent (>5000 MWK) as these cases are unrealistic given schooling costs in Malawi.

context, education is only one percent of household consumption among study households at baseline whereas food is the biggest expense, making up nearly 80 percent. In addition to the individual impacts on education expenditures at follow-up (Table 9), we also find a significant expenditure increase at the household level of approximately 200 MWK that translates to a one percent increase in the total consumption share (Malawi SCTP Evaluation Team, 2015).

In the bottom panel of Table 9, we add expenditure measures separately into the outcome regression models for enrollment and dropout. We find strong evidence that education spending explicates the SCTP impact on schooling as the original treatment effect from Table 4 is greatly attenuated in each model. By including total education expenditures (MWK), the treatment effect on enrollment (third row, column 1) is sizably reduced by almost 70 percent (from 12 to 4 pp), but is still significant at the 5 percent level while the binary measure for any expenditure (third row, column 3) fully explains the treatment effect on enrollment. Additionally, treatment effects for dropout are diminished and insignificant after accounting for expenditure measures.

Clearly schooling outcomes are dependent upon individual education expenditures; however, using measures of overall spending limit greater understanding of how the cash is spent to enable children to attend school. The household survey collected data on specific categories of education spending, and so we further examine the expenditures on different categories to untangle these relationships. At baseline, the most common expenditure category is notebooks and stationary with roughly a third of children in both arms having had expenditures in each category. School contributions and uniforms were the next most common categories. Tuition, however, is a rare expense for these children because the vast majority attends government primary schools without fees. A full list of baseline expenditure proportions and means by treatment arm (no significant differences) are provided in Appendix Table 4.

To test the effect of individual expenditure categories we continue analysis using real expenditure (in MWK) measures as effects are more easily interpretable. Table 10 shows program impacts on each expenditure category. Only two categories were significantly impacted by the program: notebooks (and

stationary) and uniforms. Expenditure increases for children in the treatment group program by 42 MWK (0.19 SD) for notebooks and stationary (column 3) and by 168 MWK (0.47 SD) for uniforms (column 4).

Table 10. Effect of SCTP on individual expenditure items (MWK)

	(1) Tuition	(2) Extra Lessons	(3) Notebooks & Stationary	(4) Uniform	(5) Boarding Fees	(6) School Contribution	(7) Transportation	(8) PTA and Other Fees
Control Mean	17.2	26.3	85.7	91.6	4.2	45.1	4.9	14.4
(SD)	(491.1)	(325.2)	(192.5)	(324.1)	(291.1)	(138.6)	(186.4)	(89.1)
Treatment effect	-6.8	12.6	41.7**	167.6***	-1.6	17.3	2.8	2.4
	(14.4)	(16.8)	(16.8)	(29.3)	(8.1)	(13.4)	(6.3)	(7.0)
Wild bootstrap p-value	0.677	0.492	0.031	0.000	0.766	0.220	0.716	0.759
Observations	12,599	12,599	12,599	12,599	12,599	12,599	12,599	12,599

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are for DD effect (H_0=0). Individual controls: age, age squared, male, baseline morbidity past 2 weeks, and baseline orphan status / Household controls (all defined at baseline): household head (female, age, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

With these two items, we estimate equation 4 again to test whether expenditures (in 100s of MWK) on notebooks and uniforms explain the program's schooling impacts. Results in Table 11 show that both notebooks and uniforms explain part of the direct treatment effects on enrollment and dropout.

Table 11. Effects of SCTP on schooling accounting for expenditure on books and uniforms (100s MWK)

	Enrolled (original effect: 0.12***)		Dropout (original effect: -0.04***)	
DD (Treatment*Time)	0.09***	0.09***	-0.04**	-0.03**
	(0.02)	(0.02)	(0.02)	(0.02)
Wild bootstrap p-value	0.000	0.000	0.020	0.027
Mediators				
Notebooks & Stationary	0.06***		-0.02***	
	(0.01)		(0.00)	
Uniform		0.02***		-0.00***
		(0.00)		(0.00)
Wild bootstrap p-value	0.000	0.000	0.000	0.000
N	12,034	12,034	8,728	8,728

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are given independently for DD effect and the mediator effect (H_0=0). Baseline values for the mediators are included in each regression to control for confounding between the mediator and treatment. Individual controls: age, age squared, male, baseline morbidity past 2 weeks, and baseline orphan status / Household controls (all defined at baseline): household head (female, age, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

Including spending on either notebooks or on uniforms reduces the treatment effect on enrollment by 25 percent from 12 to 9 percentage-points (p-value<0.01). For dropout, the direct treatment effect is only attenuated after controlling for uniform spending from -4 to -3 percentage-points (p-value<0.05), but spending on notebooks does slightly diminish the significance of the treatment effect to the 5 percent level. We also find that spending on notebooks and uniforms have protective effects on schooling, all coefficients are large and significant at the 1 percent level. Spending on notebooks has a particularly large protective effect—a 100 MWK increase in spending leads to a 6 percentage-point increase in enrollment and a 2 percentage-point reduction in dropouts.

