The Wider Impacts of Cash Transfers in Sub-Saharan Africa

Luisa Natali

Submitted for the degree of Doctor of Philosophy in Economics
University of Sussex
July 2018

Declaration

I hereby declare that this thesis has not been and will not be submitted in whole or in part to another University for the award of any other degree. Some of this thesis, specially Chapters 3 and 4, is based on co-authored work.

Signature:

Luisa Natali

UNIVERSITY OF SUSSEX

LUISA NATALI

DEGREE OF DOCTOR OF PHILOSOPHY IN ECONOMICS

THE WIDER IMPACTS OF CASH TRANSFERS IN SUB-SAHARAN AFRICA

SUMMARY

This thesis is formed of three separate essays which provide experimental evidence on the wider impacts of cash transfers in Uganda and Zambia.

In the first essay, I use data from a cluster randomized controlled trial of a food and cash transfer intervention in Karamoja, Uganda, targeted to households with young children and aimed at increasing food security and early child development. I aim to understand if transfer of resources has additional impacts which spill over into domains beyond primary objectives and onto children not directly meant to benefit from the programme. The cash transfer – although not framed as a livelihood programme – improved household productive investments and adults' labour supply. There is also a positive impact on children's workloads as children help their parents with agricultural activities. The child work effect does not affect children's schooling but rather comes at the expense of leisure time. Overall, I find no impact on intra-household allocation for the food treatment arm.

The second essay examines the impacts of the Government of Zambia's Child Grant Programme (CGP) on women's cash savings. The CGP is an unconditional cash transfer targeted to households with young children and paid directly to the child's caregiver, of whom virtually all are women. The analysis uses longitudinal data from a cluster randomized controlled trial in rural areas over 48 months. Results show that the CGP has enabled poor women to save in cash even in the absence of inclusive financial systems. In addition, the increase in women's cash savings does not crowd out other traditional forms of household savings such as livestock. Likely mechanisms which facilitate savings relate to changes in intra-household decision-making regarding control over resources and increased household investment in non-farm enterprises; feedback effects are plausible.

The third essay analyzes the impact of the same programme on child schooling and work. Although the CGP's primary objectives are related to very young children, we look to see if the programme has impacts on older children who, in principle, are not the main target population of the intervention. Using experimental impact evaluation data over 36 months, findings indicate that the CGP had a positive significant impact on attendance among children aged 11−14. This is the age range during which sharp dropout begins to occur in Zambia. The CGP has also reduced work-for-pay among children aged 11−14. Finally, the analysis provides evidence on the potential pathways through which the intervention impacts school attendance. Households in the CGP spend more on education, and in particular on uniforms, key barriers to school enrolment and attendance in study areas.

Acknowledgements

Writing this thesis has been a challenging and unique experience. I am grateful to my advisors, friends, and family who supported me along the way.

I would like to thank my supervisors. It is because of Andy McKay that I started my PhD at Sussex; I am grateful for the guidance and mentorship he provided since my Master's Degree more than 10 years ago. Sanchari Roy joined later in the journey and provided invaluable and acute comments to improve this thesis. They both taught me to persevere, be more accurate and changed my way of approaching research; I believe this has been the greatest gift they could have given me.

Special thanks go to Sudhanshu Handa for his continuous support. He has been my external PhD supervisor as well as my 'capo' at the UNICEF Office of Research. He opened the door of the Transfer Project to me, has been encouraging and pushed me to finish the doctorate. He has always been very generous with his time and ideas; most of all he believed in me and taught me that there is no stupid question. I hope to always carry with me his excitement for research.

I am indebted to Amber Peterman. I learnt a lot from her way of working and she has become a researcher role model for me. I admire the dedication she puts into everything she does. She has always been very supportive about my PhD and provided comments no matter where in the world she was. She inspires me to be a better researcher and human being.

I also thank the Transfer Project, the UNICEF Office of Research – Innocenti and UNICEF Zambia for allowing me to use the evaluation research for the Zambian Child Grant Programme; IFPRI who made Karamoja data publicly available and Dan Gilligan and Shalini Roy who were generous in sharing their knowledge about the data and context. A special thanks goes to all the individuals who took part in the surveys in Kaputa, Shangombo, Kalabo and Karamoja for their time and trust.

I am grateful to the participants of the conferences I have attended during the PhD, the Department of Economics at Sussex and my PhD colleagues. I am thankful for the great and supporting environment I had the good fortune to work in and which made it possible to finish this PhD. I am grateful to my (current and previous) colleagues at the Transfer Project and the UNICEF Office of Research – Innocenti: Adria, Alessandra, Amber, Andrea, Ashu, Audrey, Averi, Bruno, Cecile, Chris, Cinzia, Elsa, Emanuela, Eve (who proved once more her patience and support by, among other things, proofreading this thesis a number of times), Flora, Frank, Gab, Garima, George, Heidi, Ilze, Jo, Jacob, Jose, Kat, Laura, Leah, Lisa, Lucia (and the trips to Asmana!), Marloes, Michelle, Patricia, Sarah, Tia, Valeria and to all the rest of my amazing colleagues.

I thank my friends, foremost, my best friend Enrica who has been holding my hand since we were five for her unwavering support, and her family Alberto and Franca. Carlo without whom my life in Florence would have not been the same. Mimma and Silvia for putting up with my 'White Rabbit' syndrome; but also Francesca F., Laura, Virginia and Mario R.M. who kept me sane throughout the process! Maria and Tommaso who welcomed me countless numbers of times in their Brightonian home and made each time enjoyable; to my 'Sussex' friends who are now spread around the world: 'crazy' Marta who always told me not to start a PhD, Stefania who is always a welcoming face in the UK, Gonzalo for his 'pearls of wisdom' and the laughs, Elsa for sharing with me her PhD survival strategies, but also Adriana, Antonio, Cecilia, Francesca C., Giulia, Javier, Mario V., Marinella, Paola and many others.

Finally, I want to thank my family for their support: my parents, Miriam and Dario, who never got tired of asking whether I was submitting, scared I would not invite them to the graduation ceremony, but who always believed this moment would (eventually) arrive! My brother and sister, Franco and Claudia (Biso and Grolla), who have always been there in their own way.

Thanks to all those who at different times supported me during this trip and who I might have left out.

Table of Contents

List of Figures

List of Tables

List of acronyms

AIR	American Institutes for Research
ANCOVA	Analysis of Covariance
ATE	Average Treatment Eeffect
BIC	Baseline Index Child
CCT	Conditional Cash Transfers
CGP	Child Grant Programme
cRCT	Cluster Randomized Controlled Trial
CSB	Corn Soya Blend
CSCD	Community Support for Capacity Development
CT-OVC	Cash Transfer - Orphans and Vulnerable Children
CWAC	Community Welfare Assistance Committee
DD	Difference-in-difference
DEO	District Education Office
ECD	Early Childhood Development
FFE	Food for Education
FWER	Family-Wise Error Rate
GDP	Gross Domestic Product
IFPRI	International Food Policy Research Institute
ITT	Intent-to-Treat
KPAP	Karamoja Productive Assets Program
LCMS	Living Conditions and Monitoring Survey
LMIC	Low- and Middle-Income Country
LPM	Linear Probability Model
MCDMCH	Ministry of Community Development, Mother and Child Health
MCDSS	Ministry of Community Development and Social Services
MCHN	Maternal, Child Health and Nutrition
MFI	Microfinance Institution
MICS	Multiple Indicator Cluster Survey
NFE	Non-Farm Enterprise
NGO	Non-Governmental Organization
NUSAF	Northern Uganda Social Action Fund
OLS	Ordinary Least Squares

PPP	Purchasing Power Parity
PSNP	Productive Safety Net Programme
PSSN	Productive Social Safety Net
RCT	Randomized Controlled Trial
ROSCA	Rotating Savings and Credit Association
SCAE	Subsidios Condicionados a la Asistencia Escolar
SCTP	Social Cash Transfer Programme
SUTVA	Stable Unit Treatment Value Assumption
UCT	Unconditional Cash Transfer
UNCST	Uganda National Council of Science and Technology
UPE	Universal Primary Education
WFP	World Food Programme
WINGS	Women's Income Generating Support
ZDHS	Zambia Demographic and Health Survey
ZMW	Zambian kwacha

1 Introduction

In last twenty years, there has been an important paradigm shift in development thinking: from the imperative of economic growth – and the belief it would lead to sustainable poverty reduction – towards the creation of social safety nets and design of anti-poverty policies that brought cash transfers into the foreground.

Since the mid 1990s, social protection programmes that aimed at alleviating the impacts of poverty and vulnerability by providing households with regular cash payments have expanded rapidly around the globe. Cash transfers in particular have become an important part of the poverty alleviation toolkit in developing countries, even among the poorest where, for many, such programmes seemed both adminstratively complex or simply unaffordable.

Conditional cash transfers were first introduced at the end of the 1990s in Latin America. Their objectives are to alleviate short-term poverty through the cash grant and to build long-term human capital, through the fulfilment of behavioural conditions typically related to education or health (such as sending children to school or vaccinating them). Since the mid-2000s, social cash transfers began to be introduced across sub-Saharan Africa too; here, transfers are largely unconditional, meaning beneficiaries receive cash grants without any attached behavioural requirements. According to the World Bank's State of Social Safety Nets report (World Bank, 2015), the share of cash transfers is on the rise globally with sub-Saharan Africa (SSA) experiencing the greatest expansion. Indeed, in SSA, the number of countries with some form of unconditional cash transfer (UCT) doubled from 21 to 40 (out of 48) between 2010 and 2014.

The 'African model' of cash transfers has several distinguishing features which separate it from those in Latin America. First and foremost, programmes tend to be unconditional, and even in the few places where conditons are formally part of the programme rules, these are rarely monitored and never punitively sanctioned ('soft conditions'; World Bank, 2015; Garcia and Moore, 2012).[1] This difference has been driven by a number of factors including supply-side constraints (lack of adequate health and education infrastructure), weak administrative and implementation capacities in these countries, and the lack of financial resources needed to monitor and enforce conditionalities (Barrientos and Villa, 2015; Gaarder, 2012; Schubert and Slater, 2006).

The second feature is the distinct eligibility criteria and targeting method. Eligibility criteria are typically extreme poverty and demographic features such as labour-constraints (e.g. Zimbabwe, Malawi, Liberia) or presence of orphans or other vulnerable children (Ghana, Kenya, Lesotho) to reach poor and vulnerable populations, a specific concern particularly in the southern African context also as a response to the spread of HIV. Targeting itself tends to have an important role for the community, either in compiling initial eligibility lists (Kenya, Ghana) or vetting final lists which have been compiled by those outside the community (Zimbabwe, Lesotho) (Davis et al. 2016).

