The World Bank Economic Review, 0(0), 2019, 1–28
doi: 10.1093/wber/lhz004
Article

Cash Transfers, Microentrepreneurial Activity, and Child Work: Evidence from Malawi and Zambia

Jacobus de Hoop, Valeria Groppo, and Sudhanshu Handa, on behalf of the Malawi Social Cash Transfer Program and the Zambia Multiple Category Targeted Program study teams

Abstract

Cash transfer programs are rapidly becoming a key component of the social safety net of many countries in Sub-Saharan Africa. The primary aim of these programs is to help households improve their food security and to smooth consumption during periods of economic duress. However, beneficiary households have also been shown to use these programs to expand their microentrepreneurial activities. Cluster-randomized trials carried out during the rollout of large-scale programs in Malawi and Zambia show that children may increase their work in the household enterprise through such programs. Both programs increased forms of work that may be detrimental to children, such as activities that expose children to hazards in Malawi and excessive working hours in Zambia. However, both programs also induced positive changes in other child well-being domains, such as school attendance and material well-being, leading to a mixed and inconclusive picture of the implications of these programs for children.

JEL classification: D13, H43, I15, I25, J22, J24, O15, O22, Q12

Keywords: cash transfers, child labor, education, Malawi, microenterprises, RCT, Zambia

1 Introduction

Cash transfer programs providing regular unconditional income support to ultrapoor and vulnerable households are rapidly becoming a key component of the social safety net of many countries in

Jacobus de Hoop (corresponding author) is a manager of humanitarian policy research at the UNICEF Office of Research-Innocenti, Florence, Italy; his email address is jdehoop@unicef.org. Valeria Groppo is a social policy specialist at the UNICEF Office of Research-Innocenti, Florence, Italy; her email address is vgroppo@unicef.org. Sudhanshu Handa is the Lawrence I. Gilbert Distinguished Professor, Public Policy, Carolina Population Center, University of North Carolina at Chapel Hill (UNC-CH), Chapel Hill, United States of America; his email address is shanda@email.unc.edu. Authors are in alphabetical order to denote equal contribution. For comments, the authors thank the editor, the anonymous referees, Gustavo Angeles, Lauren Damme, Andrew Dillon, Kevin Hong, Peter Mvula, Luisa Natali, Stephen Smith, participants in a workshop in Malawi, a seminar at the United States Department of Labor, the 2017 SITES conference, and the 2017 North East Universities Development Conference. They are grateful to the teams that implemented the impact evaluations in Malawi and Zambia and to all external partners who made this work possible. The Malawi Cash Transfer Evaluation team consists of Sara Abdoulayi, Gustavo Angeles, Clare Barrington, Kristen Brugh, Sudhanshu Handa (Principal Investigator, PI), Kelly Kilburn, Adria Molotsky, Frank Otchere, and Susannah Zietz (UNC-CH); Peter Mvula and Maxton Tsoka (PIs) (Centre for Social Research, University of

Sub-Saharan Africa (World Bank 2018). The primary aim of these programs is to help households improve food security and to smooth consumption during periods of economic duress. Beyond this primary aim, cash transfers may increase the investment of credit-constrained beneficiary households in (typically agricultural) microenterprises. For instance, see Handa et al. (2018a), who summarize impacts of eight cash transfer programs in Sub-Saharan Africa, including the ones in Malawi and Zambia studied here.[1]

For a combination of reasons, children may increase their work in the expanded household enterprise. Cash transfer programs often target households with a high ratio of dependents to healthy adults. As a result, these households may be constrained in their capacity to augment the labor supply of adult members. Labor market imperfections may limit the ability of households to hire external labor. Liquidity constraints may raise the shadow price of inputs that require cash payments, such as hired labor, making it more efficient to rely on family labor (Singh, Squire, and Strauss 1986). And the benefits of acquiring experience working in the household enterprise may grow as the enterprise expands.[2]

Understanding program impacts on child engagement in productive activities is important because these activities may have implications for child well-being both immediately and in the long run. This paper provides experimental evidence confirming that, in households receiving unconditional cash support, children may work more for the household enterprise. The focus is on large-scale, unconditional, government-run cash transfer programs in Malawi and Zambia. These programs have been selected for the analysis because they target comparable and particularly relevant populations: labor and credit-constrained, ultrapoor, smallholder farmers. The study relies on cluster randomized designs to establish the effects of these programs.

There are differences in program design features across the two countries. Transfer size, for instance, depends on household composition in Malawi, but is flat in Zambia. And although the core identification strategy (randomization) is the same across the country studies, there are some differences in the evaluation designs. The time between baseline and endline data collection, for example, was two years in Malawi and three years in Zambia. One cannot therefore conclusively explain differences in the evaluation findings for the two countries. There are nonetheless broad similarities in these findings across

Malawi); and Jacobus de Hoop and Amber Peterman (UNICEF Office of Research-Innocenti). The Zambia Cash Transfer evaluation team consists of Juan Bonilla, Alvaro Ballarin Cabrera, Thomas De Hoop, Gilbert Kiggundu, Nisha Rai, Hannah Reeves, David Seidenfeld (PI), Joshua Sennett, Dan Sherman, Jonathan Sokoll, Amy Todd, and Rosa Castro Zarzur (American Institutes for Research); Alefa Banda, Liseteli Ndiyoi, Gelson Tembo, and Nathan Tembo (Palm Associates); Sudhanshu Handa (PI) (UNC-CH); Tia Palermo, Amber Peterman, and Leah Prencipe (UNICEF Office of Research-Innocenti). The Malawi program impact evaluation was funded by the European Union, the German government (KfW), Irish Aid, the Food and Agriculture Organization of the United Nations (FAO), the International Initiative for Impact Evaluation (3ie), and UNICEF Malawi. The Zambia program impact evaluation was funded by the UK Department for International Development (DFID), UNICEF, and development partners in Zambia. Funding for this paper was provided by the United States Department of Labor under cooperative agreement Number IL-26694-14-75-K-36 to UNICEF Office of Research-Innocenti. This material does not necessarily reflect the views or policies of the United States Department of Labor, nor does mention of trade names, commercial products, or organizations imply endorsement by the United States Government; 100 percent of the total costs of this paper is financed with federal funds, from the total cooperative agreement of USD1,384,468. The findings, interpretations, and conclusions expressed in this paper are those of the authors and do not necessarily reflect the policies or views of UNICEF. A supplementary online appendix is available with this article at *The World Bank Economic Review* website.

1 For a broader review of the literature on social protection and asset formation, see Hidrobo et al. (2018).

2 Accordingly, a nonexperimental literature finds that child labor may increase with household farmland in extremely poor rural settings. For a discussion, see Basu and Tzannatos (2003); Edmonds (2007); and Fors (2012). For recent examples, see also Basu, Das, and Dutta (2010); Dumas (2013); and Oryoie, Alwang, and Tideman (2017). Several recent papers find that the expansion of nonagricultural household enterprises may boost child engagement in household chores and economic activities. Augsburg et al. (2012), for example, find that microcredit in Bosnia led borrowers to expand their household businesses and increased the labor supply of older children. Nelson (2011) documents a similar pattern in a microcredit program in Thailand.

the two settings, suggesting that the results are relevant more generally for unconditional cash transfer programs targeting similar populations.

As documented also elsewhere (for instance, Handa et al. 2018a, b), both programs (and other programs in the region) strongly expanded household productive investment and, especially, entrepreneurial activities in agriculture. Adult household members deepened their engagement in the household enterprise and reduced their participation in work for pay outside the household. The reduction in paid work could imply that adults face heavy demands on their time and cannot expand their net labor supply or that they prefer working in the household enterprise. Beneficiary households hired more labor. And children (ages 8–17) increased their work for the household enterprise. In Malawi, children, too, shifted out of work for pay outside the household, a substitution pattern consistent with the findings of previous studies (such as Covarrubias, Davis, and Winters 2012; Prifti et al. 2017), but not observed to the same extent in Zambia.

This study also examines whether the programs affected child engagement in productive activities that may have negative consequences for their well-being. And it explores contemporaneous impacts on those domains of child well-being that may be affected by or determined simultaneously with these activities. First, it looks at child engagement in excessive working hours, defined using age-specific thresholds recommended by the International Conference of Labour Statisticians (ILO 2008). While work on the household farm increased in both countries, engagement in excessive hours of productive activities rose only in Zambia. A key concern is that child engagement in excessive working hours may lower school participation. However, both programs raised school attendance. Second, relying on a survey module extensively tested by the United Nations Children's Fund (UNICEF) (Dayıoğlu 2012), the study finds that the program in Malawi increased child exposure to work-related hazards, such as carrying heavy loads and working with dangerous tools. (This information was not collected in Zambia.) Nonetheless, child health in Malawi improved slightly.

The study contributes to the literature on cash transfer programs and child engagement in productive activities.[3] Although considerable heterogeneity exists in the conclusions about different programs, most studies find that cash transfers either have no impact or lower child participation in economic activities and household chores.[4] A comparatively large literature focuses on cash transfer programs that are conditional on schooling and that have been implemented in Latin America. The most well known is Mexico's Prospera Program (for example, see Skoufias and Parker 2001; de Janvry et al. 2006; and Behrman, Parker, and Todd 2009). However, the evidence is also growing on the effects of unconditional cash transfer programs (for instance, see Edmonds 2006; Edmonds and Schady 2012; Handa et al. 2016; and Sebastian et al. 2019). Few studies document the effects of cash transfers on potentially harmful types of child labor, such as long working hours and hazardous work. One exception is Edmonds (2006), who finds that the South African old-age pension significantly decreases child full-time employment, defined as 40 or more hours of paid work a week.[5] Another exception is Edmonds and Shrestha (2014), who find that conditional scholarships and school stipends reduce child labor in carpet weaving in Nepal, a

3 For a discussion of this literature, see Dammert et al. (2018); de Hoop and Rosati (2014); and Fiszbein and Schady (2009). This paper also links to the literature on cash transfers and schooling. For literature reviews, see Baird et al. (2014); Fiszbein and Schady (2009); Handa and Davis (2006); and García and Saavedra (2017). Moreover, the study relates to the literature on the impact of capital (grants) on microentrepreneurial activities. See de Mel, McKenzie, and Woodruff (2014); Green et al. (2016); and McKenzie, Assaf, and Cusolito (2017).

4 An exception is de Hoop et al. (2019), who show that *Pantawid Pamilia*, a large-scale conditional cash transfer program in the Philippines, raised child participation in economic activities for pay. The authors suggest that transfers that were provided conditional on children's school participation did not cover the (average) direct cost of education. Children who start attending school end up working more to make up for the cost differential.

5 Edmonds (2006) explains that the reduction in child work is consistent with liquidity constraints because the timing of the pension is fully anticipated.

sector associated with trafficking and bonded labor. These beneficial effects last only for the duration of the intervention and disappear when scholarships are discontinued.

The remainder of this paper has the following structure. Section 2 provides necessary background information by describing the design of the two programs and the accompanying evaluations. Section 3 supplies the descriptive statistics of the sample, discusses the validity of the evaluations by exploring attrition, balance, and program take-up, and describes the estimation strategy. Section 4 shows how the programs affected household entrepreneurial activities, child work, and child well-being. Section 5 discusses and concludes.

2 Background[6]

This section provides background on the two programs and information about the evaluation design and the data.

Malawi's Social Cash Transfer Program

Malawi's Social Cash Transfer Program (SCTP) is an unconditional cash transfer program that aims to reduce poverty and hunger and to increase school enrollment rates among ultrapoor households. The program is administered by the Ministry of Gender, Children and Social Welfare (MoGCSW) with policy oversight by the Ministry of Economic Planning and Development (MoEPD). The pilot stage of the SCTP, implemented in Mchinji District in 2006, had a wide range of positive impacts, including increased food security, ownership of agricultural tools, and curative care seeking (Miller, Tsoka, and Reichert 2010). Because of these results, the program was rapidly expanded, becoming a key part of Malawi's social safety net and reaching about 170,000 households across 18 districts by the end of 2015.