5.5 Other Mechanisms

Earlier we described how the Malawi SCT program's payment structure increases the transfer size (top-ups) for each member of the household under certain age thresholds to support households in sending their school-age children to school. Similar to 'labelled cash transfers' (Benhassine et al., 2015), this may further encourage households to send their children to school (on top of the income effect) if they believe that it is a requirement of receiving the transfer. We therefore examined treatment households' perception of program rules at follow-up to understand if these top-ups could further serve as a mechanism to explain the program's schooling impacts. To restate though, the program does not have rules for recipients and beneficiaries receive their full transfer amount whether or not their children are attending school.

At follow-up, beneficiaries were asked if they believed there were rules they had to follow in order to receive the payments. If they answered yes, they were also asked to list those rules and rank the most important ones. Out of 1,562 treatment respondents, 81 percent believed they had to follow rules in order to continue receiving payments. Of those who believed in rules, 70 percent believed they had to purchase school supplies, 26 percent believed they had to send their children to primary school, and only 8 percent to secondary school. Additionally, out of all rules beneficiaries listed, purchase of school supplies was the rule that was most frequently believed to be the most important with 32 percent placing

it at the top. We ran additional regressions on the follow-up treatment group to examine whether rule perception (any rule, primary school, or school supplies) significantly predicted our schooling outcomes (Appendix Table 5). Consistent with our findings on the importance of education expenditures, a belief in the rule of spending money on school supplies was most important—both significantly related to enrollment (5 pp) and dropout (-2pp). Despite the perception of rules by beneficiaries, only 30 percent believe that anyone checks that they are following the rules, which could help to explain the minimal effect sizes we find.

Finally, we also examined other explanations for observed schooling impacts in analysis not shown here. We tested other potential schooling moderators—factors that could affect schooling outcomes but would not be impacted by the cash transfer program and so would not plausibly mediate the impact from treatment to schooling. Neither of the factors we tested, the time it takes to get to school and whether there is a school-feeding program, moderated the treatment effect on schooling. We also examined other potential mediators—household food consumption, transfer share, and child labor—and find no mediation of these measures either.

6. Conclusion

6.1 Discussion

In this study, we show that Malawi's unconditional cash transfer program, the SCTP, is an effective demand-side education intervention. The cash helps poor children to attend school by alleviating the financial burden of schooling for the household. Specifically, school-age children (aged 6-17) in treatment households are 12 percentage-points more likely to be enrolled in school and 4 percentage-points less likely to dropout. Furthermore, examining the impacts by individual subgroups, we find that impacts do not differ by gender and age. However, we do find that for those children that were not enrolled at baseline, treatment effects are even stronger (20 pp for enrollment and -10 pp for dropouts), implying that the cash is helping children return to school or permitting them to go for the first time.

We also investigated mechanisms through which the cash may work to improve schooling

outcomes. Since the literature highlights how income affects children indirectly through parental decisions, our main analysis investigates mediators of parental investment and stress. In addition, we examined other explanations for observed schooling impacts including channels of household food consumption, transfer share, and child labor. Our results indicate that impacts are entirely explained by parental investment in the form of increased education related spending.

Since the cost of schooling is the biggest factor for these poor families in the decision to send their children to school, the cash works mainly by alleviating some of these economic constraints. Our results show that education spending is the mechanism for the observed treatment impact, but further analysis shows that in particular, the cash is spent on notebooks (or other stationary) and uniforms. Out of all education expenditure categories, these two items are the only ones that the program significantly impacted, and we find that individually, they both partly explain the direct treatment effects on enrollment and dropout. We also examined operational data from treatment households at follow-up to understand if the perception of rules helps explain parental investment. We find that this parental decision to spend the cash on school supplies might also be explained by beneficiary perceptions that the money is conditional on households using it for educational expenses. Even though the Malawi SCTP is unconditional, similar to the Benhassine et al. (2015) study in Morocco, many households seem to be confused by the rules of the program—a perception that may be driven by the spread of misinformation or possibly the payment structure that provides additions for school-age children. However, we do not find that the effect of rule perception is large enough to explain away our finding that the income effect is primarily responsible for the positive schooling impacts—parents can now afford certain schooling items for their children, notebooks and uniforms specifically, that help children enroll and attend school.