The most extensive examples of UCTs in SSA are found in South Africa: the Old Age Pension is targeted to poor households with older persons, while the Child Support Grant (CSG) is devoted to children and covers around 65 per cent of South Africa's child population (Cirillo and Tebaldi, 2016). Among low-income countries, Ethiopia offers an example with the Productive Safety Net Programme (PSNP), which since 2005 provides food and cash transfers to chronically food insecure households through direct support (unconditional transfers to labour-constrained households) or in exchange for work in community projects. Excluding South Africa, the Ethiopian PSNP is the largest social protection programme in SSA, with the direct support

[1] For instance, in Lesotho and Ghana programmes are nominally conditional but monitoring is not routinely performed. Tanzania (PSSN) is the only new programme with conditions that come with punitive sanctions (Davis et al. 2016).

covering approximately 1.2 million beneficiaries in 242,383 households in 2010 (Garcia and Moore, 2012). In Kenya, the Cash Transfer to Orphans and Vulnerable Children (CT-OVC) targeted to ultra-poor households became the government's main social protection programme in 2007, reaching 260,000 households. Over time, soft conditions on investment in children's health, nutrition and education have been gradually introduced to the programme (Garcia and Moore, 2012).

As in Latin America, the new African programmes have been accompanied by the increase in the number of rigorous evaluations undertaken that provide robust evidence in support of cash transfers. Indeed, cash transfers have been shown to have the potential to positively impact beneficiary households in several dimensions, including consumption and food security, monetary poverty, education and child work, and the accumulation of physical capital and productive activities among others (Fiszbein and Schady, 2009; Haushofer and Shapiro, 2016; Baird et al., 2014; de Hoop and Rosati, 2014a; Prifti et al., 2017; Handa et al., 2017; 2018; see also Bastagli et al., 2016 for a review of impacts). Unconditional cash transfers have been found less effective in improving child nutrition outcomes (Manley et al., 2013; de Groot et al. 2017).

The evidence on unconditional cash transfers however also indicates that exogenous income increases could – directly or indirectly – influence outcomes beyond the realm of programme objectives such as, for instance, productive investments and activities (Asfaw et al., 2014), labour supply (Banerjee et al., 2017), fertility (Palermo et al., 2016), stress (Hjelm et al., 2017) or intimate partner violence (Hidrobo et al., 2016) among others.

In unconditional cash transfers money is not tied to behaviour and households are free to spend the cash as they wish. These broad-based programmes are capable of affecting multiple aspects of a beneficiary's life. As a result, a broader range of effects could be found across less traditional dimensions. Exploiting the particular feature of unconditional cash transfers in SSA as

compared to conditional cash transfers (CCTs), allows investigation of relatively unexplored areas and a focus on outcomes outside the narrow primary objectives of the programmes.[2]

In this thesis, I examine the impacts of unconditional cash transfers on outcomes beyond the main stated programme objectives; I focus on the effects on child labour and schooling, women's savings and the intra-household allocation of time in two sub-Saharan African countries, Zambia and Uganda. Analyzing these second-order impacts also often implies focusing on household members who in principle are not the main target population of the programme and therefore better explore spill-over within the households and intra-household dynamics.

The first essay uses data from a cluster randomized controlled trial (cRCT) of a food and cash transfer intervention targeted to households with young children, aimed at increasing food security and early child development (ECD) in Karamoja, Uganda. I examine time allocation within the household to understand if transfer of resources has additional impacts which spill over into domains beyond primary objectives and onto children not directly intended as beneficiaries from the programme. This paper contributes to the social protection literature by highlighting unforeseen behavioural responses to the intervention. These unintended impacts may be important when it comes to assessing the overall success, design and sustainability of a programme.

The last two essays focus on the impacts of the Government of Zambia's Child Grant Programme (CGP), an unconditional cash transfer targeted to households with children under the age of five and whose objectives are mainly focused on their health and development. The longitudinal data used comes from a cluster randomized controlled trial implemented to assess the impact of the programme in three of the most remote rural districts of Zambia.

The second essay[3] examines the impacts of the same programme on women's cash

[2] As highlighted in Davis et al. (2016, p. 6) "Recipients in conditional programmes also have some flexibility in how they spend money, but there are clear incentives to spend on health and education and basic foods since receipt of the transfer is conditional on health and education behaviour. As a result, impact evaluations of CCTs have tended to focus on outcomes in these narrow areas."

[3] This is a substantially developed version of the following joint work: Natali, L., S. Handa, A. Peterman, D. Seidenfeld, G. Tembo, Zambia Cash Transfer Evaluation Team (2016). Making Money Work: Unconditional cash transfers allow women to save and re-invest in rural Zambia, *Innocenti Working Paper* No.2016-02, UNICEF Office of Research, Florence.

savings. For low-income households in developing settings, savings play an extremely important role in smoothing consumption and faciliating investment in income-generating activities and the pathway out of poverty. Yet, there is little evidence of successful programming efforts to facilitate savings, including those which specifically target women, and are cost effective and scaleable. Even though unconditional cash transfers are not traditionally thought of as saving instruments, as households receiving UCTs do not have to comply with any condition to receive the transfer and are free to use the money according only to their needs, preferences and major constraints faced, broad impacts across multiple domains are possible; moreover, the fact that the grant is paid directly to the child's caregiver – of whom 99 per cent are women (functional targeting) – opens up the potential for female-level outcomes, and might translate to improvements in women's empowerment.

The third essay reports the impact of the same programme on child schooling and work after three years.[4] Notwithstanding several published studies on the impact of CCTs on schooling, the evidence from government-run programmes using experimental data in Africa is less abundant, but is growing. Although the CGP's focus is on very young children, this chapter looks to see if the programme has impacts on older children who are not the explicit target group. Schooling impacts in this context are less straightforward compared to those from conditional cash transfers in Latin America, where participation is conditional on schooling-related behaviour; effects of the CGP, particularly among older children's school attendance, would be considered somewhat secondary level or 'spillover' effects facilitated in part by the unconditional nature of the programme.

The rest of the thesis is structured as follows: Chapter 2 investigates the impacts of transfers on the intra-household allocation of time in Karamoja, Uganda; Chapter 3 examines the impacts of the Government of Zambia's Child Grant Programme (CGP) on women's cash

[4] This is an adapted and developed version of the following joint work: Handa, S., L. Natali, D. Seidenfeld, G. Tembo, and the Zambia Cash Transfer Evaluation Team (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): 346-367.

savings; Chapter 4 examines the impacts of the same programme on child labour and schooling of children 7−14 years old; Chapter 5 provides a summary of the findings of each chapter, discusses potential further areas of research and offers some concluding remarks and policy implications.

2 The Effects of Transfers on Intra-household Time Allocation: Evidence from Northern Uganda

2.1 Introduction

Most of the literature on transfers to the poor has focused on the evaluation of the primary programme objectives, or of the key intended outcomes related to poverty, food security and human capital accumulation. Accordingly, there is increasing evidence on the positive impact of transfers on consumption and poverty (Fiszbein and Schady, 2009; Haushofer and Shapiro, 2016; Gentilini et al., 2014), education (Baird et al., 2014; Ravallion and Wodon, 2010; Alderman et al., 2012; Kazianga et al., 2012), food security, nutrition and health (Gertler, 2004; Barham, 2011; see also Bastagli et al., 2016).

Evidence is more limited when it comes to unforeseen or unintended effects, whether desirable or not. The exogenous increases in resources may influence outcomes beyond stated programme objectives, directly or indirectly. This is the case, for instance, on outcomes including productive investments and activities (Asfaw et al., 2014), labour supply (Banerjee et al., 2017), fertility (Palermo et al., 2016), stress (Hjelm et al., 2017) or intimate partner violence (Hidrobo et al., 2016).

In addition, studying second-order effects may be particularly important in the case of large-scale, broad-based programmes capable of affecting multiple aspects of a beneficiary's life. This is especially true in the context of unconditional cash transfers – where beneficiaries have

the freedom to spend money as they wish according only to their preferences and the constraints they face.

Finally, by focusing primarily on programme objectives, existing studies have largely overlooked impacts on intra-household dynamics. A transfer may differentially affect individuals within the household when – even if targeted at the household level – it has a specific household member assigned to receive it ('functional targeting'), it aims at improving the well–being of a particular group of individuals within the household, or it encourages certain activities through messaging or conditions. Such design features are likely to have cross–effects. For instance, a shift in the household's resources following the receipt of the transfer can translate into a shift in time allocation. This might happen if the transfer helps to overcome liquidity constraints that prevent individuals investing and engaging in productive activities (Prifti et al., 2017; Asfaw et al., 2014; Asfaw et al., 2016; Covarrubias et al., 2012; Fisher et al., 2017) and, as a consequence, some household members work more. The availability of new productive opportunities within the household could also trickle down to children within the household who might increase their labour supply, either to help parents in these new activities or by substituting adults in those activities they can no longer perform (de Hoop et al., 2017; Dammert et al., 2018).

As a result, a transfer may lead to non-neutral distributional impacts. From a policy perspective, it is crucial to take into account intra-household effects to fully understand both intended and unintended impacts, assess the overall success and sustainability of a programme, and to inform policy.

In terms of analysing such intra-household dynamics, several studies have looked at the impacts of transfers on child labour and schooling (see Baird et al., 2014 for a systematic review of cash transfers; Ravallion and Wodon, 2010; Alderman et al., 2012; Kazianga et al., 2012), however fewer have focused on adult labour supply (Baird et al., 2018; Banerjee et al., 2017; Handa et al., 2017; Skoufias et al., 2013; Margolies and Hoddinot, 2012) and fewer still have

examined the distinction between outcomes for children explicitly targeted by the programme and those who are not.[5]

I fill this gap in the literature by investigating the impacts of transfers on intra-household time allocation of adults and non-targeted children in the Karamoja region of Uganda. I assess whether an exogenous change in resources due to a transfer programme results in household members spending more or less time on various activities such as work, household chores, leisure, and schooling. Transfers are targeted to households with children aged 3–5 and linked to their participation in community-run Early Childhood Development (ECD) centres. I use data from a longitudinal randomized control trial with multiple arms (cash, food and control) conducted over approximately one year and a half from 2010–2012. The context is somewhat unique; the area is recovering from cattle raiding and conflict, with households very close to subsistence level, where a multitude of external assistance programmes are underway.[6] Karamoja is also plagued by chronic food insecurity, and physical infrastructure – including education and health facilities – is poor.

The programme that I study was implemented by UNICEF and the World Food Programme (WFP), the former facilitated the ECD centres – and the latter provided transfers roughly every six weeks. Transfers were either a nutrition-dense take-home food ration or cash transfer (of equivalent value, roughly USD 12). These incentives, aimed at encouraging young children's attendance at ECD centres, were paid preferentially to a woman in the household, and initially were meant to be conditional on the child attending the ECD centre. However, due to difficulties monitoring and enforcing such conditionality, transfers were made unconditional with the only requirement that the child was enrolled in the centre. The primary objectives of the programme were mainly related to food security and young children's outcomes.

[5] Predictions regarding the impacts of transfers on intra-household allocation outcomes depend on several factors such as the presence of conditionalities, the level of the transfer, the presence of supply-side interventions, the substitutability between child and adult labour, and whether leisure is a normal good for the poor, among others.

[6] Most external aid is in the form of food assistance, which is provided through a series of different channels and programmes, making cash transfers more valued. As discussed further in this chapter, these other programmes are balanced at baseline across treatment and control arms.