The SCTP targets ultrapoor, labor-constrained households. Households are classified as labor constrained if the number of household members, divided by the number of household members who are fit to work exceeds three.[7] Households are classified as ultrapoor if they are unable to meet their most basic needs.[8] Targeting is carried out in three stages. First, potentially eligible households are identified by village social protection committees based on the stated criteria of ultrapoor and labor constrained. The district commissioner's office then administers a targeting survey to these households, and the data are processed at the ministry in Lilongwe to confirm eligibility based on a proxy-means test (for the poverty criterion) and on demographic composition (for the labor-constrained criterion). The final approved lists are then sent back to the district commissioners office for confirmation. The approximate coverage rate is 10 percent of all households in program areas.

Although the SCTP is not conditional on school attendance, transfer amounts vary with the size of the beneficiary households and with the number of primary- and secondary-school-age children (table 1). During the period of the study, the average total monthly value of the transfers among households was about 2,571 Malawi Kwacha (MWK) (USD3.70), which equals 18 percent of the baseline consumption of beneficiary households. Transfers are paid in cash at local pay points every two months. At enrollment, households are told about program rules and regulations; the reason the households were selected; the

6 The descriptions of program and study design draw heavily on AIR (2016); Seidenfeld, Prencipe, and Handa (2012); and UNC-CH (2014, 2016).

7 According to the program's operation guidelines, the concept of fit to work is operationalized as household members who are 19–64 years of age who do not have any illnesses or disabilities, and, for those members ages 19–25, who are not in school.

8 A household is classified as ultrapoor if any of the following conditions is verified: (a) the household has an average of only one meal a day, (b) the household survives from begging, (c) the household is undernourished, (d) the household does not possess any valuable assets, and (e) the household does not receive any monetary help, food, or gifts from others.

Table 1. Program Parameters and Evaluation Timelines

	Malawi Social Cash Transfer Program (SCTP)	Zambia Multiple Category Targeted Program (MCP)
Location	Two rural districts: Salima and Mangochi.	Two rural districts: Serenje and Luwingu
Selection of the units of randomization	*September 2012* Four traditional authorities (TAs) randomly selected (two in each district); 29 village clusters (VCs) randomly selected (15 in Salima, 14 in Mangochi).	*June 2010* 92 community welfare assistance committees (CWACs) randomly selected (46 in each district)
Household targeting	*November 2012–May 2013* Eligible households identified based on the following criteria: - Ultrapoverty (assessed using a proxy-means test); - Labor constraints, operationalized as a dependency ratio > 3 (see table note).	*January–September 2011* Eligible households identified based on the following criteria: - Female-headed households keeping orphans; - Households with a disabled member; - Elderly-headed households (over 60 years old) keeping orphans; - Special, critically vulnerable cases.
Baseline survey	*June–October 2013* Salima: all eligible households in each VC interviewed (126 on average); Mangochi: random sample of 125 eligible households in each VC. Total of 3,531 eligible households surveyed.	*November–December 2011* 33 eligible households randomly interviewed in each of the 96 CWACs. Total of 3,078 eligible household surveyed.
Assignment of communities to treatment or control	*November 2013* Public coin toss 14 treatment, 15 control VCs.	*December 2011* Public coin toss 46 treatment, 46 control CWACs.
First cash transfer in treatment communities	*December 2013* The monthly transfer was MWK1,000, 1,500, 1,950, and 2,400 for households of size 1, 2, 3, and 4+. And additional top-up of MWK300 (600) was provided for each resident below age 21 (30). Average monthly transfer was MWK2,571 (about USD3.7). Transfers were paid in cash every two months. The transfer represents approximately 18 percent of preprogram consumption.	*February 2012* Households received ZMW55 (about USD12) a month (increased to ZMW60 over the course of 2012 and to ZMW70 in 2013) irrespective of household size. Transfers were paid in cash every two months. The transfer represents approximately 28 percent of preprogram consumption.
Endline survey	*October–November 2015*	*November–December 2014*

Source: Seidenfeld, Prencipe, and Handa 2012; UNC-CH 2014, 2016; AIR 2016.
Note: The dependency ratio is defined as the number of household members, divided by the number of household members who are fit to work, that is, ages 19–64 and without any illnesses or disabilities or, if 19–25, not in school.

main objectives of the program (to reduce food insecurity and to support the well-being of children, including their schooling); the initial three-year duration of household participation in the program, after which the participation will be reassessed; and payment procedures and amounts. During the initial three years, benefit amounts are not altered even if there are changes in household composition.

Zambia's Multiple Category Targeted Program

Zambia's Ministry of Community Development, Mother, and Child Health has been experimenting with various models of targeted cash transfer programs since 2004. The Multiple Category Targeted Program (MCP) aimed to reduce hunger and support the health, nutrition, and schooling of children. It was introduced as a pilot program in 2011, reaching approximately 10,000 households. In 2015, the government

decided to consolidate the country's various cash transfer programs into a single unified program, and MCP households were retargeted for potential eligibility in the new unified program in late 2015. The policy change and retargeting did not affect the impact evaluation of the MCP that was completed in 2014.

The MCP targeted households falling into one of the following four categories: households headed by a female member and including orphans; households headed by an elderly member (ages more than 60) and including orphans; households with a disabled member; and households with other critical situations (such as households with two elderly members who are unable to support themselves economically). Households were selected by members of the community welfare assistance committees (CWACs), in collaboration with staff from the District Social Welfare Office. Specifically, a short targeting form was administered to all households in the pilot sites to capture information on demographic composition and family relationship status. Information retrieved from these forms was processed at the ministry in Lusaka to produce final eligibility lists, which were shared with the District Social Welfare Office for final approval. There is no poverty criterion for eligibility, but the pilot sites are so poor that 90 percent of eligible households fall below Zambia's extreme poverty line. Because of the categorical targeting procedure described above, expected program coverage rates are between 10 percent and 15 percent.

The program provided a flat transfer (that is, independent of the size of the beneficiary household) equal to 55 Zambian Kwacha (ZMW) (about USD12) a month, an amount considered sufficient at the time to provide one meal a day to each household member. The transfer was raised to ZMW60 in 2012 and ZMW70 in 2013 to account for inflation; it represented approximately 28 percent of beneficiary preprogram consumption. Transfers were paid in cash at local pay points every two months. Similar to the Malawi program, recipients were told during the enrollment process about the program objectives, rules, payment amounts and payment processes, and that eligibility was for an initial three years.

Evaluation Designs

In both countries, program expansion plans included the integration of cluster randomized evaluations.[9] (See table 1 for a summary of the evaluation setup.) In Malawi, 29 village clusters (VCs) in the districts of Mangochi and Salima were randomly assigned to either a treatment or a control (delayed treatment) group. The 29 study VCs were selected in two steps in September 2012. First, within each of the two districts, two smaller geographical areas known as Traditional Authorities (TAs) were randomly selected for the study. The random selection of TAs was carried out by lottery at a meeting of the Social Protection Technical Group, chaired by the ministry. Next, 15 VCs (of 24) in Salima and 14 VCs (of 21) in Mangochi were randomly selected from these TAs to be incorporated in the study. (The number was higher in Salima because the VCs there were smaller.) In each VC, household targeting for participation in the program was carried out between November 2012 and May 2013. The baseline survey was conducted shortly thereafter, between June and October 2013. In Salima, all eligible households in each VC were interviewed. In Mangochi, where the VCs were larger, study households were randomly sampled from the eligibility lists. In November 2013, shortly after completion of the baseline survey (see below), VCs were randomly assigned to the treatment or control groups by coin toss held publicly in the district commissioners office in each district, resulting in a total of 14 VCs entering the program immediately.[10] The remaining VCs were told that they would eventually enter the program, the exact date to be determined. Households in

9 The evaluation of Malawi's SCTP was implemented by the University of North Carolina at Chapel Hill and the Center for Social Research of the University of Malawi, with the technical support of the UNICEF Office of Research-Innocenti and the Food and Agriculture Organization of the United Nations. The evaluation of Zambia's MCP was conducted by the American Institutes for Research, with the technical support of UNICEF Office of Research-Innocenti.

10 VCs were randomly ordered in each district, and the coin toss determined whether the top half or the bottom half of the list would enter the program immediately. The public coin toss was important so that all TAs understood that they had an equal chance of entering the program early and could explain this to their constituents.

these VCs were not told their eligibility status until the VCs entered the program, which was after the study ended. The disbursement of transfers in treatment VCs commenced in December 2013.

Many features of the design of the impact evaluation in Zambia resemble those of the impact evaluation in Malawi. A total of 92 CWACs in the districts of Luwingu (Northern Province) and Serenje (Central Province) in Zambia were randomly allocated to a treatment and a control group. In June 2010, the Ministry of Community Development, Mother, and Child Health selected 46 CWACs per district by means of a public lottery involving district and provincial officials. From January to September 2011, households in these CWACs were targeted for participation in the program. More than 100 eligible households were identified per CWAC, and the study team randomly selected 33 of these households for incorporation in the impact evaluation based on the desired sample size derived from power calculations. The baseline survey was conducted between November and December 2011. Shortly after the finalization of the baseline survey, the ministry assigned an equal number of study CWACs to the program group or a control group by means of a public coin toss in each district, following a process identical to the one in Malawi. The first transfers were paid out in February 2012.

Data and Outcome Variables

Both evaluations rely on information collected through surveys administered to a knowledgeable household member.[11] In Malawi, baseline data were collected from 3,531 eligible households from June to October 2013. The same households were reinterviewed at endline, between October and November 2015. In Zambia, baseline data from 3,078 eligible households were collected from November to December 2011. Endline data were collected in the same period of 2014. (See table 1 for an overview of the evaluation timelines.) In both countries, the questionnaire module on child productive activities was particularly extensive at endline. The study uses this information to construct variables for (hours of) participation in economic activities in the seven days prior to the interview and in household chores in the day prior to the interview.[12]

In Malawi, the endline questionnaire captured four categories of economic activities carried out by children in the week prior to the endline survey: farm work for the household (excluding livestock), caring for livestock owned by the household, work in the nonfarm household business, and paid work outside the household (including formal wage work and informal piecework). And the endline questionnaire captured three categories of household chores carried out on the day prior to the endline survey: collecting water or firewood; taking care of children, cooking, or cleaning; and taking care of elderly or sick household members.[13]

In Zambia, it has been possible to construct mostly comparable outcome variables. However, there are two categories of productive activities for which no information was collected: farm work for the household and taking care of elderly or sick household members. In Zambia, the household questionnaire also collected data on individual participation and hours worked in any paid work and unpaid work (the latter including household chores) during the two weeks before the interview. This information was

11 Several additional data collection efforts were undertaken as part of each evaluation, but this study does not depend on them. These include midline quantitative data, direct interviews with adolescents and youth (Malawi only), community questionnaires, and embedded qualitative interviews.

12 In Malawi, the endline questionnaires were administered directly before the start of the lean season, while, in Zambia, they were administered during the lean season. Because the intensity and the type of economic activities vary across seasons, this study also explores indicators of child economic activities during the year before the survey (see section 4, robustness checks). The lean season is the period of the year when, for example, food supplies become scarcer because of the lull between harvest and planting or between planting and harvest.

13 Our classification partially deviates from ILO (2008), which classifies "collecting water or firewood" as an economic activity.

available both at baseline and endline, and can be used to establish baseline balance. The remaining outcome variables are described in the supplementary online appendix, table S1.1.