While these specific purchases help explain why the cash transfer is improving school enrollments and dropouts, claims that these items cause the observed schooling impacts is not very

intuitive. However, in the qualitative data[8] we find that acquisition of these items is a frequently cited reason for why children can attend and stay in school. For example, although officially primary education is free and uniforms are not compulsory, sometimes schools will not allow children to attend. Such as was the case for one male youth respondent,

> What really made me drop out is the lack of money to pay for what I have just told you but also I had no school uniform, so they sent me back from school.

Additionally, youth commonly described a stigma of being without certain school items such that they could suffer ridicule by their classmates and teachers if they lacked them. For instance, respondents at baseline discussed sometimes being bullied by teachers or school administers for dirty uniforms or lack of supplies. Moreover, the most cited reason for missing or dropping out of school was not having the basic school supplies, although other reasons included competing demands on their time such as needing to do informal wage labor (ganyu) to support the household and for girls, taking care of children.

The follow-up qualitative interviews also provide support to the story emerging from the quantitative data that the cash transfer works to improve schooling outcomes because it increases education expenditures on certain items. Interviews from both caregivers and youth often mention that the reason the cash is helping them in school is because it enables the purchase of uniforms, soap, and school supplies. Caregivers, in particular, frequently discussed how the money is important in sending kids to school with clean uniforms and school supplies. For example, one caregiver says,

> We use the money to buy washing soap so that the children should put on clean clothes when they are going to school. I also use the money to buy learning materials like notebooks and pencils, sometimes the school demands a small amount of fee in which case we also use the money from the cash transfer program.

The importance of being able to wash and have clean clothes has also been found to have a positive effect on school attendance in other qualitative studies (Attah et al., 2016). These changes are also described as helping to facilitate the entire school experience including feeling socially accepted and academically

[8] Qualitative data comes from baseline and follow-up in-depth individual interviews with a caregiver and one youth from 16 treatment households selected using a stratified sampling approach.

engaged. The same caregiver says about one of her children,

> …[Child's name] was not working hard in class because we didn't have enough money to help her with her education. But she now works hard because we started receiving money from the cash transfer program.

In addition, youth also described how the program had led to improvements in their school experiences. For example, one male orphan explains how the cash has made a difference since baseline,

> In the past I used to miss a lot of classes because I had no clothes. But now I have enough clothes, including a school uniform. I hope that I will continue with school…I had no hope of continuing school the last time we talked because of what was happening to me.

While many youth explained how the cash is helping them or other children of the household attend school, in some cases, the cash was not enough to overcome the financial costs. One female simply states why she stopped, "Poverty is the reason, lack of clothes, and lack of soap." Another states that he would like to return and admires his friends in school, but to return what he needs is, "Money…[it] would help me to get some of the necessary things required for school [like] school uniform, notebooks and pencils." In his case, the money from the cash transfer was being used for other purposes such as food and caring for his disabled mother and so it was not enough to help him attend school.

Moreover, another issue for older children is the cost differential of attending secondary school. In one youth's case, he had completed primary school and started to attend secondary school but the cash transfer was not enough for his family to afford the increased fees and so he was sent home for not paying them. One last challenge that youth expressed in attending school was not the financial cost but other responsibilities and demands on their time. For example, although one female wanted to return to school after her households started receiving the cash, she did not have anyone to watch her young child.

6.2 Limitations

One limitation of this study is that there are fundamental challenges in isolating the ways in which income affects child development outcomes. For one, these pathways are mostly unobserved and endogenous to the household (Strauss & Thomas, 2008; Shadish et al., 2002). Moreover, the measures we have of parental stress and investment are latent constructs for the true parental behaviors and thus we

may be imprecisely measuring their impact. Finally, the causal mediation literature shows the strongest identification test would require randomization to mediator levels but our measures are not externally manipulated meaning the model may lack predictive power (Bullock & Ha, 2011). Nevertheless, the strength of our study design, including the longitudinal data and randomizing economic conditions, and the use of non-experimental econometric methods, means that this analysis offers reasonably strong evidence for our mediation results.

Another limitation is that we are limited in testing short-term outcomes since we do not have measures of achievement or cognitive aptitude. The goals of the SCTP are to increase child human capital and although we cannot directly test for this, evidence from Malawi shows that there are relatively large returns to schooling; the private rate of return to primary schooling is 15 percent and 44 percent for secondary schooling (World Bank, 2010). Moreover, just gaining basic literacy and numeracy skills are valuable for participation in economic activities throughout life. Therefore, prolonged school attendance that leads to grade completion is an important factor in improving children's later-life outcomes.