My findings indicate that after approximately one year of cash transfers, there is a positive impact on adults' participation in agriculture and on overall time spent on income-earning activities; the cash grant has enabled households to make productive investments in land. There is also a positive impact on primary school-aged children's involvement in productive work, driven by agricultural activities. This child work impact does not appear to come at the expense of schooling (whether measured as enrolment or attendance). Overall, I find no impact on intra-household time allocation for the food treatment arm. Null impacts could reflect either the ineffectiveness of the food intervention or relate to a lack of enforcement or implementation issues. For this reason, I focus mainly on the cash transfer impacts.

The main contributions of this study are twofold. First, I show that cash interventions can have an impact on the well-being of household members who were not explicitly targeted (intra-household spillover effects). Showing impacts on all household members provides a more comprehensive understanding of household dynamics and potential identification of cross-substitution effects. Second, I show the unintended effects of such intervention by focusing on impacts beyond main programme objectives. The analysis of these impacts and potential implications is essential for a comprehensive assessment of the intervention *vis–à–vis* its objectives. Evaluations of conditional cash transfers that target specific household members tend to focus only on these same household members. My study is part of the evaluation literature of unconditional cash transfers which increasingly assesses dimensions outside the scope of the intervention and beyond explicitly targeted household members to understand unintended behavioural responses.

The remainder of this Chapter is structured as follows. Section 2.2 provides a literature review. Section 2.3 describes the Karamoja context, the programme's details and the evaluation design; Section 2.4 describes the data, the indicators used in the analysis as well as the integrity of the experimental design. Section 2.5 reports on the empirical strategy while Section 2.6 presents the results. I further explore potential impact pathways in Section 2.7. Robustness checks are reported in Section 2.8 while Section 2.9 concludes with a discussion of findings.

2.2 Literature review

2.2.1 Theory: Impacts of transfers on intra-household allocation of time

There are several reasons why we might expect social safety net interventions to affect intra-household allocation of time, namely labour supply, education and leisure. Economic theory predicts that cash-based programmes – which constitute an increase in unearned income – would increase consumption and leisure, assuming leisure is a normal good, while reducing the labour supply of household members through the *income effect.*

In addition, conditional cash transfers could induce further behavioural responses through a *price effect*; grants conditioned on children's school attendance may, by reducing the shadow wage of children, lead to an increase in the time young children spend at school, relative to time spent in other activities – including work and leisure. An increase in schooling may not necessarily be accompanied by a reduction in child work, as it could come at the expense of the child's leisure time.

There might be an impact on other household members allocation of time[7] if the behavioural conditions (and related impacts) free up time previously devoted to child care or increase time spent on child care for parents. In the case of cross-substitution effects, a reduction in child work for instance might translate into other household members having to take over tasks previously performed by children or compensate for the loss in household income (Alzua et al., 2013; Canavire–Bacarreza and Ospina, 2015; Novella et al., 2012; Parker and Skoufias, 2000; Skoufias et al., 2001).

Apart from the income and price effect there are other channels through which a cash windfall might affect labour supply in the household. There could be an increase in the amount of work provided if the cash grant relaxes budget constraints which limited work-related investments or it helps to cover transaction and opportunity costs which previously deterred

[7] For example, in a model of family labour supply (Kilingsworth, 1983) the decision of each household member on how to allocate time is influenced by all household members value of time.

labour participation (*liquidity effect*) (Baird et al., 2018). It is possible that through an *insurance effect* a cash transfer may lead to an increase in labour supply by enabling individuals to undertake riskier but more profitable activities (Baird et al., 2018).

Also, the impact of cash transfers on child work is *a priori* ambiguous empirically. Child labour could decrease through the income effect. However, when transfers enable households to overcome liquidity constraints and increase the households' productive opportunities (liquidity effect), there could be an opposite effect as children might start to provide help in these activities.[8] Also, if adults increase their labour supply, child labour might increase through a substitution effect as children may start to carry out tasks previously performed by adults. The level of the transfer – and whether it covers completely schooling costs – could further influence the final outcomes (Dammert et al., 2018; de Hoop and Rosati, 2014a; de Hoop and Rosati, 2014b).

Skoufias et al. (2013) summarize the theory related to the potential impacts of in-kind and cash transfers on labour supply; if the in-kind transfer is *infra-marginal* – namely, it is smaller than that normally consumed by the beneficiary household – then the impacts of a food transfer would not differ from those predicted for a cash transfer (Southworth, 1945). In the case of *extra-marginal* in-kind transfers however, food transfers might lead an increase, rather than a decrease, in labour supply. Indeed, the theoretical impact of an in–kind transfer on labour supply depends on whether the subsidized good (food) and labour supply are complements or substitutes; if complements, then labour supply can be positively affected by the programme (as highlighted by Leonesio, 1988 and Gavhari, 1994).

In discussing the expected impacts of transfers on intra-household time allocation, there are other factors to consider. First, the potential impact of these interventions on the distribution of power within the household: transfers paid directly to women could have an impact on women's bargaining power which could in turn influence other household members' outcomes

[8] A review paper on public policies and child labour indicates that interventions that have an impact on household vulnerability and exposure to risk tend to reduce child work while those that increase adult labour participation or entrepreneurial activities can actually increase child labour (Dammert et al., 2018).

differently, and in particular the labour supply within the household. The net expected impacts are again ambiguous and would depend on women's preferences (Novella et al., 2012). Second, as far as child outcomes are concerned, it is also important to take into account whether these transfers, irrespective of the modality, are accompanied by supply-side interventions, for instance the provision of health and/or education facilities and services (Dammert et al., 2018).

2.2.2 Previous findings: Impacts of transfers on intra-household allocation of time

In this section, I first review the empirical literature on the impacts of cash transfers on child schooling and labour, adult labour and overall intra-household allocation of time. Second, I report the same for studies on food transfers.

There is a vast literature showing that exogenous changes in household income have demonstrated impacts on schooling and child work. Cash transfers, whether conditional or unconditional, are typically found to increase child schooling (Attanasio et al., 2010; Behrman et al., 2005; de Janvry et al., 2006; Glewwe and Kassouf, 2012; Edmonds, 2006; Edmonds and Schady, 2012; Filmer and Schady, 2011; Fiszbein and Schady, 2009; Kenya CT–OVC Study Team, 2012; Kilburn et al., 2017; Paxson and Schady, 2010; Saavedra and Garcia, 2017; Schultz, 2004; see Baird et al., 2014 for a systematic review).[9] However, impacts are often modified by the availability and access to schools. De Hoop and Rosati (2014a) provide a review of the impacts of cash transfers on child work. They find no perverse impact of cash transfers on child work, rather evidence indicates a reduction in child work in both extensive and intensive margins, with the reduction being greater in economic activities for boys and in domestic tasks for girls.

The distinction between children explicitly targeted by the programme and those who are not is often overlooked or not explicitly discussed in the literature. Transfers are rarely targeted to children in the traditional sense but rather to households. However, programmes are often either conditioned on specific children's outcomes, or households are eligible due to the presence of

[9] A few studies look at the relative impact of conditional versus unconditional cash transfers (Baird et al., 2011; Benhassine et al., 2015; Akresh et al., 2015).

children who are the actual target of the intervention; in these cases, the objectives of the programme tend to be directly related to these children. How the intervention affects other children in these households is rarely studied.

As regards the impact of cash transfers on adult labour supply, Banerjee et al. (2017) review seven rigorous evaluations on government-run primarily conditional cash transfer programmes in six countries in Latin America, North Africa and South-East Asia and find no evidence that cash transfers create labour disincentives. Handa et al. (2017) show that cash transfers do not reduce participation in productive work using data from eight RCTs of government-run unconditional cash transfers in Sub-Saharan Africa.[10] Finally, Baird et al. (2018) review evidence from low- and middle-income countries (LMICs) and conclude that cash transfers without explicit employment objectives and/or features, whether conditional or unconditional, tend to have very little impact on adult labour, with the only exceptions being transfers to the elderly and some refugees, that can result in a decrease in work.

Fewer studies look at the impact of cash transfers on overall intra-household time allocation. Parker and Skoufias (2000) and Rubio-Codina (2009) study the effects of the Progresa/Oportunidades programme in Mexico; their findings show significant increases in schooling and reductions in children's work activities (both market and non-market); they find no evidence of reduction in labour market participation rates for adults and no overall impacts on leisure. Canavire-Bacarreza and Ospina (2015) investigate the effects of the Familias en Acción in Colombia; their results are consistent with previous findings in terms of child outcomes (reduction in child work and increase in schooling) but they report an increase in the labour supply of adults. Finally, de Hoop et al. (2017) analyse the impacts of two government-run unconditional cash transfers in Zambia and Malawi; authors find adults increase their labour supply as their productive opportunities increase. Children too increase their participation in economic and

[10] Indeed, as cash transfers have been increasingly shown to have productive impacts (see for instance Prifti et al., 2017; Asfaw et al., 2014; Asfaw et al., 2016; Covarrubias et al., 2012; Fisher et al., 2017), an increase in the labour supply of household members might be expected.

domestic activities, however simultaneously increasing school attendance and a number of well-being indicators.

The impacts of food transfers are less studied, however Food for Education (FFE) is usually found to have a positive impact on school participation (Alderman et al., 2012). The impact on child labour has received less attention. Ravallion and Wodon (2010) investigate the impact of a take-home food ration provided to poor households with children regularly attending primary school in rural Bangladesh and report improved attendance and reduced child work. Alderman et al. (2012) and Kazianga et al. (2012) study the impact of two FFE programmes – school meals and take-home rations – conditional on schooling in Northern Uganda and Northern Burkina Faso. Their findings indicate a positive impact on primary school enrolment.

Skoufias et al. (2013) explore the impact of cash transfers *vis-à-vis* food transfers using the unique context of Program Apoyo Alimentario in Mexico; neither intervention had an impact on overall adult labour participation although the authors highlight a switch from agricultural to non-agricultural activities. Margolies and Hoddinot (2012) review the evidence of food aid on a number of outcomes and state that "…food aid does not appear to create dependency or present strong disincentives to labour" (p. 4) as also supported by research from rural Ethiopia (Abdulai et al., 2005); these perverse incentives might appear as people become wealthier as, if the programme is well-targeted, dependency should not be an issue (Barrett, 2006). Overall, I am not aware of studies explicitly investigating the impact of food transfers on intra-household allocation of time.

The scarcity of studies on the impacts of transfers on intra-household time allocation is explained by the tendency to focus on the primary objectives of programmes and individuals explicitly targeted by the intervention. Potential unintended or unforeseen effects – especially among household members who are not the specific target of the intervention (such as targeted siblings) – have been often ignored especially in the conditional cash transfer evaluation literature.