3 Empirical Strategy

This section supplies descriptive statistics of the sample; discusses the validity of the evaluations by exploring attrition, balance, and program take-up; and lays out the estimation strategy.

Sample and Descriptive Statistics

The regression sample is restricted to children on whom there is complete information at baseline and endline. In Malawi, the sample is further restricted to children ages 6–15 at baseline. The lower age limit is set at 6 because data on engagement in economic activities and household chores were collected from this age. The upper age limit is set at 15 to ensure that the individuals in the sample are minors at follow-up, two years after the baseline. In Zambia, the sample is restricted to children ages 5–14 at baseline. Because Zambia endline data were collected three years after the baseline, endline ages correspond with those in Malawi and range from 8 to 17 in both countries. The regression analysis consistently applies these restrictions so that each regression used to assess the impacts of the program on child activities is estimated on the same sample of children.[14] The baseline sample size is 6,733 and 4,816 for Malawi and Zambia, respectively. By endline, these samples had been reduced to 5,806 and 3,999 children, implying an average attrition rate among individuals of about 14 percent and 17 percent, respectively.

Tables 2 and 3 describe the baseline characteristics of the households and children in the samples in Malawi and in Zambia, respectively. Among households, tables 2 and 3 examine the determinants of program eligibility (panels a and b), the determinants of transfer size (panel c), and outcome variables (panel d). At the level of the child, the tables examine demographics (panel e) and outcome variables (panel f). In both tables 2 and 3, column (1) reports baseline mean values for the control group in the full sample of children (and related households) as observed at baseline. This information is used to describe the sample. Baseline balance and differential attrition are discussed in the next subsection.

Poverty and Food Consumption

The sample households are ultrapoor. The baseline daily per capita consumption of households eligible to benefit from the cash transfer programs under study was equal to approximately USD0.35 in both countries (not displayed).[15] Concomitant with low consumption, the clear majority of households in both countries occupied basic dwellings with floors and roofs made of natural materials (mostly sand, mud, or grass). It was not uncommon for household members to live on one meal a day (21 percent and 27 percent in Malawi and Zambia, respectively), and most households survived at least partly on assistance, such as cash or food aid (72 percent and 60 percent, respectively).

Credit Constraints

About 28 percent and 16 percent of the control households in Malawi and Zambia, respectively, had borrowed money in the year prior to the endline interview. (See supplementary online appendix, table S1.2 for more detail.) Households that borrowed money primarily did so to support consumption and health expenditures. Only 2 percent and 3 percent of households in Malawi and Zambia, respectively, had borrowed money to invest in the farm or nonfarm business, and, in both countries, only 2 percent of households had borrowed money to finance education expenditures. Households mostly borrowed from relatives, neighbors, and friends (67 percent and 84 percent of the control households, respectively).

14 Whenever this paper presents findings on households, the reference is to households with relevant sampled children.

15 For calculations, see Seidenfeld, Prencipe, and Handa (2012); and UNC-CH (2014).

Table 2. Baseline Balance and Differential Attrition, Malawi

	Full baseline sample		Attritor	Panel
	Control mean (S.D.) (1)	Difference T-C [*p*-value] (2)	Difference T-C [*p*-value] (3)	Difference T-C [*p*-value] (4)
Panel A: Determinants of program eligibility in Malawi				
Labor constrained (dependency ratio > 3)[a]	0.882	0.026	−0.060	0.034*
	(0.323)	[0.218]	[0.190]	[0.083]
Natural floor (sand or mud)	0.979	0.002	0.005	0.003
	(0.144)	[0.795]	[0.871]	[0.742]
Natural roof (grass or iron sheets)	0.989	−0.020	−0.045	−0.018
	(0.104)	[0.362]	[0.441]	[0.381]
Natural walls (grass or mud)	0.726	−0.012	0.148**	−0.026
	(0.446)	[0.794]	[0.036]	[0.570]
Unimproved toilet (latrine without roof or no toilet)	0.580	−0.006	−0.025	−0.004
	(0.494)	[0.880]	[0.756]	[0.925]
Owns any agricultural assets	0.898	0.001	−0.008	0.002
	(0.303)	[0.980]	[0.877]	[0.927]
Last year's harvested maize lasted < 4 months	0.525	0.009	0.000	0.010
	(0.500)	[0.805]	[0.998]	[0.792]
Household members have on average only one meal per day	0.207	0.018	0.040	0.016
	(0.405)	[0.576]	[0.401]	[0.615]
Received any assistance (cash, food or other aid), past 12 months	0.717	0.005	0.044	0.001
	(0.451)	[0.925]	[0.442]	[0.982]
Panel B: Determinants of program eligibility in Zambia				
Female headed household with orphans	0.213	0.028	0.020	0.028
	(0.410)	[0.390]	[0.709]	[0.405]
Household has a disabled member	0.540	0.065	0.01	0.069
	(0.499)	[0.134]	[0.907]	[0.120]
Elderly (>60 years old) headed household with orphans	0.141	0.024	0.039	0.021
	(0.348)	[0.396]	[0.477]	[0.465]
Panel C: Determinants of transfer size in Malawi				
Household size = 2	0.059	−0.001	−0.072	0.003
	(0.236)	[0.922]	[0.386]	[0.743]
Household size = 3	0.122	−0.001	−0.006	−0.002
	(0.327)	[0.945]	[0.915]	[0.920]
Household size ≥ 4	0.819	0.002	0.078	−0.002
	(0.385)	[0.927]	[0.282]	[0.937]
N individuals aged < 21	3.714	−0.041	0.029	−0.033
	(1.735)	[0.747]	[0.882]	[0.784]
N individuals aged 21–29	0.200	0.012	0.091	0.005
	(0.474)	[0.716]	[0.168]	[0.864]
Panel D: Household outcomes				
Owned or cultivated any land (past agricultural season)	0.969	0.003	0.059*	−0.002
	(0.173)	[0.800]	[0.070]	[0.886]
Total area of land owned or cultivated (ha, log)	0.785	−0.002	0.025	−0.004
	(0.369)	[0.964]	[0.565]	[0.934]
Sold any crop (past agricultural season)	0.201	0.008	−0.003	0.008
	(0.401)	[0.830]	[0.967]	[0.809]
Hired any person to work on land (past agricultural season)	0.027	0.016*	0.034	0.014
	(0.164)	[0.083]	[0.131]	[0.147]
Owns any livestock[b]	0.242	0.009	0.090*	0.002
	(0.428)	[0.801]	[0.096]	[0.938]

Table 2. (Continued)

	Full baseline sample		Attritor	Panel
	Control mean (S.D.) (1)	Difference T-C [*p*-value] (2)	Difference T-C [*p*-value] (3)	Difference T-C [*p*-value] (4)
Sold any livestock (past 12 months)	0.072	0.020	0.082**	0.015
	(0.259)	[0.162]	[0.031]	[0.325]
Operated any nonfarm business (past 12 months)	0.240	0.052	0.027	0.054
	(0.427)	[0.238]	[0.654]	[0.239]
Monthly profit of nonfarm business (constant 2011 USD)	2.616	0.952	0.054	1.021
	(14.750)	[0.230]	[0.980]	[0.213]
N (households)	1,456	2,759	219	2,540
Panel E: Child demographics				
Age	10.197	0.172**	0.109	0.159*
	(2.771)	[0.047]	[0.661]	[0.082]
Female	0.490	0.001	0.037	−0.006
	(0.500)	[0.912]	[0.228]	[0.660]
Panel F: Child outcomes				
Attends school	0.729	−0.012	0.045	−0.02
	(0.445)	[0.662]	[0.306]	[0.492]
Any education expenditure	0.650	0.003	0.062*	−0.006
	(0.477)	[0.903]	[0.091]	[0.824]
Highest grade completed	2.541	−0.141	0.128	−0.190
	(2.975)	[0.398]	[0.511]	[0.289]
Engaged in:				
Livestock herding for the household (past week)	0.013	0.002	0.007	0.002
	(0.111)	[0.598]	[0.323]	[0.755]
Household nonagricultural business (past week)	0.049	−0.008	−0.022	−0.006
	(0.215)	[0.766]	[0.745]	[0.770]
Paid work outside the household (past week in Malawi, past 2 weeks in Zambia)	0.086	−0.001	−0.048**	0.006
	(0.280)	[0.933]	[0.016]	[0.615]
Collecting water or firewood (past day)	0.434	0.008	−0.012	0.009
	(0.496)	[0.719]	[0.721]	[0.686]
Taking care of children, cooking or cleaning (past day)	0.353	0.007	−0.015	0.008
	(0.478)	[0.837]	[0.718]	[0.815]
Unpaid work and chores (past 2 weeks)	N.A.			
Joint test of orthogonality		[0.214]	[0.636]	[0.490]
Panel G: Child attrition				
Attrited	0.128	0.019		
	(0.334)	[0.128]		
N (children)		6,733	927	5,806
N (children, treatment village clusters)		3,146	468	2,678

Source: Malawi Economic, Health and Demographic Survey (MEHDS).

Notes: a. The dependency ratio is defined as the number of household members divided by the number of household members who are "fit to work" (aged 19–64 years without any illnesses or disabilities and, if 19–25, not in school). b. Outcome variable is a determinant of eligibility in Malawi. The sample includes children aged 6–15 years at baseline (8–17 years at endline). Column (1) reports standard deviations in parentheses. Regressions in columns (2) to (4) include fixed effects for the stratification variable (traditional authority). *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (village clusters). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3. Baseline Balance and Differential Attrition, Zambia

	Full baseline sample		Attritor	Panel
	Control mean (S.D.) (1)	Difference T-C [*p*-value] (2)	Difference T-C [*p*-value] (3)	Difference T-C [*p*-value] (4)
Panel A: Determinants of program eligibility in Malawi				
Labor constrained (dependency ratio > 3)[a]	0.713	0.024	0.067	0.021
	(0.453)	[0.266]	[0.322]	[0.302]
Natural floor (sand or mud)	0.943	0.007	0.031	0.005
	(0.233)	[0.562]	[0.352]	[0.700]
Natural roof (grass or iron sheets)	0.981	−0.002	0.019	−0.004
	(0.137)	[0.721]	[0.513]	[0.517]
Natural walls (grass or mud)	0.536	0.035	0.032	0.036
	(0.499)	[0.483]	[0.720]	[0.463]
Unimproved toilet (latrine without roof or no toilet)	0.989	−0.003	−0.024*	−0.001
	(0.107)	[0.517]	[0.075]	[0.839]
Owns any agricultural assets	0.918	−0.003	−0.005	−0.002
	(0.275)	[0.873]	[0.877]	[0.883]
Last year's harvested maize lasted < 4 months	N.A.			
Household members have on average only one meal per day	0.271	0.025	−0.052	0.035
	(0.445)	[0.475]	[0.422]	[0.302]
Received any assistance (cash, food or other aid), past 12 months	0.601	−0.07	−0.019	−0.076
	(0.490)	[0.156]	[0.823]	[0.124]
Panel B: Determinants of program eligibility in Zambia				
Female headed household with orphans	0.572	0.013	0.064	0.007
	(0.495)	[0.687]	[0.376]	[0.835]
Household has a disabled member	0.259	0.045**	−0.065	0.059***
	(0.438)	[0.033]	[0.225]	[0.007]
Elderly (>60 years old) headed household with orphans	0.294	0.008	−0.057	0.016
	(0.456)	[0.806]	[0.431]	[0.651]
Panel C: Determinants of transfer size in Malawi				
Household size = 2	0.041	−0.012	−0.062**	−0.007
	(0.199)	[0.132]	[0.038]	[0.427]
Household size = 3	0.107	0.019	0.053	0.014
	(0.309)	[0.282]	[0.303]	[0.451]
Household size ≥ 4	0.852	−0.007	0.008	−0.007
	(0.356)	[0.760]	[0.890]	[0.745]
N individuals aged < 21	3.904	0.053	0.262	0.036
	(1.971)	[0.729]	[0.383]	[0.808]
N individuals aged 21–29	0.448	−0.029	−0.061	−0.028
	(0.737)	[0.554]	[0.624]	[0.560]
Panel D: Household outcomes				
Owned or cultivated any land (past agricultural season)	0.919	−0.012	−0.073**	−0.005
	(0.273)	[0.393]	[0.024]	[0.746]
Total area of land owned or cultivated (ha, log)	0.305	−0.012	0.005	−0.015
	(0.205)	[0.354]	[0.788]	[0.282]
Sold any crop (past agricultural season)	0.479	−0.065*	0.062	−0.081**
	(0.500)	[0.095]	[0.430]	[0.036]
Hired any person to work on land (past agricultural season)	0.029	0.003	−0.006	0.004
	(0.167)	[0.839]	[0.841]	[0.764]
Owns any livestock[b]	0.578	−0.090**	−0.075	−0.093**
	(0.494)	[0.031]	[0.298]	[0.030]