One final limitation is that expenditures on educational items are only collected for enrolled students. In order to better understand the impact of educational resources on schooling, we would ideally collect ownership of material items in addition to expenditures for all school-age children. However, given that our results are strong and robust across the qualitative evidence, it appears that we are identifying the most likely material items that are producing improved schooling outcomes.

6.3 Concluding Remarks

This study provides causal evidence from Malawi's SCTP that an unconditional social cash transfer program can have strong effects on school attendance for children in beneficiary households, and it works by relieving some of the financial barriers of schooling. More specifically, it helps families to purchase uniforms, notebooks, and other school supplies. Although improving schooling and child human capital is an objective of the SCTP (and many similar programs across the developing world), there is no obligation for families to send their children to school to receive the money. Therefore, our findings

indicate that parents are eager to invest in their child's education, and by helping families meet the costs of schooling, unconditional cash transfers can directly increase the demand for education.

Although Malawi's SCTP may help children enroll and stay in school, it is not clear, however, that this will lead to greater human capital accumulation. Malawi's education system is stressed—there are not enough teachers or classrooms, schools are overcrowded and dilapidated, and facilities often lack proper sanitation or clean water (World Bank, 2010). Even if programs are successful in increasing short-term outcomes such as enrollment, poor educational quality is a threat to achieving medium-term outcomes such as greater student achievement. Therefore, it may be that efforts to improve the demand for education through cash transfers will be undermined without improvements to poor-quality educational systems.

Intuitively, we would expect that supply-side investments are important but evidence from other countries is ambiguous as to how they interact with cash transfers to impact both short-term and medium-term outcomes. More evidence is needed on whether cash transfers that are accompanied by supply-side interventions lead to improvements in both short-term outcomes like enrollment and medium-term outcomes like improved educational achievement. However, even if quality improvements are gradual, schooling appears to be one of the most promising pathway through which cash transfers may contribute to the successful transition to adulthood. Recent cash transfer evidence has been showing the protective relationship school attendance appears to have on a number of child and adolescent development outcomes such as early pregnancy (Handa et al., 2015), sexual behaviors, (Baird, Garfein, McIntosh, & Özler, 2012) and mental health (Baird et al., 2013a; Kilburn et al., 2016).

Overall, this study contributes to emerging evidence on the influence of social cash transfer programs in SSA to promote child development by targeting household poverty. Results reveal that within a relatively short amount of time, unconditional cash programs can improve child-schooling outcomes and that parents will invest resources in their children even without an explicit condition. Implications are that in these ultra-poor contexts where enrollments are lower than socially desired, this

type of poverty-targeted cash transfer program could result in large, cost-effective improvements in child schooling and human capital. Policymakers should therefore be conscious of the potential efficacy of these programs to meet world development goals by increasing the demand for education.

Acknowledgements

The authors recognize the contributions of several parties, without which this study would not have been possible. The impact evaluation of the Malawi Social Cash Transfer Programme (SCTP) is commissioned by the Ministry of Gender, Children, Disability and Social Welfare (MoGCDSW) and UNICEF Malawi and implemented by the Carolina Population Center at the University of North Carolina and the Center for Social Research at the University of Malawi. Funding for the evaluation is provided by UNICEF, KfW, European Union, Food and Agriculture Organization (FAO)-Rome and 3IE. The study has benefited from input of the wider evaluation team who are listed here by affiliation and then alphabetically within affiliation: Carolina Population Center (Sara Abdoulayi, Kristen Brugh, Adria Molotsky, and Frank Otchere), FAO-Rome (Solomon Asfaw, Benjamin Davis), MoGCDSW (Charles Chabuka, Gideon Kachingwe, Esmie Kainja, Laurent Kansinjiro), Ministry of Finance and Economic Planning (Harry Mwamlima), UNICEF Malawi (Edward Archibald, Maki Kato, Lisa-Marie Ouedraogo, Sophie Shawa, Tayllor Spadafora), and UNICEF Office of Research (Bruno Martorano, Tia Palermo, Amber Peterman). Special thanks to Chantal Elmont of Ayala Consulting for detailed information about program implementation.

References

Attah, R., Barca, V., Kardan, A., MacAuslan, I., Merttens, F., & Pellerano, L. (2016). Can social protection affect psychosocial wellbeing and why does this matter? Lessons from cash transfers in Sub-Saharan Africa. *The Journal of Development Studies*, *52*(8), 1-17.

Baird, S., De Hoop, J., & Özler, B. (2013a). Income shocks and adolescent mental health. *Journal of Human Resources*, *48*(2), 370-403.

Baird, S., Ferreira, F. H., Özler, B., & Woolcock, M. (2013b). Relative effectiveness of conditional and unconditional cash transfers for schooling outcomes in developing countries: a systematic review. *Campbell Systematic Reviews*, *9*(8).