2.3 Context, programme description and evaluation design

2.3.1 Context: Karamoja

Geographically, the Karamoja sub-region is one of the poorest and remotest areas of Uganda located in the north-east, bordering with Kenya and South Sudan (see map in Appendix, Figure A.1). Historically, Karamoja has been at the centre of protracted armed conflicts related to cattle rustling and civil unrest which has made the region particularly vulnerable.[11]

Karamoja is not only characterized by high levels of insecurity, but is also the least developed area in Uganda. Physical infrastructure – including education and health facilities – is poor and employment opportunities are lacking (Harmer, 2012). According to the Uganda National Household Survey 2009/2010, 76 per cent of the population lived in absolute poverty in the North-Eastern region of Uganda (Karamoja, 74 per cent according to UNHS 2012/3), an extremely high rate especially when compared to the national average of 25 per cent in Uganda (UBOS, 2010). Karamoja is also plagued by chronic food insecurity (FEWS NET, 2016) and high levels of child malnutrition: 45 per cent of children under five are stunted compared to a national average of 33 per cent (UBOS and ICF international Inc., 2011). Primary school enrolment rates (net) are much lower in Karamoja (35.5 per cent) compared to the national average (79.5 per cent) according to the Uganda National Household Survey 2016/2017 (UBOS, 2017).

Seasonal patterns in Karamoja partly explain the high level of food insecurity experienced by the region, and the inability of local production to satisfy the population's food requirements throughout the year. Unlike the rest of Uganda, the region is unimodal, meaning that it has only one rainy season and therefore one harvest over a one-year period. The rainy season extends from April to October – though it has become increasingly erratic and unpredictable – and is followed

[11] Since the 1970s the use of small arms has proliferated. They have been used in cattle raids and to access resources and pastures but also in violence linked to cross-border invasions (from Kenya and Southern Sudan) and in inter-ethnic conflicts. Since 2011, which also coincides with the period of beginning of the period of the evaluation, a process of disarmament was started that lasted into 2015 and was characterized by different phases. In 2007, the Karamoja Integrated Disarmament and Development Programme (KIDDP) was implemented as a medium–term programme to help the disarmament and transition to recovery. In 2015, the disarmament component was dropped and the KIDDP was transformed into the KIDP (Karamoja Integrated Development Programme). One of the challenges experienced in the conflict management period was Karamoja's governance where the customary and formal roles of authorities overlapped without clear jurisdiction (FEWS NET, 2016).

by a hot and dry season. Crops are planted between April and May and are harvested between August and December whereas the lean season typically runs from March to July (see Figure A.2 in the Appendix). It is a shock-prone area, affected especially by chronic droughts and flash floods (FEWS NET, 2016).

For decades, Karamoja has received multiple external assistance programmes ranging from food assistance, health care and education to agricultural support and livelihood regeneration. In recent years there has been a shift from emergency to development initiatives aimed at strengthening local livelihoods, however food assistance remains important to satisfy the population's food requirements (FEWS NET, 2016). Among the main food programmes operating in the area are: the World Food Programme (WFP) General Food Distribution, the Karamoja Productive Assets Program (KPAP), the Maternal Child Health and Nutrition (MCHN) programme, and the Community-based Supplementary Feeding Programs.

The General Food Distribution is funded by the WFP and aims at improving nutrition and health conditions of very poor households in Karamoja. It provides food rations (including corn soy blend − CSB) and is targeted to extremely vulnerable and moderately food insecure members of the communities. The KPAP aims to encourage community asset-building with the potential to strengthen livelihoods. It provides poor able-bodied household heads (around 50,000 in 2010) with food-for-work; food transfers (including CSB) are provided conditional on participation in public works activities. The MCHN program is another nutrition intervention supported by WFP. The program is targeted to Pregnant and Lactating Women as well as children under 2 years old (aged 6-23 months) with the objective to prevent acute malnutrition (stunting), reduce the prevalence of anaemia and improving access to health, particularly antenatal, services. MCHN beneficiaries receive at health facilities a ration of specialized nutritious food baskets which consists of CSB, oil and sugar and amounts to roughly 1,200 calories per person per day (Gilligan et al., 2013; Gilligan and Roy 2015). Finally, the Community-based Supplementary Feeding Programs aim to treat moderate acute malnutrition by providing malnourished children and adults with nutrition/health sensitization and training activities as well as a basket of highly nutritious

foods, very similar to the food transfer ration targeted to ECD children as part of the IFPRI evaluation studied in this chapter. All of these programs were operating at baseline and ongoing throughout the full duration of the impact evaluation.

During the period of the randomized control trial used in this study (September 2010-May 2012), there were no significant atypical events. No major security events were recorded rather a process of disarmament was started in 2011 that lasted into 2015 (see footnote 11); civil security improved with less widespread and more opportunistic events. From a climatic point of view, however, 2011 was an exceptional year in Karamoja for rains; they started later and were exceptionally longer than usual extending well into October/November and resulting in better yields (Gayfer et al. 2012); conversely, dry conditions in the following year (2012) led to a below average harvest and anticipated the arrival of the lean season (FEWS NET, 2016).

2.3.2 The WFP food and cash transfer intervention linked to the UNICEF-supported ECD programmes[12]

The programme is comprised of two interventions: 1) UNICEF-supported community-run ECD centres and 2) WFP intervention to provide transfers – food or cash – to households with children enrolled in ECD centres.

Since 2007, UNICEF in collaboration with the government's District Education Offices (DEOs), supported ECD centres for preschool age children in an attempt to improve their school readiness and cognitive development. ECD centres are typically informal settings that operate 5 days a week between 8–11am; they are often run under a tree in the village and very poorly supplied. Each ECD centre has one to three caregivers who alternate over the week and lead activities, including singing and dancing, as well as learning numbers and local customs.

[12] The programme description and evaluation design section are based on the Official Evaluation Reports (Gilligan et al., 2011; 2013) and Gilligan and Roy (2015).

In April 2011, the WFP started providing transfers to households with children aged 3–5 linked to their participation in ECD centres in three districts of the Karamoja sub-region, Kaabong, Kotido and Napak.[13] These incentives, aimed at encouraging young children's attendance at ECD centres, were paid preferentially to a woman in the household, and initially were meant to be conditional on the child attending the ECD centre. The parents in intervention communities had been sensitized on the conditionality.

However, due to difficulties monitoring and enforcing such conditionality, transfers were made unconditional with the only requirement that the child was enrolled in the centre.[14] Given the initial messaging and the potential impact of the labelling of the programme as an education support programme for very young children, these interventions could also potentially be referred to as cash transfers with 'soft conditions' (see for instance Benhassine et al., 2015; Pace et al., 2016).

Transfers were either a take-home food ration or cash. The former consisted of a 6-week supply of multiple micronutrient-fortified food rations of approximately 1,200 calories per day per child; it included primarily corn soy blend (CSB) – which provides 99 per cent of daily iron requirements in addition to other nutrients – but also vitamin-A fortified oil, and sugar.

The cash transfer was equivalent to the estimated amount needed to purchase that same food basket in the market (Ugandan shillings/UGX 25,500 or roughly USD 12 over the 6 weeks). The 6-week transfer is equivalent to about 15 per cent of total household monthly consumption (UGX 171,200), therefore representing a 10 per cent increase over pre-programme average consumption per month.[15] These transfers are low compared to the 20 per cent threshold of baseline consumption that has been suggested as crucial to reach transformative impacts (see Davis and Handa, 2015).

[13] The districts chosen were those where UNICEF had an established presence in supporting ECD-centres.
[14] It is not clear however whether they had been informed once the conditionality had been removed.
[15] Around 67 per cent of beneficiary households in the study received a value below 20 per cent of baseline expenditures.

A household could receive multiple transfers if they had more than one child fulfilling eligibility criteria.[16] Cash transfers were paid electronically to cards provided to children's parents and redeemable at mobile money agents purposefully brought to cash villages to disburse transfers for the programme whereas food transfers were distributed by truck. Transfers were originally intended to be distributed in 6-week cycles, although in practice there were variations in the frequency (See Table A.1 in Appendix which indicates 6 to 8-week intervals over approximately 12 months). Even though the number of transfers received was comparable across the two treatment arms, cash tended to be delivered later than food.

2.3.3 Evaluation design[17]

From 2010–2012, the International Food Policy Research Institute (IFPRI) in collaboration with UNICEF and WFP, carried out an experimental evaluation of the food and cash transfers programme. The study was designed as a stratified cluster randomized trial, made possible due to funding constraints WFP faced in giving transfers to all potentially vulnerable households in the study area.

The unit of randomization is the ECD centre and the randomization is stratified at the district level for Kotido and Napak and at the subdistrict level for Kaabong; the stratification guarantees that within each stratum (11) the same number of ECD centres is assigned to each arm. Ninety-eight ECD centres (clusters) were randomly allocated to one of three arms: the food arm (35 ECD centres), the cash arm (31 ECD centres), and the control group which received no transfer (32 ECD centres). In order to foster transparency, in each district, ECD clusters were assigned to arms by public lottery.[18]

---

[16] It was intended that new children enrolling in (treatment) ECD centres would be added to the list of beneficiaries; this might have led some parents to start sending other children, not previously enrolled, to the ECD centre. It is not clear however whether in practice the inclusion of new enrolees to the beneficiary list occurred with regularity.
[17] See footnote 12.
[18] In each sub-/district, meetings were held in the presence of local officials/representatives of the WFP, Community Support for Capacity Development and DEOs. For each cluster, one of the persons present was asked to pick a bead out of a bag of coloured beads; where the number of beads was equivalent to the total number of clusters in the stratum and the colour of the bead determined the allocation to the control (white), food (red) or cash (yellow) arm.

All households with a child aged 3–5 enrolled in the same ECD centre were assigned the same intervention: either food, cash, or control; in order to avoid contamination, namely eliminate the possibility that children could easily move from an ECD centre allocated to an intervention to a nearby one assigned to a different treatment, centres very near to each other (1–2 km) had been grouped together and considered as a unique cluster.[19]

Based on sample size calculations, the evaluation aimed at sampling 25 households with a child aged 3–5 enrolled per ECD centre.[20] Out of the list of eligible children in each ECD census register, a group of 40–80 children was randomly sampled to allow for alternates.[21] In each sampled household, over the course of the baseline survey, a 3–5 year-old child enrolled in ECD was nominated as the Baseline Index Child (BIC); this sub-sample represents the children explicitly targeted by the intervention.[22]

2.3.4 Existing literature on the programme

Gilligan and Roy (2015) explored the impacts of the intervention on the primary objectives related to 3–5 year-old children, namely cognitive and non-cognitive development. While there is no impact on non-cognitive measures for either cash or food transfers, they find that cash transfers increase a total cognitive index by about 9 percentage points (0.33 standard deviations – SD) with respect to the control group. The authors suggest these impacts happen through a 'nutrition pathway', namely an improvement in children's diet quality and hygiene and a reduction in

[19] After this process, there remained 109 of the 128 originally identified UNICEF-supported ECD centres. During baseline fieldwork 16 ECD centres in Kotido district had to be dropped from the study as they were found to be run by Save the Children and therefore not eligible; only 6 of these clusters were replaced with additional ECD centres. A second randomization followed for the newly added clusters where allocation to arms was made proportional to the original treatment allocations of the ECD centres removed from the study. This resulted in a final sample of 99 ECD centres. After baseline, it was discovered that one of the ECD centres was a private nursery and it was therefore dropped leading to a final sample of 98 ECD centres. This reduced sample still satisfies the sample size calculations that determined that 30 to 40 clusters per treatment arm would be required to detect fairly small effects of the treatment arms on main outcome variables such as food security.