Table 3. (Continued)

	Full baseline sample		Attritor	Panel
	Control mean (S.D.) (1)	Difference T-C [*p*-value] (2)	Difference T-C [*p*-value] (3)	Difference T-C [*p*-value] (4)
Sold any livestock (past 12 months)	0.190	−0.019	−0.07	−0.013
	(0.393)	[0.462]	[0.230]	[0.612]
Operated any nonfarm business (past 12 months)	N.A.			
Monthly profit of nonfarm business (constant 2011 USD)	N.A.			
N (households)	1,045	2,153	236	1,917
Panel E: Child demographics				
Age	9.846	−0.238**	−0.232	−0.248**
	(2.784)	[0.010]	[0.329]	[0.008]
Female	0.479	0.013	0.026	0.011
	(0.500)	[0.355]	[0.553]	[0.482]
Panel F: Child outcomes				
Attends school	0.661	−0.057**	−0.084	−0.051*
	(0.473)	[0.029]	[0.105]	[0.051]
Any education expenditure	0.565	−0.045	−0.070	−0.039
	(0.496)	[0.209]	[0.291]	[0.268]
Highest grade completed	1.837	−0.303***	−0.366*	−0.297***
	(1.986)	[0.004]	[0.076]	[0.008]
Engaged in:				
Livestock herding for the household (past week)	N.A.			
Household nonagricultural business (past week)	N.A.			
Paid work outside the household (past week in Malawi, past 2 weeks in Zambia)	0.014	−0.003	0.006	−0.005
	(0.117)	[0.511]	[0.521]	[0.270]
Collecting water or firewood (past day)	N.A.			
Taking care of children, cooking or cleaning (past day)	N.A.			
Unpaid work and chores (past 2 weeks)	0.682	−0.028	−0.060	−0.021
	(0.466)	[0.401]	[0.236]	[0.523]
Joint test of orthogonality				
		[0.256]	[0.913]	[0.256]
Panel G: Child attrition				
Attrited	0.156	0.027		
	(0.363)	[0.166]		
N (children)		4,816	817	3,999
N (children, treatment community welfare assistance committees)		2,511	455	2,053

Source: Zambia Multiple Category Targeted Program (MCP).

Note: a. The dependency ratio is defined as the number of household members divided by the number of household members who are "fit to work" (aged 19–64 years without any illnesses or disabilities and, if 19–25, not in school). b. Outcome variable is a determinant of eligibility in Malawi. The sample includes children aged 5–14 years at baseline (8–17 years at endline). Column (1) reports standard deviations in parentheses. Regressions in columns (2) to (4) include fixed effects for the stratification variable (district). *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (community welfare assistance committees). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Livelihoods

A large majority of households in both countries owned or cultivated land (97 percent and 92 percent, respectively). However, the area of land owned or cultivated was small (respectively, an average of about 2 and 1 hectares in Malawi and Zambia). Among more than half the households in Malawi, the harvested maize—the staple crop in Malawi—lasted for less than four months (not measured in Zambia). Livestock ownership was less common in Malawi than in Zambia (24 percent vs. 58 percent). It was

also less common for households in Malawi to sell livestock (7 percent vs. 19 percent). In Malawi, about 24 percent of households ran nonagricultural enterprises (not measured in Zambia).[16] The monthly profits of these enterprises were low: an average of about USD11 a month for households reporting that they ran nonagricultural enterprises.

Household Composition

Most households (88 percent and 71 percent in Malawi and Zambia, respectively) were labor constrained (see section 2). In both countries, the majority of households each had nearly four members below the age of 21. Possibly because of differences in targeting criteria across the two countries, there were dissimilarities in household composition between the two countries. In Malawi, over half the households had one or more disabled members. In Zambia, this share was lower, at 26 percent. In Malawi, respectively, 21 percent and 14 percent of households hosted orphans and were headed by a female or by an elderly person. In Zambia, the latter two categories (female or elderly headed) were more common, at 57 percent and 29 percent.

Children

In both countries, children were about 10 years old on average at baseline. About 73 percent and 66 percent of children in Malawi and Zambia, respectively, were in school. Children had completed about 2.5 and 1.8 grades of education, on average. Although the baseline indicators are somewhat limited, the available information suggests that it was quite common for children in both countries to engage in economic activities and household chores. In Malawi, 1 percent of children cared for livestock; 5 percent supported a household nonagricultural business; and 9 percent engaged in economic activities for pay outside the household. About 43 percent and 35 percent of children collected water or firewood or helped by taking care of children, cooking, or cleaning. In Zambia, about 1 percent engaged in economic activities for pay outside the household, and 68 percent carried out unpaid economic activities and household chores.

Attrition and Balance

Tables 2 and 3 examine whether the attrition rate among children was affected by the programs by regressing the indicator for attrition on the indicator for living in a treated village and on the stratification variables, that is, the four TAs in Malawi and the two districts in Zambia. As shown in panel g, the attrition rate in the treatment group exceeds that of the control group by about 2 and 3 percentage points in Malawi and Zambia, respectively, but this difference is not statistically significant.

Tables 2 and 3 examine balance in the baseline characteristics of all households and children observed at baseline (column (2)), households and children that could not be reobserved at endline (the attritor sample, column (3)), and households that could be reobserved (the panel sample, column (4)). There are generally few differences observed in the baseline characteristics of households in the control and treatment clusters. In both countries, there is a small but statistically significant difference in child age. In Malawi, children in the treatment group are slightly older than those in the control group, and vice versa in Zambia. There is no clear explanation for this difference. The impact analysis presented in this paper controls for age fixed effects, thereby limiting concerns that age differences may drive impacts on child work.

The most salient and important violation of baseline balance occurs in the case of Zambia, where children in treatment clusters had significantly worse educational outcomes than children in control clusters. In the full baseline sample and in the panel sample, they were about 6 percentage points less likely

16 Reported businesses at baseline were mainly petty trading (46 percent), selling charcoal or firewood (29 percent), and selling craft goods (14 percent). Other, less common businesses included home brewing, taxi and transportation services, and hospitality (bars and restaurants).

to attend school and had completed about 0.3 fewer school grades (table 3, column (2), panel f). This finding appears to be a statistical coincidence because the experiment in Zambia was balanced on most other domains captured at baseline (see also Seidenfeld, Prencipe, and Handa 2012). Work for pay within Malawi's attritor sample is not balanced either (table 2, column (3), panel f). The joint test of orthogonality does not reject the null hypothesis that the baseline variables are jointly orthogonal to the treatment, providing support to the internal validity of the estimates (tables 2 and 3, panel f).

The baseline characteristics of children in the panel sample differ from those of attritors in a few respects (see supplementary online appendix, table S1.3). For example, in both countries, panel children were significantly younger than children who attrited, presumably because older children are more likely to move out of the household. The findings represent program impacts only on the sample of children that could be observed at endline, and impacts may not be identical among children who could not be tracked.

Program Take-Up

In Malawi, nearly all households (99 percent) in the treatment clusters reported that they were program beneficiaries (online appendix, table S1.4). Among these households, 96 percent had received transfer payments in the three months prior to the endline interview. In the control clusters, effectively none of the households reported that they had received transfer payments.[17] In Zambia, the share of households reporting at endline that they were beneficiaries was lower, at 75 percent. Among these households, 95 percent had received transfer payments in the three months prior to the endline interview.[18] The share of households reporting program receipt in control clusters was 2 percent.

In Malawi, beneficiary households were asked how they spend the transfers. They indicated spending the transfers on immediate priorities such as food, clothing, and health care, but also commonly reported investing in education (56 percent), livestock (31 percent), or agricultural inputs (23 percent) (supplementary online appendix, table S1.4).[19]

Regression Specification

Because many of the outcomes of interest are only measured at endline, the study mainly relies on cross-sectional regressions estimated based on data from the endline surveys. The robustness of the results is tested using a variety of alternative specifications, as described below.

The main cross-sectional specification is as follows:

$$Outcome_{iv} = \beta_1 + \beta_2 T_v + \boldsymbol{\beta_3}' \boldsymbol{X_{iv}} + \varepsilon_{iv}, \quad (1)$$

where $Outcome_{iv}$ represents the outcome variable for household (or individual) i living in cluster v. The term T_v is a dummy variable equal to 1 if the cluster in which the household or individual is located received the transfer program. $\boldsymbol{X_{iv}}$ is a vector comprised of a small set of baseline control variables, including fixed effects for the stratification variables and fixed effects for the enumerators who carried out the interviews.[20] For individual-level regressions, the control variables also include gender and age fixed

17 At endline, the government of Malawi had enrolled 95 percent of households in control clusters as part of the expansion of the program. However, these households had not yet started receiving transfer payments.

18 If reported household beneficiary status one year earlier (measured as part of a midline survey) is taken into account, this share rises to 90 percent. These self-reported figures are slightly at odds with information from the ministry. While no household refused the offer of the transfer at the launch of the program, some child-headed households graduated from the program, and some households no longer fulfilled other demographic eligibility criteria. However, ministry records suggest that this figure is closer to 10 percent than to the 25 percent calculated on the basis of self-reports.

19 In Zambia, information on how households spent the transfers was collected only at midline, showing the most substantial increases in the same expenditure categories.

20 Enumerator fixed effects improve the precision of the estimates, particularly in Zambia, without substantive implications for the point estimates.

effects. For Malawi, there are also controls for the determinants of transfer size as measured at baseline (table 2, panel c). ε_{iv} is the error term.

In all regressions, standard errors are clustered at the unit of randomization (village cluster in Malawi; the CWAC in Zambia). To account for the low number of village clusters in Malawi, p-values are estimated using the wild bootstrap method (Cameron, Gelbach, and Miller 2008).[21] The coefficient of interest is β_2, which represents the impact of the transfer on household and individual outcomes. Given nonuniversal take-up, the estimated impacts in Zambia should be interpreted as intent-to-treat effects. In Malawi, where take-up was effectively universal, the intent-to-treat effect is equal to the average effect of treatment on the treated.

4 Results

This section examines the effects of the programs on household entrepreneurial activities, child work, and child well-being.