Baird, S. J., Garfein, R. S., McIntosh, C. T., & Özler, B. (2012). Effect of a cash transfer programme for schooling on prevalence of HIV and herpes simplex type 2 in Malawi: a cluster randomised trial. *The Lancet*, *379*(9823), 1320-1329.

Baron, R. M., & Kenny, D. A. (1986). The moderator–mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. *Journal of Personality and Social psychology*, *51*(6), 1173.

Barrientos, A., & DeJong, J. (2006). Reducing child poverty with cash transfers: A sure thing?. *Development Policy Review*, *24*(5), 537-552.

Bastagli, F., Hagen-Zanker, J., Harman, L., Barca, V., Sturge, G., Schmidt, T., & Pellerano, L. (2016). *Cash transfers: what does the evidence say? A rigorous review of programme impact and the role of design and implementation features*. London: Overseas Development Institute (ODI). www.odi/org/projects/2797-social-protection-literature-review-poverty-impact

Becker, G. S. (1962). Investment in human capital: A theoretical analysis. *Journal of Political Economy*, *70*(5, Part 2), 9-49.

Becker, G. S., & Tomes, N. (1994). Human capital and the rise and fall of families. In *Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education (3rd Edition)* (pp. 257-298). The University of Chicago Press.

Behrman, J. R. (2010). Investment in education inputs and incentives. *Handbook of Development Economics*, *5*, 4883-4975.

Ben-Porath, Y. (1967). The production of human capital and the life cycle of earnings. *The Journal of Political Economy*, 352-365.

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*(3), 86-125.

Bullock, J. G., & Ha, S. E. (2011). Mediation analysis is harder than it looks. *Cambridge handbook of experimental political science*, 508-521.

Bruns, B., Mingat, A., & Rakotomalala, R. (2003). *A chance for every child: Achieving universal primary education by 2015*. Washington, DC: World Bank.

Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, *90*(3), 414-427.

Conger, R. D., & Elder Jr, G. H. (1994). *Families in Troubled Times: Adapting to Change in Rural America.* Transaction Publishers.

Cunha F, Heckman JJ. (2007). The technology of skill formation. *American Economic Review, 97* (2), 31–47.

Cunha, F., Heckman, J. J., & Schennach, S. M. (2010). Estimating the technology of cognitive and noncognitive skill formation. *Econometrica*, *78*(3), 883-931.

Cunha, F., & Heckman, J. J. (2009). The economics and psychology of inequality and human development. *Journal of the European Economic Association*, *7*(2-3), 320-364.

Handa, Sudhanshu; Davis, Benjamin (2015). How Much Do Programmes Pay? Transfer size in selected national cash transfer programmes in sub-Saharan Africa, *Innocenti Research Briefs* no. 5, UNICEF Innocenti Research Centre, Florence. https://www.unicef-irc.org/publications/786/

Duflo, E. (2003). Grandmothers and granddaughters: old-age pensions and intrahousehold allocation in South Africa. *The World Bank Economic Review*, *17*(1), 1-25.

Education Policy Research Center (EPDC). (2014). Malawi National Education Profile. FHI 360. http://www.epdc.org/education-data-research/malawi-national-education-profile

Fernald, L. C., & Hidrobo, M. (2011). Effect of Ecuador's cash transfer program (Bono de Desarrollo Humano) on child development in infants and toddlers: a randomized effectiveness trial. *Social Science & Medicine*, *72*(9), 1437-1446.

Fiszbein, A. & Schady, N. R. (2009). *Conditional cash transfers: reducing present and future poverty*. Washington, DC: World Bank Publications.

Flores, C. A., & Flores-Lagunes, A. (2009). Identification and estimation of causal mechanisms and net effects of a treatment under unconfoundedness. *IZA Discussion Paper* No. 4237. https://papers.ssrn.com/sol3/papers2.cfm?abstract_id=1423353

Gershoff, E. T., Aber, J. L., Raver, C. C., & Lennon, M. C. (2007). Income is not enough: Incorporating material hardship into models of income associations with parenting and child development. *Child Development*, *78*(1), 70-95.

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.

Glewwe, P., & Muralidharan, K. (2015). Improving school education outcomes in developing countries: evidence, knowledge gaps, and policy implications. Oxford, UK: *Research on Improving Systems of Education (RISE) Working Paper.* http://www.bsg.ox.ac.uk/sites/www.bsg.ox.ac.uk/files/

documents/RISE_WP- 001_Glewwe_Muralidharan.pdf

Guo, G., & Harris, K. M. (2000). The mechanisms mediating the effects of poverty on children's intellectual development. *Demography*, *37*(4), 431-447.