[20] In order to estimate spillover effects, in each cluster five additional households with at least one child aged 3-5 not enrolled in an ECD centre at baseline were sampled; these households however are not included in the sample I use to estimate treatment effects of the food and cash modalities.

[21] As siblings were rarely identified as such, alternates were used when two children from the same household had been sampled; in this case the household would be interviewed only once and a household from the list of alternates would be used. Alternates were also used when a household sampled was actually found not to have any child aged 3-5 enrolled at an ECD centre.

[22] In cases where there was more than one child aged 3-5 enrolled in an ECD centre, the BIC was selected randomly.

anaemia, which combined could lead to improved mental alertness.[23] Impacts also work through the 'stimulation pathway', as transfers also increased child preschool attendance that lead to more frequent and better quality stimulation.

Gilligan et al. (2014) focus on the relative impact of food and cash transfer modalities on household food consumption patterns. The authors find that cash transfers increase food and total consumption, including consumption of significantly more meat/seafood and eggs. Again, there is no impact for the food treatment.

Finally, Peterman et al. (2015) analyse the impact of the intervention on female decision-making power within the household; the food treatment does not have any significant impact whereas the cash transfer has a significant impact on the measures of sole or joint decision-making but not on measures related to only sole decision-making.

In general, previous studies have found a fairly consistent lack of – or limited – impacts of the food intervention on the outcomes considered which seems at odds with the food transfer literature. Gilligan et al. (2014) highlight that food beneficiaries had received the last transfer much earlier than cash beneficiaries and had therefore probably already depleted it by the time of endline data collection.[24] Authors also suggest that the food basket was not highly valued in these 'cash strapped' communities where food assistance is provided through a series of different channels and programmes in the area; finally, these primarily corn-soya-blend (CSB) take-home food rations are not regularly available or typically sold in local markets making it difficult for food beneficiary households to resell for cash.[25]

It is therefore difficult to draw firm conclusions on whether the null impacts for the food intervention reflect its ineffectiveness or relate to a lack of enforcement or implementation issues.

---

[23] Gilligan and Roy (2015) do not report on impacts on child anthropometry however the official evaluation (Gilligan et al., 2013) reports that neither the food nor the cash treatment had significant impacts on anthropometric measures; some exceptions refer to the cash transfer in certain age groups.

[24] Moreover, the seventh and final food transfer took place after the endline survey.

[25] Endline data indicates that virtually no food recipient household had sold the transfer in order to buy non-staple food, non-food goods or to repay debts. Moreover, in order to check whether there could be a food black market, I would want to check if the household incomes of the food arm show any variability correlated with the timing of receipt of the food transfer. However, I lack a measure of household income and I cannot use consumption as I expect it to be correlated with the timing of receipt of the food transfer.

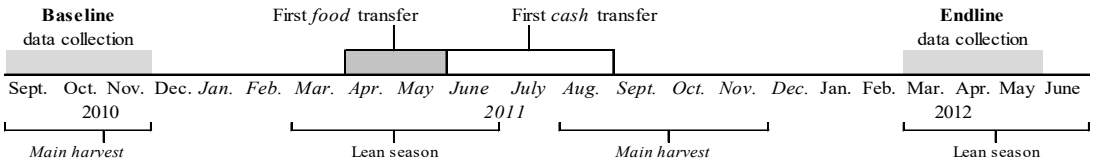
For this reason, I report impacts disaggregated by transfer modality but primarily focus on the cash impacts.

2.4 Data

2.4.1 Data collection, survey instruments and study samples

From September to November 2010, a baseline survey was conducted in study areas prior to the start of the intervention. The first transfer cycle was delivered between April and August 2011, with some slight variation by geographic area and type of transfer (see Figure 2.1 and Table A.1 in the Appendix). The follow-up, or endline, survey was conducted between March and May 2012. Longitudinal data was therefore collected at baseline and then after 18 months; at endline, households had been receiving transfers for roughly a year. Baseline data collection was conducted during the harvest season, whereas endline took place during the lean season; this seasonal trend in the data is captured by the control group. The baseline sample consisted of 2,560 households with an enrolled child aged 3–5; at endline 2,357 households were re-interviewed.

Figure 2.1: Evaluation timeline



The survey instruments encompass four main components: a household questionnaire, a child assessment, an ECD caregiver questionnaire and finally, a community questionnaire. My analysis relies mainly on the detailed household questionnaire, which was administered to the household member most knowledgeable on the topic. It gathers demographic and socioeconomic characteristics of the household, information on (food and non-food) consumption, dwelling characteristics, and some information on main livelihood strategies including livestock, productive assets, land and crop patterns. The survey instrument did not collect information on

household agricultural production overall (the value of harvest or the amount sold) or on use of inputs.

At the individual level, information was collected on: young children (3–6 years old) participation in ECD centres, as well as their involvement in chores and domestic work; the schooling of children 6 to 17 years-old and their participation and time spent in productive and reproductive work; the time use of adults, including their participation in different economic activities, as well as time spent on domestic work, income generating activities and leisure; information on these last two activities was collected only at endline. Overall, and as discussed in more detail later on, time use data might not be comprehensively capturing all the time adults spend on daily activities; it captures on average around 9 hours per day. A complete list of household modules included in the questionnaire and the age group to which each module was administered at baseline and at endline is reported in Table A.2 in the Appendix. The impact evaluation underwent ethics approval at IFPRI and the Uganda National Council of Science and Technology (UNCST, Ref. Number SS 2513).

2.4.2 Main outcome indicators by age group

In this section, I discuss key outcome variables; a full list of outcomes is reported in Table A.3 in the Appendix.

Very young children

I aim to estimate impacts on ECD attendance and child work. I report on the impact of transfers on ECD enrollment and attendance for all children aged 3–5 years. Given that the programme had a demonstrated impact on ECD attendance and various primary objectives related to very young children, these results are shown for completeness and consistency only (Gilligan et al., 2015). I also have information on how many hours per day on average children aged 3–5 spent on four activities in the last seven days: 1) looking after younger children and babies, 2) doing other

chores outside or far from the home (such as gathering greens, collecting firewood, or carrying water); 3) doing other chores near or in the home (such as cooking, cleaning, washing clothes, or doing other domestic work); and 4) looking after livestock, which is the only productive/economic activity for which there is information in this age group. I use these questions to compute measures of participation and time spent on any of these specific activities, as well as any productive activity, any domestic activity and their combination (any work).

Primary school-age children

For primary school-age children, aged 7 to 12 at baseline, I have measures of schooling and child work. Schooling measures include whether the child is currently enrolled. For enrolled children, there are two further indicators capturing attendance and schooling expenditures: the first is defined as the number of days (or partial days) the child attended school out of number of days the school was open in the last four weeks, while the second is the total amount paid for schooling this year including fees,[26] uniforms, transport and supplies for that child.

Based on the questions that collect information on the average number of hours per day children spent in the last seven days on several activities, I construct measures of participation and time spent in 1) looking after younger children and babies; 2) looking after or caring for sick household members; 3) doing other chores outside or far from the home; 4) doing other chores near or in the home; 5) looking after livestock; 6) helping with other agricultural work on own land; and 7) doing wage work. I further aggregate the first four activities to look at domestic or reproductive work, and the last three to capture productive or economic activities. Further combining productive and reproductive work, I also computed a measure of the total number of hours worked on average per day on any type of work (productive or reproductive). There is no measure on leisure time for children.

[26] Although primary schooling is free in Uganda there might still be examination fees.

Prime-age adults

I construct key outcomes for working-age adults who are aged 18 to 59 at baseline. I compute indicators that capture whether the individual has been involved in the preceding 6 months in any of the following productive activities: agriculture on own land, looking after livestock, wage work, non-agricultural self-employment. I can also measure the hours spent on average per day in the last two weeks on 1) income-earning activities which include work in the four productive activities mentioned above; 2) domestic work that is defined as taking care of children, cooking, washing clothes, cleaning, taking care of other sick household members, gathering greens, collecting firewood, carrying water, and so on; and 3) leisure that captures activities such as spending time with family, visiting friends, attending celebrations, going to church and so on.

2.4.3 Study samples, descriptive statistics and baseline balance

Given the targeting of the intervention to households with very young children, at baseline the sample is large with 3,550 children aged 3–5. Figure 2.2 below shows the age distribution of household members at baseline. There are 5,111 children aged 6 to 17, out of which 3,338 are primary school-age children aged 7 to 12; finally, there are 5,269 prime age adults (18 to 59 years old).

Figure 2.2: Age distribution of 3 to 60 year-old household members (panel of households)

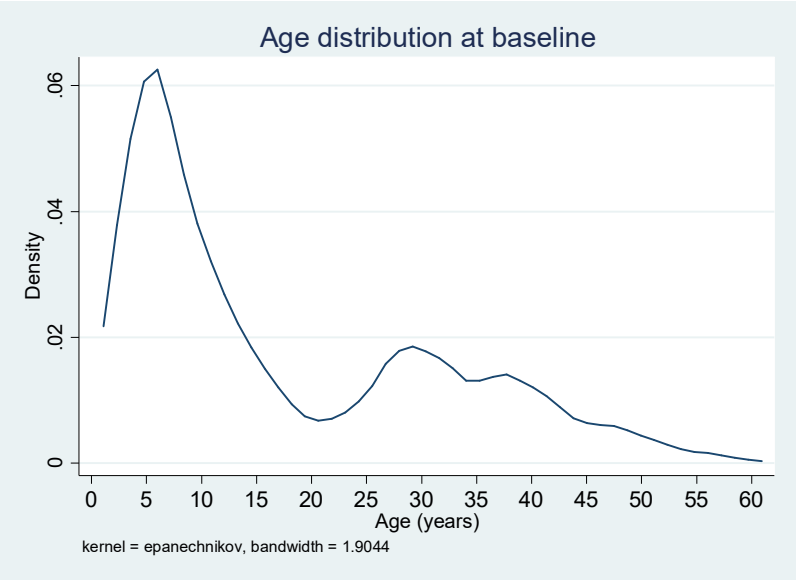


Table 2.1 reports the main characteristics at baseline for the panel of households. Mean household size is 6.3 with on average 4 children and 2 adults per household. Mean age of the household head is 40 and almost 90 per cent of households are male-headed; education in the sample is low with two–thirds of household heads in the sample who never attended school; 90 per cent of heads are married, more than half of them in polygamous marriages or unions. Around 70 per cent of household heads are from the Dodoth ethnic group; 20 per cent are Bokara (Karamojong) and 10 per cent from the Jie ethnic group.