Household Entrepreneurial Activities

Consistent with Handa et al. (2018a), the study finds that the two programs enabled households to expand their agricultural enterprises substantially. Beneficiary households were 2 and 9 percentage points more likely to own or cultivate land in Malawi and Zambia, respectively (table 4). The area of land owned and cultivated increased by approximately 9 and 10 percent. The probability that households sold crops rose by 11 and 16 percentage points. The impacts on livestock ownership and sales were even more pronounced. In Malawi, the probability that households owned and sold livestock both more than doubled because of the program (an increase of 34 and 8 percentage points, respectively). In Zambia, the ownership and sale of livestock increased by 27 and 7 percentage points, respectively.[22] In Malawi, there was also an increase in the likelihood that households run a nonagricultural business (12 percentage points). Monthly profits from these businesses rose by about USD4 for the average household, indicative of a markedly larger increase among households with active enterprises. In Zambia, the point estimate for household engagement in nonagricultural businesses, while positive, is smaller and not statistically significant. A possible explanation is that study sites in Zambia were more isolated than those in Malawi, with fewer opportunities for engagement in nonagricultural businesses. (Zambian households in the control group were significantly less likely to be operating a nonagricultural business than those in Malawi, 9 percent vs. 21 percent.)

In both countries, adults increased their engagement in household enterprises accordingly, while simultaneously lowering their engagement in work for pay outside the household (table 5). In Malawi (Zambia), adults increased their participation in nonlivestock agricultural activities by 12 (5) percentage points, livestock herding by 13 (3) percentage points, and work in nonagricultural businesses by 3 (3) percentage points. Paid work outside the household decreased by 12 (6) percentage points. As a result, program impacts on the net engagement of adults in economic activities were limited in both countries.

21 After regression specification (1) is estimated with standard errors clustered at the unit of randomization, the Stata boottest command is applied on the null hypothesis of zero treatment effect (default parameters: 999 replications, Rademacher weights).

22 Ownership of livestock may be not only a productive investment, but also a way for households to store value in the absence of access to savings instruments. Although impacts on livestock are large at the extensive margin, these impacts do not necessarily reflect drastic increases at the intensive margin. For instance, in Malawi and Zambia, the average number of chickens owned by households in the sample rose by 1 and 2, respectively. The average number of goats and sheep owned increased by 0.5 and 0.6, respectively. Impacts on other types of livestock are more limited. (The results are available on request.)

Table 4. Impact on Household Productive Activities

	Malawi		Zambia	
	Control mean (1)	Impact (2)	Control mean (3)	Impact (4)
Owned or cultivated any land (past agricultural season)	0.973	0.019***	0.878	0.088***
		[0.000]		[0.000]
Total area of land owned or cultivated (ha. in logs)	0.782	0.091**	0.347	0.098***
		[0.029]		[0.000]
Sold any crop (past agricultural season)	0.201	0.107***	0.350	0.160***
		[0.001]		[0.000]
Hired any person to work on land (past agricultural season)	0.018	0.048***	0.053	0.180***
		[0.000]		[0.000]
Owns any livestock	0.306	0.339***	0.448	0.269***
		[0.000]		[0.000]
Sold any livestock (past 12 months)	0.058	0.076***	0.096	0.069***
		[0.000]		[0.000]
Operated any nonfarm business (past 12 months)	0.205	0.115***	0.089	0.022
		[0.006]		[0.382]
Monthly profit of nonfarm business (constant 2011 USD)	2.515	3.651***	1.332	1.129
		[0.000]		[0.103]
N		2,540		1,917

Source: Malawi Economic, Health and Demographic Survey (MEHDS), Zambia Multiple Category Targeted Program (MCP).
Note: The sample includes households with relevant sampled children. Results are obtained using the endline data and regression specification (1), including fixed effects for stratification variable (Traditional Authority for Malawi, district for Zambia) and enumerator. For Malawi, regressions also include controls for household composition (dummy variables for whether the household includes 3 members, 4 or more members, number of members below 21 years of age, number of members between 21 and 29 years of age). *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (village cluster for Malawi, Community Welfare Assistance Committee for Zambia). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The point estimates for engagement in economic activities are 2 and 3 percentage points in Malawi and Zambia, respectively; only the latter was statistically significant. These findings could imply that adults face heavy demands on their time also in the absence of the program and cannot substantially expand their labor supply. In Malawi, adults also boosted their engagement in household chores (not measured in Zambia). Some of these activities, such as collecting water and firewood (which increased by 5 percentage points), may play a role in the expansion of the household enterprise.

Households hired more labor to support the expansion of household entrepreneurial activities. During the agricultural season before the endline interview, beneficiary households in Malawi and Zambia were, respectively, 5 and 18 percentage points more likely to hire help to work on the household farm, that is, hired labor roughly quadrupled relative to the control group (table 4). The data do not allow to establish whether labor market imperfections prevented households from hiring even more labor.

The findings favor the interpretation that, because of poverty and credit constraints, households were limited in their capacity to invest in household enterprises. The introduction of the cash transfer programs enabled households to boost their productive investment. Handa et al. (2018b) study the effects of the MCP and another cash transfer program in Zambia on long-term living standards, including productive activities, in more detail. They show that these investments resulted in substantial multiplier effects, allowing households to raise their expenditure and investments beyond the value of the transfers, which helps explain the drastic expansion in household productive activities. Households accommodated the expansion of the household enterprise by reallocating adult labor and recruiting on local labor markets. Children also increased their work in the household enterprise.

Table 5. Impact on Adult Participation in Economic Activities and Household Chores

	Malawi		Zambia	
	Control mean (1)	Impact (2)	Control mean (3)	Impact (4)
Any economic activities	0.673	0.022	0.909	0.028**
		[0.411]		[0.025]
Agricultural work for the household (excluding livestock)	0.373	0.120***	0.710	0.051***
		[0.000]		[0.000]
Livestock herding for the household	0.088	0.125***	0.033	0.027**
		[0.000]		[0.014]
Household nonagricultural business	0.072	0.034**	0.015	0.033**
		[0.071]		[0.044]
Paid work outside the household	0.477	−0.119***	0.321	−0.064**
		[0.001]		[0.014]
Any chores	0.720	0.040**	N.A.	
		[0.010]		
Collecting water or firewood	0.622	0.052**	N.A.	
		[0.010]		
Taking care of children, cooking or cleaning	0.638	0.032**	N.A.	
		[0.030]		
Taking care of elderly or sick household members	0.150	−0.006	N.A.	
		[0.713]		
Any economic activities or chores	0.865	0.018	N.A.	
		[0.183]		
N		3,064		3,381

Source: Malawi Economic, Health and Demographic Survey (MEHDS), Zambia Multiple Category Targeted Program (MCP).

Note: Economic activities refer to the week and the year before the interview, in Malawi and Zambia respectively. The sample includes individuals ages 16–62 and 15–61 years at baseline, in Malawi and Zambia respectively (18–64 years at endline in both countries). Results are obtained using the endline data and regression specification (1), including fixed effects for gender, age, stratification variable (traditional authority for Malawi, district for Zambia) and enumerator. For Malawi, regressions also include controls for household composition (dummy variables for whether the household includes 3 members, 4 or more members, number of members below 21 years of age, number of members between 21 and 29 years of age). *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (village cluster for Malawi, Community Welfare Assistance Committee for Zambia). $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Children's Productive Activities

Child engagement in nonlivestock agricultural activities rose by 6 percentage points in Malawi (not measured in Zambia) (table 6). Caring for livestock more than doubled, increasing by 7 and 4 percentage points in Malawi and Zambia, respectively. Support among children for household nonagricultural enterprises was unaffected in Malawi, but expanded by 3 percentage points in Zambia. In Malawi, mirroring the findings on adults, children shifted away from economic activity for pay outside the household (a reduction of 6 percentage points). In Zambia, where children were less likely to engage in economic activities for pay to begin with, no such shift was observed. Accordingly, the probability that children were engaging in any economic activities did not change significantly in Malawi (point estimate of 3 percentage points), while it increased significantly, by 6 percentage points, in Zambia.[23]

23 Supplementary online appendix, table S1.5 shows the impacts on the hours worked by children. The findings are qualitatively similar to those at the extensive margin and confirm that children augmented their engagement in work in household enterprises. In Malawi, this increase was offset by the reduction in hours worked outside the household for pay, leaving net working hours unaffected. In Zambia, engagement in economic activities was up by over 20 minutes a week, a rise of about 45 percent.

Table 6. Impact on Child Participation in Economic Activities and Household Chores

	Malawi		Zambia	
	Control mean (1)	Impact (2)	Control mean (3)	Impact (4)
Any economic activities	0.302	0.034	0.180	0.055**
		[0.133]		[0.014]
Agricultural work for the household (excluding livestock)	0.144	0.063**	N.A.	
		[0.024]		
Livestock herding for the household	0.038	0.068***	0.029	0.039***
		[0.000]		[0.007]
Household nonagricultural business	0.019	0.002	0.139	0.034*
		[0.715]		[0.056]
Paid work outside the household	0.188	−0.061***	0.045	0.001
		[0.001]		[0.889]
Any chores	0.651	0.097***	0.711	0.028
		[0.000]		[0.105]
Collecting water or firewood	0.581	0.095***	0.677	0.034*
		[0.000]		[0.066]
Taking care of children, cooking or cleaning	0.424	0.084***	0.331	0.005
		[0.000]		[0.798]
Taking care of elderly or sick household members	0.099	0.019	N.A.	
		[0.178]		
Any economic activities or chores	0.715	0.091***	0.742	0.031*
		[0.000]		[0.081]
N		5,806		3,999

Source: Malawi Economic, Health and Demographic Survey (MEHDS), Zambia Multiple Category Targeted Program (MCP).
Note: The sample includes children aged 6–15 and 5–14 years at baseline, in Malawi and Zambia respectively (8–17 years at endline in both countries). Results are obtained using the endline data and regression specification (1), including fixed effects for gender, age, stratification variable (traditional authority for Malawi, district for Zambia) and enumerator. For Malawi, regressions also include controls for household composition (dummy variables for whether the household includes 3 members, 4 or more members, number of members below 21 years of age, number of members between 21 and 29 years of age). *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (village cluster for Malawi, Community Welfare Assistance Committee for Zambia). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Both programs also increased child engagement in household chores. Children in Malawi and Zambia were, respectively, 10 and 3 percentage points more likely to collect water or firewood. And, in Malawi, children were 8 percentage points more likely to care for other children, cook, and clean. Some of these household chores may contribute directly to the household enterprise; others may do so indirectly, by relieving adults of some duties.[24]

24 Endline data for the control group show clear gender patterns in the activities carried out by boys and girls in both countries (supplementary online appendix, table S1.6). Boys were significantly less likely to engage in household chores and significantly more likely to engage in economic activities. Gender differences were bigger in Malawi than in Zambia. The study examined whether program impacts on child activities differed among boys and girls. In Malawi, some of the increases in engagement in household agricultural enterprises and household chores were significantly weaker among girls than among boys. In Zambia, where gender differences in child activities are smaller, statistically significant heterogeneity is not observed in program impacts by gender.

Table 7. Robustness Checks

	Malawi		Zambia	
	Attrition reweighting (1)	Randomization inference (2)	Attrition reweighting (3)	Randomization inference (4)
Any economic activities	0.035	0.034	0.056**	0.055**
	[0.127]	[0.197]	[0.016]	[0.020]
Agricultural work for the household (excluding livestock)	0.065**	0.063**	N.A.	
	[0.020]	[0.019]		
Livestock herding for the household	0.068***	0.068***	0.038***	0.039***
	[0.000]	[0.000]	[0.007]	[0.007]
Household nonagricultural business	0.001	0.002	0.034*	0.034*
	[0.781]	[0.836]	[0.054]	[0.088]
Paid work outside the household	−0.061***	−0.061**	0.003	0.001
	[0.001]	[0.024]	[0.806]	[0.907]
Any chores	0.095***	0.097***	0.026	0.028
	[0.000]	[0.000]	[0.119]	[0.152]
Collecting water or firewood	0.093***	0.095***	0.033*	0.034*
	[0.000]	[0.000]	[0.072]	[0.094]
Taking care of children, cooking or cleaning	0.083***	0.084***	0.002	0.005
	[0.000]	[0.001]	[0.901]	[0.761]
Taking care of elderly or sick household members	0.016	0.019	N.A.	
	[0.229]	[0.173]		
Any economic activities or chores	0.090***	0.091***	0.030*	0.031
	[0.000]	[0.000]	[0.092]	[0.107]
N	5,806	5,806	3,999	3,999

Source: Malawi Economic, Health and Demographic Survey (MEHDS), Zambia Multiple Category Targeted Program (MCP).