Handa, S., Kilburn, K., Barrington, C., Angeles, G., Mvula, P., & Tsoka, M. (2013). *Impact evaluation of the Malawi SCT inception report.* Chapel Hill, NC: Carolina Population Center at the University of North Carolina.

Handa, S., Peterman, A., Huang, C., Halpern, C., Pettifor, A., & Thirumurthy, H. (2015). Impact of the kenya cash transfer for orphans and vulnerable children on early pregnancy and marriage of adolescent girls. *Social Science & Medicine*, *141*, 36-45.

Haveman, R., & Wolfe, B. (1995). The determinants of children's attainments: A review of methods and findings. *Journal of Economic Literature*, 33, 1829–1878.

Heckman, J. J. (2006). Skill formation and the economics of investing in disadvantaged children. *Science*, *312*(5782), 1900-1902.

Heckman, J. J., & Pinto, R. (2015). Econometric mediation analyses: Identifying the sources of treatment effects from experimentally estimated production technologies with unmeasured and mismeasured inputs. *Econometric Reviews*, *34*(1-2), 6-31.

Heckman, J., Pinto, R., & Savelyev, P. (2013). Understanding the mechanisms through which an influential early childhood program boosted adult outcomes. *The American Economic Review*, *103*(6), 2052-2086.

Hanlon, J., Barrientos, A., & Hulme, D. (2010). *Just give money to the poor: The development revolution from the global South*. Kumarian Press.

Imai, K., Keele, L., & Tingley, D. (2010). A general approach to causal mediation analysis. *Psychological Methods*, *15*(4), 309.

Keele, L., Tingley, D., & Yamamoto, T. (2015). Identifying mechanisms behind policy interventions via causal mediation analysis. *Journal of Policy Analysis and Management*, *34*(4), 937-963.

Kilburn, K., Thirumurthy, H., Halpern, C. T., Pettifor, A., & Handa, S. (2016). Effects of a large-scale unconditional cash transfer program on mental health outcomes of young people in Kenya. *Journal of Adolescent Health*.

Linver, M. R., Brooks-Gunn, J., & Kohen, D. E. (2002). Family processes as pathways from income to young children's development. *Developmental Psychology*, *38*(5), 719.

Macours, K., Schady, N., & Vakis, R. (2012). Cash transfers, behavioral changes, and cognitive development in early childhood: evidence from a randomized experiment. *American Economic Journal: Applied Economics*, 247-273.

MacKinnon, D., Lockwood, C., Hoffman, J., West, S., & Sheets, V. (2002). A comparison of methods to test mediation and other intervening variable effects. *Psychological Methods*, *7*(1), 83.

Malawi SCTP Evaluation Team. (2014). *Malawi Social Cash Transfer Program baseline evaluation report*. Chapel Hill, NC: Carolina Population Center at the University of North Carolina. https://transfer.cpc.unc.edu/?page_id=1258

Malawi SCTP Evaluation Team. (2015). *Malawi Social Cash Transfer Program midline evaluation report*. Chapel Hill, NC: Carolina Population Center at the University of North Carolina. https://transfer.cpc.unc.edu/?page_id=1258

Maluccio, J., & Flores, R. (2005). *Impact evaluation of a conditional cash transfer program: The Nicaraguan Red de Protección Social*. Washington, DC: International Food Policy Research Institute (IFPRI).

Mayer, S. E. (1997). *What money can't buy: Family income and children's life chances*. Cambridge, MA: Harvard University Press.

Pearl, J. (2001). Direct and indirect effects. In *Proceedings of the seventeenth conference on uncertainty in artificial intelligence* (pp. 411-420). Morgan Kaufmann Publishers Inc.

Psacharopoulos, G., & Patrinos, H. A. (2004). Returns to investment in education: a further update. *Education economics*, *12*(2), 111-134.

Schubert, B., & Slater, R. (2006). Social cash transfers in low-income African countries: Conditional or unconditional?. *Development Policy Review*, *24*(5), 571-578.

Shadish, W.R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and quasi-experimental designs for generalized causal inference*. Houghton Mifflin.

UIS (UNESCO Institute for Statistics) and EFA (Education for All Global Monitoring Report) (2015). A growing number of children and adolescents are out of school as aid fails to meet the mark. *Institute for Statistics Fact Sheet No. 31/ Policy Paper 22*. http://www.uis.unesco.org/Education/Documents/fs-31-out-of-school-children-en.pdf

UNESCO. (2015). *Education for All 2000-2015: Achievements and Challenges: Education for All Global Monitoring Report*. http://unesdoc.unesco.org/images/0023/002322/232205e.pdf

UNICEF. (2016). *Education: Secondary net attendance rate – Percentage.* UNICEF Global Database [data.unicef.org]. Retrieved from: https://data.unicef.org/topic/education/secondary-education/

VanderWeele, T., & Vansteelandt, S. (2009). Conceptual issues concerning mediation, interventions and composition. *Statistics and Its Interface*, *2*, 457-468.