Total monthly consumption per capita was low and around UGX 30,000 − roughly equivalent to USD 14[27] (or USD 0.5 per day) – and almost completely used for food. Around three-fourths of the households live in traditional manyatas (extended family settings often located close to farmed space and gated by a fence of briars). Living conditions are extreme: the vast majority of households live in dwellings with thatched roofs and a mix of soil and cow dung floors, have no improved access to water, and only half have a toilet. Around a third of households owns chickens and around 11 per cent owns cattle; almost all households have some farming implements (83 per cent). Table 2.1 also indicates that randomization was successful and baseline

[27] Using an exchange rate in October 2010 of 2,204 UGX/USD.

household characteristics are balanced across the three arms.[28] Out of 49 variables, only three are statistically significantly different between the cash and the control group and one between the food and the control arms ($p<0.05$). The fact that pre-programme characteristics are jointly uncorrelated with treatment assignment, whether food or cash, is confirmed by the joint tests of orthogonality[29] at the bottom of Table 2.1 ($p>0.1$).

Households in the sample were already beneficiaries of other assistance programmes at baseline. It is important to discuss differences in access to these programmes across arms in order to be confident that the estimated impacts shown in this analysis are causally driven by the intervention under analysis and not by these differences. Statistics in Table 2.2 support the presence of several programmes in the study area. On average, households received 1.3 programmes in the last 6 months for an overall value of UGX 37,460, equivalent to roughly USD 17; 83 per cent of households (not shown) received assistance from at least one programme. More than a third of households received the WFP general food distribution (UGX 10,198 per household or roughly USD 4.6), 29 per cent received the WFP Karamoja Productive Assets Programme (UGX 6,635 per household or roughly USD 3) and 27 per cent received other food transfers from NGOs or government (UGX 5,547 per household or roughly USD 2.5); however, none of the households were beneficiaries of any other cash transfer intervention. As programmes were operating in all study districts, I find no significant differences across the arms either in terms of coverage or value received from these programmes (see last two columns of Table 2.2) with the sole exception of the WFP general distribution that is received by a significantly higher proportion of households in the cash arm as compared to the control (39 versus 32 per cent, $p<0.05$).

[28] I also checked baseline balance for these same household-level characteristics in the various sub-samples used in this paper (namely, young children aged 3-5, primary school–age children 7–12 and working–age adults 18-59). The same conclusion of successful randomization applies.

[29] I regress each treatment status on pre-programme background characteristics, accounting for strata fixed effects and adjusting standard errors to correct for stratified design and clustering at the ECD level. I then test whether pre-programme background characteristics are jointly significant.

Table 2.1: Baseline background characteristics of panel households, by treatment arm

	All	Control	Cash	Food	Cash vs. Control	Food vs. Control
	Mean	Mean	Mean	Mean	p-value	p-value
Household head						
Age (years)	39.54	40.11	38.34	40.10	0.00***	0.86
Female	0.11	0.13	0.11	0.09	0.57	0.08
Education: None	0.68	0.66	0.68	0.69	0.32	0.14
Marital status:						
Never married	0.00	0.01	0.00	0.01	0.32	0.60
Polygamous	0.54	0.51	0.55	0.56	0.15	0.13
Monogamous	0.38	0.40	0.37	0.36	0.25	0.21
Divorced/separated	0.01	0.01	0.01	0.01	0.42	0.51
Widowed	0.07	0.08	0.06	0.07	0.61	0.94
Ethnicity:						
Dodoth	0.69	0.69	0.68	0.71	0.74	0.78
Karimojong (Bokara)	0.19	0.19	0.20	0.17	0.39	0.82
Jie	0.09	0.09	0.11	0.09	0.09*	0.48
Household level						
Household size	6.32	6.35	6.21	6.39	0.21	0.81
Number of members aged 0 2	0.79	0.77	0.81	0.79	0.33	0.77
Number of members aged 3–5	1.39	1.37	1.43	1.36	0.07*	0.79
Number of members aged 6–13	1.66	1.68	1.57	1.73	0.18	0.47
Number of members aged 14–17	0.34	0.37	0.32	0.33	0.08	0.33
Number of members aged 18–59	2.06	2.06	2.03	2.09	0.35	0.67
Number of members aged 60 or older	0.08	0.10	0.05	0.09	0.01**	0.69
Monthly consumption per capita, ('000 UGX)						
Total	31.14	28.77	27.06	36.73	0.53	0.39
Food	27.99	25.64	23.99	33.48	0.49	0.40
Non–food	3.16	3.12	3.07	3.25	0.96	0.63
Dwelling characteristics						
Manyata-style dwelling	0.76	0.77	0.81	0.72	0.18	0.88
Roof: Thatched/vegetable matter/sticks	0.90	0.88	0.91	0.90	0.34	0.91
Floor: Cow dung/soil mix	0.77	0.76	0.75	0.78	0.64	0.03**
Main source of lighting: Fire	0.83	0.81	0.84	0.83	0.61	0.91
Drinking water source: Borehole, well, spring	0.87	0.86	0.87	0.89	0.73	0.38
Sanitation: No toilet	0.46	0.48	0.41	0.49	0.00***	0.90
Pastoralist household	0.10	0.11	0.08	0.10	0.27	0.94
Livestock, productive and domestic assets owned:						
Oxen	0.05	0.04	0.03	0.06	0.37	0.11
Bulls	0.05	0.05	0.04	0.06	0.96	0.52
Cattle	0.11	0.11	0.09	0.12	0.44	0.75
Calves	0.06	0.06	0.05	0.07	0.50	0.40
Sheep	0.11	0.11	0.10	0.12	0.84	0.57
Goats	0.38	0.16	0.17	0.76	0.74	0.26
Pigs	0.05	0.05	0.04	0.04	0.20	0.10
Chickens	0.34	0.36	0.31	0.35	0.10	0.60
Plough	0.23	0.22	0.23	0.25	0.19	0.17
Farming implements	0.83	0.79	0.82	0.86	0.28	0.05*
Bed/mattress	0.11	0.12	0.11	0.10	0.98	0.90
Table	0.06	0.05	0.06	0.05	0.23	0.72
Mobile	0.08	0.09	0.10	0.07	0.84	0.59
Radio	0.06	0.05	0.06	0.07	0.43	0.09*
Bicycle	0.06	0.06	0.05	0.07	0.28	0.14
Additional indicators used in mechanisms section:						
Any irrigated land	0.01	0.01	0.01	0.01	0.32	0.59
Irrigated land, logged	0.01	0.01	0	0.01	0.14	0.68
Any investment on land in the last 12 months	0.59	0.55	0.63	0.59	0.18	0.5
Land with investment in last 12 months, logged	0.66	0.65	0.68	0.66	0.73	0.9
N	2,357	737	760	860		
Joint orthogonality test (p-value)					0.919	0.891

Notes: P-values from OLS regressions (characteristic on treatment) with controls for strata and robust standard errors clustered at the ECD level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 2.2: Other transfers received by household in the last six months, by treatment arm

	All	Control	Cash	Food	Cash vs. Control	Food vs. Control
	Mean	Mean	Mean	Mean	p-value	p-value
N. of transfers received	1.30	1.24	1.34	1.32	0.36	0.49
Proportion of beneficiaries receiving:						
Remittances	0.02	0.03	0.01	0.02	0.00***	0.06*
WFP Karamoja Productive Assets Program (KPAP) or NUSAF	0.29	0.27	0.29	0.31	0.47	0.08*
WFP Maternal, child health and nutrition program (MCHN)	0.13	0.12	0.14	0.12	0.72	0.81
WFP general food distribution	0.36	0.32	0.39	0.38	0.03**	0.10
Other food transfer	0.27	0.27	0.30	0.24	0.92	0.26
Other cash transfer	0.00	0.01	0.00	0.00	0.36	0.14
Inheritances	0.01	0.01	0.01	0.01	0.51	0.46
Bride price or dowry payments	0.02	0.02	0.02	0.02	0.86	0.56
Transfer from loans or credit agency	0.01	0.01	0.01	0.00	0.85	0.19
Food transfers from friends or family	0.15	0.16	0.12	0.17	0.14	0.88
Cash transfers from friends or family	0.04	0.03	0.04	0.05	0.51	0.58
Total value transfers received ('000 UGX)	37.46	35.89	42.02	34.78	0.49	0.55
Value received by assistance programme ('000 UGX)						
Remittances	1.02	1.55	1.05	0.54	0.27	0.03**
WFP Karamoja Productive Assets Program (KPAP) or NUSAF	6.63	7.17	5.87	6.85	0.21	0.89
WFP Maternal, child health and nutrition program (MCHN)	2.52	2.39	3.8	1.52	0.48	0.04
WFP general food distribution	10.20	10.1	9.81	10.62	0.97	0.67
Other food transfer	5.55	6.37	5.46	4.92	0.18	0.20
Other cash transfer	0.14	0.02	0.13	0.26	0.17	0.29
Inheritances	3.62	3.04	2.08	5.49	0.87	0.80
Bride price or dowry payments	3.83	3.06	6.89	1.78	0.42	0.16
Transfer from loans or credit agency	1.12	0.41	2.04	0.93	0.00***	0.45
Food transfers from friends or family	1.11	1.21	0.81	1.3	0.05*	0.82
Cash transfers from friends or family	0.70	0.57	0.97	0.58	0.73	0.25
N	2,357	737	760	860		

Notes: P-values from OLS regressions (characteristic on treatment) with controls for strata and robust standard errors clustered at the ECD level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Hereafter, I describe the key outcomes at baseline for the analysis samples. Table 2.3 focuses on children aged 3–5. Around 93 per cent of children in this age group are currently enrolled in an ECD centre and the vast majority (88 per cent) attended at least some time during the school year. Children are involved in a number of activities in the past 7 days, including looking after younger children and babies (34 per cent), carrying out chores outside (25 per cent) or in the home (20 per cent); however, none of these activities is carried out for more than an hour per day. Overall key outcomes are balanced with the sole exception being the proportion of children currently enrolled in an ECD centre, which is significantly higher in the food arm.

Table 2.4 shows outcome indicators for primary-school children aged 7–12 in terms of schooling and work outcomes. Only half of these children (51 per cent) are currently enrolled in school and attend on average around half of the days in which the school is open (48 per cent). Half of the enrolled children had not attended in the previous four weeks; for children going to school in the reference period attendance is high and reaches 94 per cent. On average, school expenditures (including fees, uniform, transport, supplies) per child in a year were around UGX 5,744, or roughly USD 2.5; mean school expenditures in my sample across attending children only are almost double, UGX 10,834 or USD 4.9. Although school fees and Parent Teacher Association charges were abolished for primary schooling through Uganda's Universal Primary Education (UPE) programme in 1997, parents still need to contribute to the cost of scholastic materials, uniforms, examination fees and other costs for education (Riddell, 2003) which add to the opportunity cost of schooling.[30]

These children are involved in a number of household chores and economic activities (see Table 2.4). Over 90 per cent of children are involved in some type of work in the last seven days, be it domestic (88 per cent) or economic (57 per cent), and spend on average six and a half hours per day on it. Five hours are spent on household chores, mostly looking after younger children and babies (almost 2 hours) or carrying out chores outside (roughly an hour and 20 minutes) or

[30] Primary school is free; it includes seven years of education and is also often delayed: legal school entry is 6 years old even though in practice many children start later.

in the home (almost an hour and a half). The remaining hour and a half is spent on productive work, almost entirely on agriculture (an hour) and livestock (20 minutes).[31]

Overall, Table 2.4 indicates that key outcomes are balanced across treatment arms with only few exceptions: in the cash group, the time spent looking after younger children and babies is significantly lower than for children in the control arms; in the food treatment, the proportion of children caring for sick household members and looking after livestock is significantly lower with respect to the control group.