Note: In columns (1) and (3), the study uses the endline data and regression specification (1), adding inverse probability weights. *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (village cluster for Malawi, Community Welfare Assistance Committee for Zambia). In columns (2) and (4), the study uses randomization inference: the study randomly reassigned treatment status (at the original level of randomization) and estimated the impact of this placebo treatment using specification (1), clustering standard errors at the unit of randomization level. This procedure is repeated 1,000 times to obtain *p*-values, which equal the proportion of reestimated impacts that are larger in absolute value than the original estimated impact (implemented applying the Stata command "ritest" by Heß 2017). The sample includes children aged 6–15 and 5–14 years at baseline, in Malawi and Zambia respectively (8–17 years at endline in both countries). Regressions include fixed effects for gender, age, stratification variable (traditional authority for Malawi, district for Zambia) and enumerator. For Malawi, regressions also include controls for household composition (dummy variables for whether the household includes 3 members, 4 or more members, number of members below 21 years of age, number of members between 21 and 29 years of age). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Robustness Checks

This section explores robustness to using attrition corrections and randomization-based inference.[25] It first examines attrition corrections. The findings are robust to correcting for attrition using inverse probability weights (Fitzgerald, Gottschalk, and Moffitt 1998). The predicted probability that children are observed at follow-up was obtained based on equation (1), but with the term $\boldsymbol{X}$ augmented to include all the covariates in table 2. The weights are obtained as the inverse of this probability. The weighted point estimates and significance levels reported in table 7, columns (1) and (3) are effectively unchanged relative to the original estimates displayed in table 6.

25 The study also explored robustness to using alternative outcome measures: productive activities carried out in the 12 months before the endline interview (as opposed to 7 days) and, in Malawi, self-reported engagement in economic activities (as opposed to proxy-reported). These alternative outcome measures are not available for all productive activities and only for older children. The findings are consistent with an increase in children's work in the household agricultural enterprise. (The results are available on request.)

Supplementary online appendix, table S1.7 shows Horowitz-Manski bounds (Horowitz and Manski 1998). Lower and upper bounds are obtained by estimating regression specification (1) on the full sample of children present at baseline, imputing outcome values for attritors according to the two most extreme possible scenarios. The most extreme scenarios correspond to assigning the values 0 to all attritors from the treatment group and 1 to all attritors from the control group or vice versa. The Horowitz-Manski bounds are wide and comprise zero for all outcome variables. Hence, strong differences in the outcomes for attritors by treatment status could render the estimates invalid.

The results are robust to bounding treatment effects in the spirit of Lee (2009). The control panel sample, which had a lower attrition rate, was trimmed so that the attrition rate equals the rate in the treatment group.[26] To obtain the upper bound of the treatment effect, the control group was trimmed by randomly removing children with outcome variables equal to 1. To obtain the lower bound, the trim involved randomly removing children with outcome variables equal to 0. Regression (1) was then reestimated on these trimmed samples. As expected given the small difference in attrition rates among treatment groups, these bounds are sufficiently narrow for most of the originally statistically significant program impacts to remain significant (supplementary online appendix, table S1.7). The only exceptions occur in the case of Zambia, in which the lower bounds for the collection of water and firewood and for participation in any economic activities or household chores have the same sign as the original estimate, but are no longer statistically significant.

The findings are also robust to using randomization inference (table 7, columns (2) and (4)). Following Fujiwara and Wantchekon (2013), treatment status at the original level of randomization is randomly reassigned, and the impact of this placebo treatment is estimated using specification (1). This procedure is repeated 1,000 times to obtain *p*-values, which equal the proportion of reestimated impacts that are larger in absolute value than the original estimated impact.[27]

Child Well-Being

This subsection examines whether the programs affected child engagement in productive activities that may have negative implications for child well-being. Those domains of child well-being are explored that may be affected by or simultaneously determined with these activities.

Excessive Working Hours

At low levels of intensity, child engagement in common economic activities and household chores may be innocuous or beneficial to children. However, long hours in these activities may have negative implications. The study therefore tested the program impacts on excessive working hours. Because the appropriate number of working hours depends on the age of the child, the analysis focuses on three age-groups recommended by the International Labour Organization (ILO 2008): under 12, 12–14, and 15–17. Within these age groups, ILO (2008) recommends, respectively, applying the following thresholds to establish excessive hours of economic activities: 1, 14, and 43 hours per week. For all age categories, ILO (2008) suggests a threshold of 28 hours for household chores.

In Malawi, children in the two older ILO age-groups (12–14 and 15–17) had increased their engagement in economic activities by about 5 to 6 percentage points by endline. (The point estimate for older children is not statistically significant; see table 8, panel a, columns (1)–(3)). Among children in the youngest ILO age bracket (under 12), the point estimate is small and not significant. Children in all age-groups increased their engagement in household chores, although, at 13 percentage points, this

26 In Malawi, 76 children were removed to equalize the attrition rate at 14.9 percent in the treatment and control groups. In Zambia, 57 children were removed to equalize the attrition rate at 18.1 percent.

27 This is implemented with the user-written ritest Stata command (Heß 2017).

Table 8. Impact, by Age-Group at Endline

	Malawi			Zambia		
	Impact < 12 (1)	Impact 12–14 (2)	Impact 15–17 (3)	Impact < 12 (4)	Impact 12–14 (5)	Impact 15–17 (6)
Panel A: Economic activities and household chores, extensive margin						
Any economic activities	0.018	0.047**	0.055	0.049*	0.058**	0.063**
	[0.389]	[0.034]	[0.227]	[0.086]	[0.045]	[0.023]
Any chores	0.126***	0.087***	0.071**	0.025	0.006	0.045*
	[0.000]	[0.000]	[0.031]	[0.330]	[0.799]	[0.077]
Any economic activities or chores	0.122***	0.074***	0.069**	0.036	0.01	0.038
	[0.000]	[0.000]	[0.016]	[0.154]	[0.697]	[0.122]
Panel B: Excessive hours						
Excessive hours in economic activities	0.018	−0.009	−0.004	0.049*	−0.007	0.005*
	[0.389]	[0.653]	[0.501]	[0.086]	[0.408]	[0.091]
Excessive hours in economic activities or chores	0.023	0.027	−0.027	0.056*	0.046*	0.055**
	[0.263]	[0.261]	[0.34]	[0.063]	[0.064]	[0.044]
Panel C: Education (difference-in-differences)						
Attends school	0.082*	0.075**	0.146***	0.029	0.071*	0.085**
	[0.061]	[0.018]	[0.000]	[0.409]	[0.086]	[0.027]
Attends school regularly[a]	0.128***	0.148***	0.190***	0.036	0.094*	0.103**
	[0.003]	[0.000]	[0.000]	[0.525]	[0.073]	[0.039]
Any education expenditure	0.103**	0.084**	0.115***	0.045	0.101**	0.107**
	[0.023]	[0.015]	[0.004]	[0.207]	[0.035]	[0.039]
Highest grade of education completed	0.405	0.249	0.136	−0.060	0.024	−0.002
	[0.350]	[0.133]	[0.568]	[0.539]	[0.822]	[0.990]
N (unique observations)	2,482	2,097	1,227	1,462	1,331	1,206
N (Attends school regularly, Zambia, unique observations)				1,333	1,210	1,101

Source: Malawi Economic, Health and Demographic Survey (MEHDS), Zambia Multiple Category Targeted Program (MCP).

Note: a. Regular school attendance is defined as follows. Malawi: the child did not miss more than two consecutive weeks of school during the 12 months before the interview. Zambia: the child attended five days of school during the week before the interview (lower number of observations, due to children in boarding schools or children who were on holidays during the week before the interview). In Panels A and B, regressions are obtained by using the endline data and estimating regression specification (1) on subsamples by age group. In Panel C, regressions are obtained by using both the baseline and the endline data and estimating a difference-in-differences specification, on subsamples by age group. Hence, the number of observations used for Panel C regressions is double the number displayed in the table. The sample includes children aged 6–15 and 5–14 years at baseline, in Malawi and Zambia respectively (8–17 years at endline in both countries). Regressions include fixed effects for gender, age, stratification variable (Traditional Authority for Malawi, district for Zambia) and enumerator. For Malawi, regressions also include controls for household composition (dummy variables for whether the household includes 3 members, 4 or more members, number of members below 21 years of age, number of members between 21 and 29 years of age). *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (village cluster for Malawi, Community Welfare Assistance Committee for Zambia). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

increase appears especially strong among younger children. In Zambia, children in all age-groups increased their engagement in economic activities by about 5 to 6 percentage points (table 8, panel a, columns (4)–(6)). Engagement in household chores rose only among older children, by about 5 percentage points.

In Malawi, there is no evidence that program receipt encourages child engagement in excessive hours performing economic activities and household chores (table 8, panel b). These findings (1) are explained by a shift away from work for pay outside the household, which leaves net engagement in economic activities in the younger age-group unaffected, and (2) suggest that children who engage in household chores in response to the program do so at modest intensity. In Zambia, excessive engagement in economic activities and household chores increases by about 5 to 6 percentage points in all age-groups. For younger children, this increase is driven primarily by greater engagement in economic activities, while, among the two older age-groups, the increase is driven by excessive engagement in household chores. However, cumulative

density functions suggest that changes in the hours doing household chores may not have been drastic for children who were pushed over the 28-hour threshold. (The results are available upon request.)

Education Outcomes

A key concern is that increased child engagement in economic activities, household chores, and, particularly, excessive hours may interfere with schooling. Therefore, table 8, panel c, examines program impacts on education. Because education outcomes were not balanced at baseline in the case of Zambia, difference in differences estimates are shown for the impacts on education. Program impacts on school attendance are positive, and the impacts are larger among older children, who are at higher risk of dropping out. In Malawi, school attendance rose by about 8 percentage points among the two lower age-groups and 15 percentage points among the older age-group. In Zambia, school attendance increased by 7 and 8 percentage points, respectively, among the older age-groups, while it did not increase significantly among the youngest age-group. In Malawi, there was a transfer top-up precisely to support school-age children, which could explain the larger impacts. Changes in education expenditures broadly correspond to the impacts on school attendance. As shown in table S1.8 of the supplementary online appendix, the expansion in school attendance is driven by a drop in the share of children who are out of school because of a lack of the ability to pay for education.

The results here are consistent with the review by Handa et al. (2018a), who show that six of the eight cash transfer programs in Sub-Saharan Africa (including the ones studied here) increased school enrollment among secondary-school-age children. The results are also consistent with the program impacts on schooling outcomes presented by Kilburn et al. (2017) on Malawi and Handa et al. (2018b) on Zambia. Kilburn et al. analyze the short-term impact of Malawi's SCTP on education outcomes, relying on the same experimental setup and using midline data collected one year after the start of the program. They find that the program raised school enrollment and reduced dropouts. An increase in household spending on education (uniforms and school supplies, in particular) is proposed as the key mechanism driving the observed impacts.[28]

The study here does not find that the effects of the transfers on the highest grade of education completed in either Malawi or Zambia were statistically significant.[29] There are multiple plausible explanations for the lack of impact on grade progression. One possibility is that the children who enter school because of the programs are disadvantaged students who do not progress so easily. Results from conditional cash transfer programs in Brazil, Colombia, and Mexico confirm that school achievement is lower among age-groups among which there are enrollment effects (Behrman, Sengupta, and Todd 2000; de Janvry et al. 2006; Garcia and Hill 2010). Beyond student selection, the study locations are extremely poor, and school quality is low; so simply sending children to school may not necessarily raise achievement in this context. A final possible explanation is that program-induced participation in (excessive hours of) economic activities and household chores slowed grade progression.