Yeung, W. J., Linver, M. R., & Brooks–Gunn, J. (2002). How money matters for young children's development: Parental investment and family processes. *Child development*, *73*(6), 1861-1879.

World Bank. (2010). The Education System in Malawi. Africa education country status report, World Bank working paper, no. 182. *World Bank Working Papers*. https://openknowledge.worldbank.org/handle/10986/5937

World Bank. (2011). *Learning for all: Investing in People's Knowledge and Skills to Promote Development, World Bank Group education strategy 2020*. Washington, DC: The World Bank.

Appendix

Appendix Table 1. Success of randomization: Mean values of key indicators at baseline by treatment status

	Treatment	Control	Difference (T-C)	P-value
Head female (%)	83.6	82.4	1.2	0.60
Head age (%)	56.3	58.3	-2.0	0.40
Head ever attended school (%)	32.0	33.3	-1.3	0.82
Head literate (%)	20.7	19.4	1.3	0.73
Head widow (%)	41.3	43.9	-2.6	0.53
Head never married (%)	3.0	2.7	0.3	0.79
Numbers of persons in household	4.6	4.5	0.1	0.79
Per capita expenditure (MWK)	43,780	46,465	-2,685	0.47
Expenditure per cap < poverty line (%)	91.6	89.3	2.3	0.26
Eat only one meal/day (%)	20.3	22.1	-1.8	0.68
Cultivate land (%)	95.7	95.7	0.0	0.99
Sell Crops (%)	21.3	21.7	-0.4	0.91
Own an enterprise (%)	23.5	26.0	-2.5	0.58
Work ganyu labor (%)	59.5	57.5	2.0	0.72
Work wage labor (%)	5.7	4.4	1.3	0.46
Observations	*1,678*	*1,853*		
Clusters	*14*	*15*		

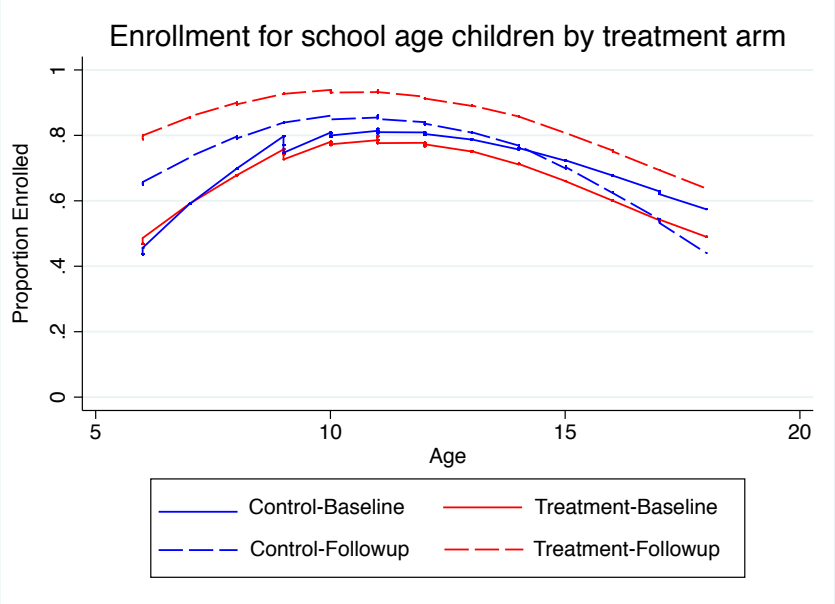
Notes: No significant differences found between T and C groups. P-values based on T-tests with standard errors clustered at the VC level.