Table 2.5 combines schooling and work information and shows that only 5 per cent of children are enrolled in school and do not work, 46 per cent work only, 45 per cent combine school and work, whereas the remaining 3 per cent falls into the idle category; Figure 2.3 shows these four mutually exclusive combinations of school and work by age highlighting that the proportion of children who combine school and work tends to increase with age. Figure 2.4 shows how schooling and work vary by age; enrolment tends to increase up to 10 years old, whereas it starts to decrease already starting at age 11/12 indicating early dropout rates; domestic work is high and constant across ages whereas productive work tends to increase with age.

There are some gender differences among primary school-aged children (not shown). Boys are significantly more likely to be enrolled in school (61 versus 42 per cent), whether combined with work or not, and their attendance tends to be higher. Girls are more likely to work only (55 versus 37 per cent). A similar proportion of boys and girls is engaged in productive work (around 57 per cent), but significantly more girls perform domestic chores (93 versus 83 per cent): eight out of ten girls looked after younger children and babies, cared for sick household members, carried out chores in or outside the home compared to six out of ten boys.

Finally, Table 2.6 reports primary outcomes for working-age adults. Eight out of ten (not shown) adults report agriculture as the productive activity they spent most time on in the last 6 months, more than on livestock, wage work or non-agricultural self-employment. Approximately

[31] Here and after, time spent on each activity is a mean across all children and not only participants.

93 per cent of my sample had worked in agriculture on land operated by the household in the last 6 months. The second and third most common productive activities in the last six months are wage work (39 per cent) and non-agricultural self-employment, such as small business (27.5 per cent); only 14 per cent had spent some time looking after livestock.

At baseline, significantly more working-age women were involved in agriculture and non-agricultural self-employment than men (98 versus 88, and 34 versus 20 per cent respectively) whereas they were significantly less likely to be involved in looking after livestock or wage work (8 versus 20, and 37 versus 41 per cent respectively, not shown).

Overall, working-age adults tend to spend four hours and 20 minutes per day on domestic work. Information on time spent on income-earning activities was only collected at endline so I look at follow-up control means: at endline, adults spent on average per day around 3 hours and 20 minutes on income-earning activities and 2 hours and 40 minutes on leisure. Considering that adults in control areas spend around 2 hours and 45 minutes on domestic work at endline, total hours per day reported to be spent on domestic work, labour and leisure is 8 hours and 45 minutes which suggests some under-reporting or inability of the survey to comprehensively capture all time spent on daily activities (discussed further in the results section). Overall, however, Table 2.6 indicates that outcomes are balanced across treatment arms.

Table 2.3: Baseline key outcomes for children aged 3–5

	All	Control	Cash	Food	P-value 'Cash-control'	P-value 'Food-control'	N Control	N Cash	N Food
ECD outcomes									
Currently enrolled in ECD centre	0.93	0.91	0.91	0.95	0.70	0.01**	839	908	1,026
Attended any time during this school year	0.88	0.85	0.86	0.91	0.98	0.15	831	906	1,023
Work outcomes, last seven days									
Participation rates by activity									
Looking after younger children and babies	0.34	0.36	0.35	0.31	0.90	0.12	688	727	809
Looking after livestock	0.02	0.04	0.02	0.02	0.06*	0.11	687	723	813
Other chores outside or far from the home	0.25	0.26	0.26	0.24	0.90	0.79	689	728	813
Other chores near or in the home	0.20	0.23	0.22	0.17	0.88	0.15	688	725	812
Time spent by activity									
Looking after younger children and babies, logged hours	0.36	0.39	0.38	0.32	0.61	0.04***	688	727	809
Looking after livestock, logged hours	0.02	0.04	0.02	0.02	0.10	0.14	687	723	813
Doing other chores outside/far from home, logged hours	0.22	0.22	0.23	0.21	0.81	0.79	689	728	813
Doing other chores near or in the home, logged hours	0.18	0.20	0.19	0.15	0.78	0.13	688	725	812

Notes: Ns refer to individual panels. P-values are reported from Wald tests on the equality of means of cash and control for each variable. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Standard errors are clustered at the ECD level.

Table 2.4: Baseline key outcomes for children aged 7–12

	All	Control	Cash	Food	P-value 'Cash-control'	P-value 'Food-control'	N Control	N Cash	N Food
Schooling outcomes									
Currently enrolled	0.51	0.54	0.49	0.51	0.47	0.64	794	772	912
N. days attended school out of n. days school was open	0.48	0.50	0.46	0.48	0.55	0.76	775	759	898
Amount paid for schooling this year, logged	4.31	4.51	4.14	4.27	0.52	0.63	786	765	897
Work outcomes, last seven days									
Any work (productive or reproductive)									
Participates in any work	0.92	0.93	0.91	0.92	0.40	0.62	722	714	837
Total hours in any work, logged	1.79	1.86	1.74	1.78	0.17	0.36	704	697	806
Reproductive (or domestic) work									
Participates in reproductive/domestic work	0.88	0.90	0.86	0.89	0.23	0.69	722	714	837
Hours in reproductive/domestic work, logged	1.57	1.63	1.49	1.58	0.09*	0.47	717	709	832
Participation rates by activity									
Looking after younger children and babies	0.71	0.74	0.67	0.72	0.14	0.58	720	714	835
Looking after or caring for sick household members	0.25	0.31	0.24	0.21	0.16	0.04**	720	709	836
Doing other chores outside or far from the home	0.72	0.73	0.70	0.73	0.55	0.98	722	714	836
Doing other chores near or in the home	0.72	0.73	0.72	0.71	0.81	0.53	721	714	835
Time spent by activity									
Looking after younger children and babies, logged hours	0.87	0.91	0.79	0.89	0.03**	0.67	720	714	835
Looking after or caring for sick household members, logged hours	0.24	0.29	0.22	0.20	0.10	0.07*	720	709	836
Doing other chores outside or far from the home, logged hours	0.72	0.75	0.69	0.73	0.25	0.66	722	714	836
Doing other chores near or in the home, logged hours	0.74	0.75	0.75	0.75	0.97	0.98	721	714	835
Productive (or economic) work									
Participates in productive work	0.57	0.61	0.57	0.54	0.55	0.29	718	708	818
Hours in productive work, logged	0.66	0.71	0.64	0.63	0.45	0.33	708	697	808
Participation rates by activity									
Looking after livestock	0.08	0.10	0.09	0.05	0.49	0.01**	719	707	830
Helping with other agricultural work on own land	0.53	0.57	0.52	0.51	0.40	0.33	722	711	834
Doing wage work	0.05	0.04	0.03	0.06	0.51	0.43	711	701	812
Time spent by activity									
Looking after livestock, logged hours	0.12	0.14	0.14	0.08	0.95	0.09*	719	707	830
Helping with other agricultural work on own land, logged hours	0.55	0.61	0.52	0.54	0.24	0.38	722	711	834
Doing wage work, logged hours	0.05	0.05	0.04	0.06	0.57	0.37	711	701	812

Notes: Ns refer to individual panels. P-values are reported from Wald tests on the equality of means of cash and control for each variable. * $p<0.1$, ** $p<0.05$,; *** $p<0.01$ Standard errors are clustered at the ECD level.

Table 2.5: Baseline complementarity or mutual exclusivity of schooling and work for children aged 7–12 (%) by treatment

	All	Control	Cash	Food	P-value 'Cash-control'	P-value 'Food-control'
School only	5.45	5.22	6.17	5.01	0.65	0.90
Work and/or chores and school	45.30	48.88	41.64	45.38	0.25	0.54
Chores + work and school	*25.07*	*29.96*	*23.49*	*22.24*	0.19	0.12
Chores (only) and school	*18.54*	*17.59*	*16.45*	*21.21*	0.78	0.30
Work (only) and school	*1.69*	*1.34*	*1.76*	*1.93*	0.66	0.41
Work and/or chores only	46.10	43.52	48.83	46.01	0.39	0.64
Chores + work	*27.51*	*27.12*	*27.61*	*27.76*	0.93	0.90
Chores (only)	*16.29*	*14.31*	*17.62*	*16.84*	0.38	0.45
Work (only)	*2.3*	*2.09*	*3.52*	*1.41*	0.34	0.46
Idle	3.15	2.38	3.38	3.6	0.40	0.37
N	2,130	671	681	778		

Figure 2.3: Mutually exclusive combinations of school and work by age group (children aged 7–12) at baseline

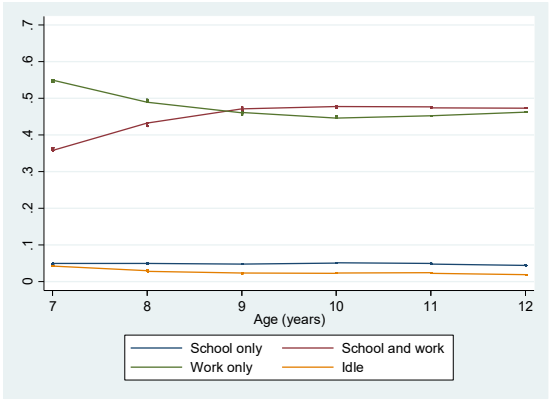


Figure 2.4: Mutually exclusive combinations of school and work by age group (children aged 7–12) at baseline

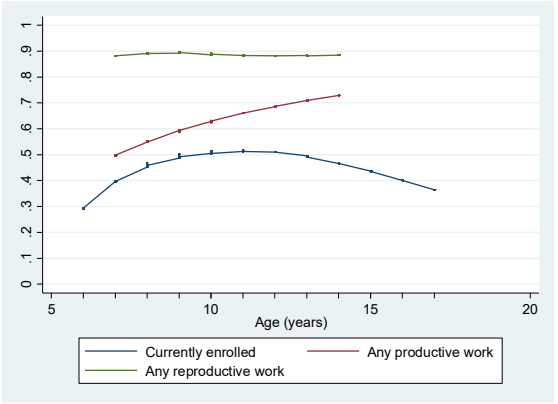


Table 2.6: Baseline key outcomes for adults aged 18–59

	All	Control	Cash	Food	P-value 'Cash-control'	P-value 'Food-control'	N Control	N Cash	N Food
Work outcomes									
Participation rates by activity, last six months									
Worked in agriculture on land operated by household	0.93	0.93	0.93	0.94	0.90	0.46	1,366	1,414	1,621
Spent time looking after livestock for the household	0.14	0.16	0.15	0.11	0.73	0.14	1,364	1,404	1,619
Did work that paid a salary or wages	0.39	0.38	0.43	0.36	0.36	0.64	1,364	1,407	1,620
Did work at non-agriculture self-employment	0.28	0.28	0.29	0.26	0.83	0.68	1,367	1,409	1,619
Time spent by activity, last two weeks									
Time spent on domestic work per day, logged hours	1.34	1.36	1.35	1.31	0.82	0.20	1,370	1,410	1,622
Endline (not collected at baseline)									
Time spent on income-earning activities per day, logged	1.32	1.27	1.50	1.21	0.00***	0.42	1,369	1,408	1,622
Time spent on leisure activities per day, logged hours	1.13	1.17	1.13	1.11	0.45	0.35	1,366	1,400	1,605

Notes: Ns refer to individual panels. P-values are reported from Wald tests on the equality of means of cash and control for each variable. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Standard errors are clustered at the ECD level.