A dedicated, embedded qualitative data collection exercise on child labor was implemented in Malawi in June 2016, that is, about a half year post-endline. In-depth interviews and focus group discussions were held with youth, caregivers, and teachers in eight treatment villages. A full discussion of the qualitative findings can be found in Zietz, de Hoop, and Handa (2018). Because they confirm and help to

28 The findings of the study here are also consistent with Ravallion and Wodon (2000), who conclude that school subsidies boosted school attendance significantly in Bangladesh without a corresponding reduction in child labor, a result challenging the common view that child labor displaces schooling.

29 The impacts on the probability of completing primary education and on the probability of completing lower-secondary education were also examined. There is no evidence that these probabilities are significantly affected by the programs. Similarly, there are no statistically significant impacts on child logical reasoning in Malawi, as measured by a Raven's test. (The results are available upon request.)

contextualize and interpret the quantitative findings, the qualitative findings related to work and school attendance are briefly discussed here.

Caregivers, youth, and teachers all identified potential interference with school as the main negative aspect of child engagement in household chores and economic activities. All three groups of respondents discussed instances of children not attending school or reducing their effort in school because of obligations to provide for their households. Caregivers, however, generally indicated that child work is prioritized only in situations of severe and acute need. Moreover, caregivers generally perceived school attendance as important and indicated that prioritizing child work causes feelings of guilt. The latter may explain why, in the quantitative analysis, an increase in work on the household farm did not appear to deter school attendance.

Types of Economic Activities

For well-being, the implications of child engagement in productive activities depend not only on working hours, but also on the type of activities in which children engage. Children in Malawi shifted away from work for pay outside the household into work for the household enterprise. A decomposition in the main analysis sample reveals that the category "paid work outside the household" consists almost entirely (97 percent) of *ganyu* labor, that is, informal piecework at low pay. *Ganyu* is the option of last resort among the very poor. Indeed, other studies examining the effects of the SCTP on mental well-being suggest that a reduction in *ganyu* is associated with a significant reduction in indications of depression among adolescents (Angeles et al. 2019). Hence, a reduction in paid work represents an improvement among children.

Engaging in economic activities may also be problematic if it exposes children to hazards that may affect their physical or mental health. Based on a UNICEF survey module extensively tested by Dayıoğlu (2012), the Malawi endline data capture exposure to four hazards during engagement in economic activities and household chores: carrying heavy loads; working with dangerous tools; exposure to dust, fumes, or gas; and exposure to extreme cold, heat, or humidity.[30] Malawi's SCTP increased the likelihood that children engage in any of these hazardous activities by 4 percentage points (relative to 25 percent among children in control villages) (table 9). Exposure to all four individual hazards rose significantly.

To understand more clearly what drives the observed increase in hazardous work, table S1.9 in the supplementary online appendix relies on data from control villages and compares the prevalence of hazards between children who participated in any economic activities during the week before the interview and those who did not. Exposure to hazards is nearly 80 percentage points higher in the first group than in the second. Thus, the economic activities in which children engage in this setting inherently entail the four measured hazards, and changes at the extensive margin result in almost one-to-one changes in exposure to hazards.

Health Outcomes

Increased exposure to hazards did not translate into more illness and injury in the short term. The program in Malawi slightly reduced illness during the 12 months prior to the endline survey (bottom of table 9), suggesting that, in the short run, any negative effects on health because of work-related hazards may be offset by positive income effects.[31] Of course, the economic activities carried out by children may have longer-term implications not captured by this study.

30 The survey module also captured a fifth hazard: exposure to loud noise or vibration. The findings on this hazard are not displayed because only 2 percent of children in the data are exposed to loud noise. These five hazards were considered because Dayıoğlu (2012) shows that, after accounting for these five hazards, the measurement of additional hazards does not significantly change estimates of the incidence of hazardous work.

31 Point estimates for the same health outcomes in Zambia are also negative, but smaller and not statistically significant. (The results are available on request.)

Table 9. Impact on Hazardous Work and Health Outcomes, Malawi

	Control mean (1)	Impact (2)
Engages in any hazardous productive activities	0.250	0.044**
		[0.026]
Carrying heavy loads	0.147	0.046***
		[0.001]
Working with dangerous tools	0.151	0.048*
		[0.062]
Exposure to dust, fumes, or gas	0.209	0.060***
		[0.002]
Exposure to extreme heat, cold, or humidity	0.162	0.067***
		[0.002]
Ill or injured during the 2 weeks prior to the interview	0.169	−0.013
		[0.309]
Ill during the 12 months prior to the interview	0.015	−0.007***
		[0.003]
N		5,806

Source: Malawi Economic, Health and Demographic Survey (MEHDS).
Note: The sample includes children aged 6–15 years at baseline (8–17 years at endline). Results are obtained using the endline data and regression specification (1), including fixed effects for gender, age, stratification variable (Traditional Authority), and enumerator. Regressions also include controls for household composition (dummy variables for whether the household includes 3 members, 4 or more members, number of members below 21 years of age, number of members between 21 and 29 years of age). *P*-values in brackets, estimated using the wild bootstrap method (parameters: 999 replications, Rademacher weights), clustering at the unit of randomization level (village cluster). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The qualitative findings presented in Zietz, de Hoop, and Handa (2018) help in understanding the hazards measured in the endline survey. Children carry heavy loads when they collect water and firewood for the household, when they harvest maize and sugarcane on the household farm, and when they transport sand and water while making bricks for pay. Dangerous tools are mostly limited to panga knives and shovels, while the use of heavy machinery is not reported. Children are exposed to dust when they farm on dry land and sweep the floor in and around the household dwelling, which can result in coughs and breathing problems. Children are also exposed to smoke, while cooking, for instance. There was some mention of exposure to extreme heat because of farm work in the midday sun and exposure to extreme noise when working near maize mills. Injuries were regularly reported, but they tended to be minor.[32] These qualitative observations help explain why an increase in hazards does not translate into a deterioration in health outcomes in the quantitative data.

5 Concluding Discussion

This paper examines the potential cascading effects on children of increased investment in the household enterprise induced by unconditional cash transfer programs. The focus is on cash transfers provided to ultrapoor, labor- and credit-constrained households in Malawi and Zambia. In both countries, cash transfers resulted in an expansion of (mainly agricultural) household microentrepreneurial activity. Households accommodated the expansion of the household enterprise by reallocating adult labor supply and hiring external labor. Children, too, began to work more on the household farm. In Malawi, the increase in child work on the household farm was offset by a reduction in work outside the household for pay. In Zambia, no such reduction was observed, possibly because the areas studied in Zambia are particularly remote

32 Cases were also mentioned of youth facing bullying, violence, or intimidation during their chores or economic activities. Zietz, de Hoop, and Handa (2018) describe these cases in more detail.

and far from markets and trading centers. Limited opportunities to work outside the household for pay likely reduced the margin for program impact on these types of economic activities.

The implications for child well-being of increased work on the household farm cannot be fully captured. In Zambia, child engagement in excessive working hours, defined using age-specific thresholds, increased. In Malawi, because of the offsetting reduction in work for pay outside the household, no such increase is observed. Both programs raised school attendance. However, the study did not measure longer-term indicators of human capital accumulation, such as learning in school and progression to higher education levels. The study also did not explore how increased work on the farm, along with school attendance, affected child leisure and sleep, critical elements of child well-being and development.

The program in Malawi increased child exposure to work-related hazards, such as carrying heavy loads and working with dangerous tools. Nonetheless, contemporaneous indicators of child health improved slightly in Malawi, suggesting that positive income effects may have outweighed the implications of exposure to hazards in the short run. Indeed, both programs substantially improved nutrition outcomes and indicators of child material well-being, such as ownership of a pair of shoes, possessing two sets of clothes, and sleeping under an insecticide-treated mosquito net (AIR 2016; UNC-CH 2016). However, some negative health consequences of hazardous work may become manifest only in the longer run, for instance, the effects of exposure to toxic materials or back injuries resulting from carrying heavy loads at an early age. Because the study observed both positive and potentially negative changes in the lives of children, it reached no definitive conclusion about the net effect of these programs on child well-being.

The study did not conclusively establish the exact reasons why children work more in the household enterprise. Here are some brief reflections on plausible mechanisms. First, adults in beneficiary households may have limited ability to expand their net labor supply. Both programs target households with few able-bodied adults. While both programs increased adult work in the household enterprise, this increase was largely offset by a decrease in work for pay outside the household. The latter could imply that adults already face heavy demands on their time and cannot expand their net labor supply. Second, while beneficiary households hire more labor, it nonetheless seems plausible that the recruitment of external labor is hampered by labor market imperfections, such as seasonality in agricultural production, which can lead to shortages on the labor market when households need additional hands (Rosenzweig 1988). Furthermore, hired labor and family labor may not be perfect substitutes (de Janvry, Fafchamps, and Sadoulet 1991; Taylor and Adelman 2003). Bhalotra and Heady (2003, 198), for example, suggest that "the problem of moral hazard with hired labor may generate preference for family labor."

Third, the enterprises run by program beneficiaries typically require skills obtained outside the formal education system. The importance of these skills may grow as the household enterprise expands, as suggested, for instance, by Bhalotra and Heady (2003). Early engagement in the household enterprise may be a way to acquire these skills and, hence, a reason for children to work. The quantitative surveys do not capture on-the-job learning. However, participants in the qualitative interviews mentioned that work in the household enterprise is important to skill formation. Thus, in relation to the acquisition of farming skills, a 15-year-old girl mentioned that "if we were to just sit and watch the parents do it, we could be in trouble in future after the parent's passing" (Zietz, de Hoop, and Handa 2018, 250). Finally, liquidity constraints extend the shadow price of inputs that require cash payments, such as hired labor, making it more efficient to rely on family labor to expand productive activity (Singh, Squire, and Strauss 1986). The study areas are mostly in remote locations, and households have little contact with the market economy. It is plausible that having cash is valuable for these households, limiting their willingness to spend it on hired labor.

The main policy implication of this study is that programs resulting in expanded household productive activities need to take the potential implications for children seriously. As shown here, even in cash transfer programs, which are widely considered an important tool for supporting poor and vulnerable

children, the implications for child well-being are not obvious. The same holds for programs that have the explicit objective of expanding household entrepreneurial activity. Monitoring changes in children's time use and the implementation of complementary interventions, such as the provision of information on the importance of schooling, should be considered in conjunction with these programs.

Future studies might consider using outcome variables beyond the time-use indicators used in this study. Besides their effect on schooling, excessive hours of work and chores may also affect the time available for other activities, such as play and sleep. And, if direct interviews with children are feasible, measures of psychosocial well-being and affect should also be considered as a tool for capturing mental health. In programs that strongly affect long working hours, it would be helpful to use additional indicators that capture performance in school and a broader spectrum of child activities. Furthermore, a generic module for the measurement of hazardous activities, such as the one used in this study, may miss relevant work-related hazards. The measures of hazards used in quantitative surveys could be adapted to reflect more closely the hazards that are most relevant among children in a study population. Moreover, while child engagement in work and chores may have negative effects, it may also have benefits, such as on-the-job learning. A better view on the benefits of child engagement in work and chores could improve understanding of the impact of policy interventions on child well-being. More could be done to understand how changes in household production technology affect the child's experience in supporting the household enterprise, including exposure to hazards and opportunities for on-the-job learning.