Appendix Table 2. Attrition analysis of key household indicators

	Treatment			Control			Difference	
	Attritors (1)	Non-attritors (2)	P-value (3)	Attritors (4)	Non-attritors (5)	P-value (6)	Col(1)-Col(4) (7)	P-value (8)
Head female (%)	78.6	82.6	0.43	69.2	84.3	0.01	-9.3	0.19
Head age (%)	61.19	58.2	0.43	54.5	56.4	0.56	-6.7	0.26
Head ever attended school (%)	30.0	33.5	0.66	35.2	31.8	0.62	5.2	0.63
Head literate (%)	21.4	19.3	0.72	28.6	20.3	0.3	7.1	0.48
Head widow (%)	50.0	43.7	0.49	41.8	41.3	0.94	-8.2	0.50
Head never married (%)	1.4	2.8	0.38	4.4	2.9	0.33	3.0	0.18
Per capita expenditure (MWK)	65148.5	45,651.3	0.01	51,728.9	43,369.1	0.24	-13,419.6	0.18
Expenditure per cap < poverty line (%)	80.0	89.7	0.08	89.0	91.7	0.54	9.0	0.20
Numbers of persons in household	3.3	4.6	0.00	4.0	4.6	0.02	0.7	0.16
Eat only one meal/day (%)	20.0	22.2	0.70	24.2	20.1	0.32	4.2	0.53
Cultivate land (%)	88.6	96.0	0.27	94.5	95.7	0.4	5.9	0.41
Sell Crops (%)	16.4	21.9	0.15	25.6	21.1	0.39	9.2	0.11
Own an enterprise (%)	18.6	26.3	0.41	22.0	23.6	0.75	3.4	0.71
Work ganyu labor (%)	45.7	58.0	0.17	48.4	60.0	0.15	2.6	0.83
Work wage labor (%)	2.9	4.5	0.14	7.7	5.6	0.45	4.8	0.21
Credit constrained-loans (%)	38.6	45.5	0.35	44.0	43.7	0.97	5.4	0.59
Credit constrained on purchases on credit (%)	75.8	68.2	0.17	67.4	70.1	0.61	-8.4	0.33

Notes: No significant differences found between T and C groups. P-values based on T-tests with standard errors clustered at the VC level. Overall N for control is 1,853 (In study/non-attritors=1,762; Attritors=91). Overall N for treated is 1,678 (In study/non-attritors=1,608; Attritors=70).

Appendix Figure 1. Lowess graph of school enrollment over age by treatment arm



Appendix Table 3. Single-difference estimates of average treatment effects of the SCTP on schooling outcomes for children ages 6 to 17

	(1)	(2)	(3)	(4)	(5)	(6)
	Enrolled in school		Dropout		Withdrawal for at least 2 weeks	
Treatment	0.10***	0.10***	-0.02**	-0.03***	-0.03*	-0.03***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Individual and Household Controls	No	Yes	No	Yes	No	Yes
Wild boot p-value	0.000	0.000	0.022	0.002	0.071	0.009
Observations	6,447	6,419	4,916	4,898	5,362	5,342

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Wild bootstrap p-values are for treatment effect (H_0=0). Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household controls (defined at baseline): household head (female, age, age squared, ever attended school, chronic illness, married), log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+) and dummies for Traditional Authority residence

Appendix Table 4. Baseline values of education expenditure items for enrolled children by treatment arm

	Treatment	Control	P-value (T-C)
		Mean (SD) or %	
Expenditure Proportions			
Any Tuition	1.3	0.8	0.34
Any extra lessons	9.7	9.2	0.89
Any books	35.5	30.4	0.28
Any uniform	13.9	12.3	0.61
Any boarding fees	0.2	0.1	0.23
Any school contribution	29.5	22.0	0.12
Any transportation	0.3	0.3	0.95
Any PTA and other fees	13.5	0.9	0.16
Expenditure Expenditures (MWK)			
Education total	745.7 (1208)	848.5 (1586)	0.39
Tuition	36.4 (729)	37.4 (700)	0.97
Extra lessons	32.8 (207)	41.1 (440)	0.66
Books	112.3 (292)	95.2 (261)	0.43
Uniform	128.9 (374)	123.2 (398)	0.86
Boarding fees	8.5 (328)	4.4 (164)	0.58
School contribution	70.3 (185)	59.3 (191)	0.41
Transportation	3.8 (82)	7.6 (233)	0.55
PTA and other fees	22.6 (87)	22 (111)	0.93
Observations	2,149	2,411	

Notes: No significant differences between T and C groups. T-tests based on standard errors clustered at the VC level.

Appendix Table 5. Impact of rule perception on schooling outcomes for treatment households

	(1)	(2)	(3)	(4)	(5)	(6)
	Enrolled in school			Dropout		
Do you families participating in the SCT program have to follow any rules?	0.05**			-0.02		
	(0.02)			(0.02)		
Rule-Sending has to attend primary school		-0.02			-0.00	
		(0.02)			(0.01)	
Rule-Have to purchase school supplies			0.06***			-0.02**
			(0.02)			(0.01)
Observations	3,067	3,067	3,067	2,452	2,452	2,452

Notes: Robust standard errors in parentheses clustered at the VC level. *** p<0.01, ** p<0.05, * p<0.1. Each regression includes a dummy variable for the post period to control for the main program effect.