References

AIR (American Institutes for Research). 2016. "Zambia's Multiple Category Targeting Grant: 36-Month Impact Report." February, AIR, Washington, DC.

Angeles, G., J. de Hoop, S. Handa, K. Kilburn, A. Milazzo, and A. Peterman. 2019. "Government of Malawi's Unconditional Cash Transfer Improves Youth Mental Health." *Social Science & Medicine* 225, 108–19.

Augsburg, B., R. De Haas, H. Hatmgart, and C. Meghir. 2012. "Microfinance, Poverty, and Education." IFS Working Paper No. W12/15, Institute for Fiscal Studies, London, UK.

Baird, S., F. H. G. Ferreira, B. Özler, and M. Woolcock. 2014. "Conditional, Unconditional, and Everything in Between: A Systematic Review of the Effects of Cash Transfer Programs on Schooling Outcomes." *Journal of Development Effectiveness* 6 (1): 1–43.

Basu, K., S. Das, and B. Dutta. 2010. "Child Labor and Household Wealth: Theory and Empirical Evidence of an Inverted-U." *Journal of Development Economics* 91 (1): 8–14.

Basu, K., and Z. Tzannatos. 2003. "The Global Child Labor Problem: What Do We Know and What Can We Do?" *World Bank Economic Review* 17 (2): 147–73.

Behrman, J. R., S. W. Parker, and P. E. Todd. 2009. "Medium-Term Impacts of the *Oportunidades* Conditional Cash Transfer Program on Rural Youth in Mexico." In *Poverty, Inequality, and Policy in Latin America*, edited by S. Klasen and F. Nowak-Lehmann, 219–70. Cambridge, MA: MIT Press.

Behrman, J. R., P. Sengupta, and P. E. Todd. 2000. "Final Report: The Impact of Progresa on Achievement Scores in the First Year." International Food Policy Research Institute, Washington, DC.

Bhalotra, S., and C. Heady. 2003. "Child Farm Labor: The Wealth Paradox." *World Bank Economic Review* 17 (2): 197–227.

Cameron, A. C., J. B. Gelbach, and D. L. Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90 (3): 414–27.

Covarrubias, K., B. Davis, and P. Winters. 2012. "From Protection to Production: Productive Impacts of the Malawi Social Cash Transfer Scheme." *Journal of Development Effectiveness* 4 (1): 50–77.

Dammert, A., J. de Hoop, E. Mvukiyehe, and F. C. Rosati. 2018. "Effects of Public Policy on Child Labor: Current Knowledge, Gaps, and Implications for Program Design." *World Development* 110 (October): 104–23.

Dayıoğlu, M. 2012. "How Sensitive Are Estimates of Working Children and Child Labour to Definitions? A Comparative Analysis." MICS Methodological Paper No. 1, Statistics and Monitoring Section, Division of Policy and Strategy, United Nations Children's Fund, New York, NY.

de Hoop, J., J. Friedman, E. Kandpal, and F. C. Rosati. 2019. "Child Schooling and Child Work in the Presence of a Partial Education Subsidy." *Journal of Human Resources* 54 (2): 503–31.

de Hoop, J., and F. C. Rosati. 2014. "Cash Transfers and Child Labor." *World Bank Research Observer* 29 (2): 202–34.

de Janvry, A., M. Fafchamps, and E. Sadoulet. 1991. "Peasant Household Behaviour with Missing Markets: Some Paradoxes Explained." *Economic Journal* 101 (409): 1400–17.

de Janvry, A., F. Finan, E. Sadoulet, and R. Vakis. 2006. "Can Conditional Cash Transfer Programs Serve as Safety Nets in Keeping Children at School and from Working When Exposed to Shocks?" *Journal of Development Economics* 79 (2): 349–73.

de Mel, S., D. J. McKenzie, and C. Woodruff. 2014. "Business Training and Female Enterprise Start-Up, Growth, and Dynamics: Experimental Evidence from Sri Lanka." *Journal of Development Economics* 106 (January): 199–210.

Dumas, C. 2013. "Market Imperfections and Child Labor." *World Development* 42 (February): 127–42.

Edmonds, E. V. 2006. "Child Labor and Schooling Responses to Anticipated Income in South Africa." *Journal of Development Economics* 81 (2): 386–414.

——— 2007. "Child Labor." In *Handbook of Development Economics*, vol. 4, edited by T. P. Schultz and J. A. Strauss, 3607–3709. Amsterdam: Elsevier.

Edmonds, E. V., and N. Schady. 2012. "Poverty Alleviation and Child Labor." *American Economic Journal: Economic Policy* 4 (4): 100–24.

Edmonds, E. V., and M. Shrestha. 2014. "You Get What You Pay For: Schooling Incentives and Child Labor." *Journal of Development Economics* 111 (November): 196–211.

Fiszbein, A., and N. R. Schady. 2009. *Conditional Cash Transfers: Reducing Present and Future Poverty*. Edited by F. H. G. Ferreira, M. E. Grosh, N. Keleher, P. Olinto and E. Skoufias. World Bank Policy Research Report Series. Washington, DC: World Bank.

Fitzgerald, J., P. Gottschalk, and R. Moffitt. 1998. "An Analysis of Sample Attrition in Panel Data: The Michigan Panel Study of Income Dynamics." *Journal of Human Resources* 33 (2): 251–99.

Fors, H. C. 2012. "Child Labour: A Review of Recent Theory and Evidence with Policy Implications." *Journal of Economic Surveys* 26 (4): 570–93.

Fujiwara, T., and L. Wantchekon. 2013. "Can Informed Public Deliberation Overcome Clientelism? Experimental Evidence from Benin." *American Economic Journal: Applied Economics* 5 (4): 241–55.

Garcia, S., and J. Hill. 2010. "Impact of Conditional Cash Transfers on Children's School Achievement: Evidence from Colombia." *Journal of Development Effectiveness* 2 (1): 117–37.

García, S., and J. E. Saavedra. 2017. "Educational Impacts and Cost-Effectiveness of Conditional Cash Transfer Programs in Developing Countries: A Meta-Analysis." *Review of Educational Research*, 87 (5): 921–65.

Green, E. P., C. Blattman, J. Jamison, and J. Annan. 2016. "Does Poverty Alleviation Decrease Depression Symptoms in Post-Conflict Settings? A Cluster-Randomized Trial of Microenterprise Assistance in Northern Uganda." *Global Mental Health* 3 (e7): 1–9.

Handa, S., S. Daidone, A. Peterman, B. Davis, A. Pereira, T. Palermo, and J. Yablonski. 2018a. "Myth-Busting? Confronting Six Common Perceptions about Unconditional Cash Transfers as a Poverty Reduction Strategy in Africa." *World Bank Research Observer* 33 (2): 259–98.

Handa, S., and B. Davis. 2006. "The Experience of Conditional Cash Transfers in Latin America and the Caribbean." *Development Policy Review* 24 (5): 513–36.

Handa, S., L. Natali, D. Seidenfeld, and G. Tembo. 2016. "The Impact of Zambia's Unconditional Child Grant on Schooling and Work: Results from a Large-Scale Social Experiment." *Journal of Development Effectiveness* 8 (3): 346–67.

Handa, S., L. Natali, D. Seidenfeld, G. Tembo, and B. Davis. 2018b. "Can Unconditional Cash Transfers Raise Long-Term Living Standards? Evidence from Zambia." *Journal of Development Economics* 133 (July): 42–65.

Heß, S. 2017. "Randomization Inference with Stata: A Guide and Software." *Stata Journal* 17 (3): 630–51.

Hidrobo, M., J. Hoddinott, N. Kumar, and M. Olivier. 2018. "Social Protection, Food Security, and Asset Formation." *World Development* 101 (January): 88–113.

Horowitz, J. L., and C. F. Manski. 1998. "Censoring of Outcomes and Regressors Due to Survey Nonresponse: Identification and Estimation Using Weights and Imputations." *Journal of Econometrics* 84 (1): 37–58.

ILO (International Labour Organization). 2008. "Report III: Child Labour Statistics." Report No. ICLS/18/2008/III, ILO, Geneva, Switzerland.

Kilburn, K., S. Handa, G. Angeles, P. Mvula, and M. Tsoka. 2017. "Short-Term Impacts of an Unconditional Cash Transfer Program on Child Schooling: Experimental Evidence from Malawi." *Economics of Education Review* 59 (August): 63–80.

Lee, D. S. 2009. "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects." *Review of Economic Studies* 76 (3): 1071–1102.

McKenzie, D. J., N. Assaf, and A. P. Cusolito. 2017. "The Additionality Impact of a Matching Grant Programme for Small Firms: Experimental Evidence from Yemen." *Journal of Development Effectiveness* 9 (1): 1–14.

Miller, C., M. Tsoka, and K. Reichert. 2010. "The Impact of the Social Cash Transfer Scheme on Food Security in Malawi." *Food Policy* 36 (2): 230–38.

Nelson, L. K. 2011. "From Loans to Labor: Access to Credit, Entrepreneurship, and Child Labor." Working paper, Department of Economics, University of California at San Diego, San Diego, CA.

Oryoie, A. R., J. Alwang, and N. Tideman. 2017. "Child Labor and Household Land Holding: Theory and Empirical Evidence from Zimbabwe." *World Development* 100 (December): 45–58.

Prifti, E., E. Estruch, S. Daidone, B. Davis, P. Van Ufford, S. Michelo, S. Handa, D. Seidenfeld, and G. Tembo. 2017. "Learning about Labour Impacts of Cash Transfers in Zambia." *Journal of African Economies* 26 (4): 433–42.

Ravallion, M., and Q. Wodon. 2000. "Does Child Labour Displace Schooling? Evidence on Behavioural Responses to an Enrollment Subsidy." *Economic Journal* 110 (462): C158–75.

Rosenzweig, M. 1988. "Labor Markets in Low-Income Countries." In *Handbook of Development Economics*, vol. 1, edited by H. Chenery and T. N. Srinivasan, 713–62. Amsterdam: North-Holland.

Sebastian, A., A. P. de la O Campos, S. Daidone, N. Pace, B. Davis, O. Niang, and L. Pellerano. 2019. "Cash Transfers and Gender Differentials in Child Schooling and Labor: Evidence from the Lesotho Child Grants Programme." *Population and Development Review* DOI:10.1111/padr.12269.

Seidenfeld, D., L. Prencipe, and S. Handa. 2012. "Zambia's Multiple Category Cash Transfer Program: Baseline Report." August 7. American Institutes for Research, Washington, DC.

Singh, I., L. Squire, and J. Strauss. 1986. *Agricultural Household Models: Extensions, Applications, and Policy*. Baltimore: Johns Hopkins University Press.

Skoufias, E., and S. W. Parker. 2001. "Conditional Cash Transfers and Their Impact on Child Work and Schooling: Evidence from the PROGRESA Program in Mexico." *Economía* 2 (1): 45–96.

Taylor, J. E., and I. Adelman. 2003. "Agricultural Household Models: Genesis, Evolution, and Extensions." *Review of Economics of the Household* 1 (1–2): 33–58.

UNC-CH (University of North Carolina at Chapel Hill). 2014. "Malawi Social Cash Transfer Program: Baseline Impact Evaluation Report." July 8. Chapel Hill, NC: Carolina Population Center, UNC.

———. 2016. "Malawi Social Cash Transfer Program: Endline Impact Evaluation Report." November 15. Chapel Hill, NC: Carolina Population Center, UNC.

World Bank. 2018. *The State of Social Safety Nets 2018*. Washington, DC: World Bank.

Zietz, S., J. de Hoop, and S. Handa. 2018. "The Role of Productive Activities in the Lives of Adolescents: Photovoice Evidence from Malawi." *Children and Youth Services Review* 86 (February): 246–55.