2.4.4 Attrition

Overall attrition

Out of the 2,560 households sampled at baseline, 2,357 were successfully re-interviewed at endline. The overall attrition rate over 18 months is therefore 7.9 per cent. Table 2.7 reports the attrition rate by treatment arm. A higher proportion of households in the control group dropped out of the sample; however, differences in attrition rates between the treatment and the control arms are fairly small and not statistically significant.

Table 2.7: Attrition rate at the household level by treatment group

	All	Control	Cash	Food	P-value 'Cash-control'	P-value 'Food-control'
Attrition rate	7.93	9.35	7.32	7.23	0.34	0.30
N at baseline	2,560	813	820	927		

Notes: P-values are reported from Wald tests on the equality of means of Cash and Control for each variable.
Standard errors are clustered at the ECD level.

Tables 2.8 and 2.9 show differential attrition analysis, comparing baseline characteristics between treatment (food or cash) and control households that were lost to follow-up. The only statistically significant differences (at the 5 per cent level) are those relative to the level of non-food consumption per capita which is higher for control attritors than for intervention attritors. I conclude that benefits of randomization are preserved as I do not find any significant differential attrition after eighteen months among the other household characteristics tested (see columns 7 and 8 and test of joint orthogonality at the bottom of Tables 2.8 and 2.9). As attrition is random and does not lead to unbalanced samples, internal validity is maintained and I do not carry out further adjustments for attrition.[32]

[32] Attrition is higher at the individual level: 18 and 36 per cent in the working–aged adults and in the primary school–aged children samples respectively. There is however no evidence of differential attrition: the proportion of individuals lost to follow-up does not significantly differ between treatment (food or cash) and control arms and attritors do not differ in terms of pre-programme background characteristics and outcomes as is also confirmed by the respective joint orthogonality tests (primary school–aged children: p-value of 0.825 and 0.775 for cash and food respectively; working–age adults: p-value of 0.456 and 0.262 for cash and food respectively).

Table 2.8: Testing household differential attrition by baseline characteristics between control and cash arms

	Control			*Cash*			*Difference*	
	Attritors (1)	Non-attritors (2)	P-value (3)	Attritors (4)	Non-attritors (5)	P-value (6)	Col(1)-Col(4) (7)	P-value (8)
Household head								
Age (years)	37.43	40.11	0.01**	39.41	38.34	0.32	-1.97	0.16
Female	0.17	0.13	0.30	0.07	0.11	0.25	0.10*	0.06
Education: none	0.72	0.66	0.37	0.68	0.68	0.98	0.04	0.64
Marital status:								
Never married	0.01	0.01	0.56	0.00	0.00	0.16	0.01	0.30
Polygamous	0.49	0.51	0.77	0.59	0.55	0.46	-0.11	0.27
Monogamous	0.38	0.40	0.78	0.32	0.37	0.39	0.06	0.55
Divorced/separated	0.01	0.01	0.81	0.03	0.01	0.41	-0.02	0.44
Widowed	0.11	0.08	0.40	0.05	0.06	0.67	0.05	0.24
Ethnicity:								
Dodoth	0.71	0.69	0.77	0.76	0.68	0.25	-0.05	0.71
Karimojong (Bokara)	0.16	0.19	0.57	0.17	0.20	0.68	-0.01	0.92
Jie	0.09	0.09	0.99	0.05	0.11	0.15	0.04	0.63
Household level								
Household size	6.22	6.35	0.69	6.58	6.21	0.27	-0.36	0.43
Number of members aged 0–2	0.88	0.77	0.19	0.78	0.81	0.78	0.10	0.46
Number of members aged 3–5	1.42	1.37	0.29	1.40	1.43	0.68	0.02	0.80
Number of members aged 6–13	1.61	1.68	0.68	1.93	1.57	0.03**	-0.33	0.17
Number of members aged 14–17	0.25	0.37	0.13	0.35	0.32	0.73	-0.10	0.47
Number of members aged 18–59	2.04	2.06	0.80	2.08	2.03	0.52	-0.04	0.68
Number of members aged 60 or older	0.03	0.10	0.02**	0.03	0.05	0.37	-0.01	0.81
Monthly consumption per capita, (UGX '000)								
Total	40.84	28.77	0.29	27.41	27.06	0.92	13.43	0.25
Food	36.94	25.64	0.31	25.60	23.99	0.66	11.34	0.31
Non-food	3.90	3.12	0.30	1.81	3.07	0.01**	2.09**	0.03
Dwelling characteristics:								
Manyata-style dwelling	0.75	0.77	0.80	0.83	0.81	0.74	-0.08	0.48
Roof: Thatched/vegetable matter/sticks	0.82	0.88	0.14	0.87	0.91	0.49	-0.05	0.59
Floor: Cow dung/soil mix	0.80	0.76	0.34	0.82	0.75	0.21	-0.01	0.84
Main source of lighting: Fire	0.76	0.81	0.33	0.85	0.84	0.79	-0.09	0.34
Drinking water source: Borehole, well, spring	0.92	0.86	0.13	0.83	0.87	0.61	0.09	0.32
Sanitation: No toilet	0.42	0.48	0.40	0.48	0.41	0.35	-0.06	0.59
Pastoralist household	0.14	0.11	0.48	0.05	0.08	0.37	0.09	0.11
N	76	737		60	760			
Joint test of orthogonality (p-value)								0.222

Notes: Overall N for control is 813. Overall N for Cash is 820. *** p<0.01, ** p<0.05, *** p<0.1. T-tests based on standard errors clustered at the ECD level.

Table 2.9: Testing household differential attrition by baseline characteristics between control and food arms

	Control			*Food*			*Difference*	
	Attritors (1)	Non-attritors (2)	P-value (3)	Attritors (4)	Non-attritors (5)	P-value (6)	Col(1)-Col(4) (7)	P-value (8)
Household head								
Age (years)	37.43	40.11	0.01**	39.02	40.10	0.34	-1.58	0.26
Female	0.17	0.13	0.30	0.15	0.09	0.18	0.02	0.75
Education: none	0.72	0.66	0.37	0.72	0.69	0.67	0.01	0.93
Marital status:								
Never married	0.01	0.01	0.56	0.00	0.01	0.06*	0.01	0.30
Polygamous	0.49	0.51	0.77	0.58	0.56	0.73	-0.09	0.32
Monogamous	0.38	0.40	0.78	0.26	0.36	0.08*	0.12	0.21
Divorced/separated	0.01	0.01	0.81	0.03	0.01	0.26	-0.02	0.50
Widowed	0.11	0.08	0.40	0.14	0.07	0.10	-0.03	0.56
Ethnicity:								
Dodoth	0.71	0.69	0.77	0.85	0.71	0.01**	-0.14	0.26
Karimojong (Bokara)	0.16	0.19	0.57	0.12	0.17	0.20	0.04	0.70
Jie	0.09	0.09	0.99	0.02	0.09	0.08*	0.08	0.33
Household level								
Household size	6.22	6.35	0.69	5.99	6.39	0.08*	0.24	0.53
Number of members aged 0–2	0.88	0.77	0.19	0.72	0.79	0.29	0.17	0.13
Number of members aged 3–5	1.42	1.37	0.29	1.31	1.36	0.28	0.11*	0.05
Number of members aged 6–13	1.61	1.68	0.68	1.61	1.73	0.55	-0.01	0.98
Number of members aged 14–17	0.25	0.37	0.13	0.27	0.33	0.34	-0.02	0.85
Number of members aged 18–59	2.04	2.06	0.80	2.01	2.09	0.46	0.02	0.84
Number of members aged 60 or older	0.03	0.10	0.02**	0.06	0.09	0.29	-0.03	0.32
Monthly consumption per capita, (UGX '000)								
Total	40.84	28.77	0.29	34.81	36.73	0.89	6.04	0.65
Food	36.94	25.64	0.31	32.89	33.48	0.97	4.06	0.75
Non-food	3.90	3.12	0.30	1.92	3.25	0.01**	1.98**	0.03
Dwelling characteristics:								
Manyata-style dwelling	0.75	0.77	0.80	0.82	0.72	0.06*	-0.07	0.50
Roof: Thatched/vegetable matter/sticks	0.82	0.88	0.14	0.96	0.90	0.07*	-0.14*	0.05
Floor: Cow dung/soil mix	0.80	0.76	0.34	0.85	0.78	0.14	-0.05	0.53
Main source of lighting: Fire	0.76	0.81	0.33	0.87	0.83	0.38	-0.10	0.24
Drinking water source: Borehole, well, spring	0.92	0.86	0.13	0.96	0.89	0.01**	-0.03	0.44
Sanitation: No toilet	0.42	0.48	0.40	0.40	0.49	0.17	0.02	0.87
Pastoralist household	0.14	0.11	0.48	0.12	0.10	0.59	0.03	0.70
N	76	737		67	860			
Joint test of orthogonality (p-value)								0.778

Notes: Overall N for control is 813. Overall N for Food is 927. *** p<0.01, ** p<0.05, *** p<0.1. T-tests based on standard errors clustered at the ECD level.

2.5 Empirical strategy

In order to estimate the causal impact of the interventions, I run an Analysis of Covariance (ANCOVA) model as follows:

$$y_{it} = \beta_0 + \beta_1 Food_i + \beta_2 Cash_i + \beta_3 y_{it-1} + \beta_4 X_{it-1} + \theta + \varepsilon_i$$

where y_{it} represents the main outcome of interest measured for each individual/household i at time t (endline). $Food_i$ ($Cash_i$) is a dummy equal to 1 if the household/individual is in a cluster allocated to the food (cash) treatment, and 0 if allocated to the cash (food) or control arm. X_{it-1} is a set of basic control variables measured at baseline; at the individual level these consist of age and gender, whereas no additional control is used in household level specifications. θ denotes strata fixed effects. β_1 and β_2 capture the intent-to-treat (ITT) effect of being assigned to the food and cash arm respectively relative to the control group; it is then also possible to determine relative impacts between the food and the cash arm by testing whether β_1 and β_2 are statistically significantly different. Robust standard errors are corrected for the stratified design and clustering at the ECD level. Impact estimates are obtained through linear probability models for binary outcomes;[33] OLS is also used to estimate the impact on time spent on different activities.[34]

The ANCOVA model basically implies controlling for the baseline value of the dependent variable and is usually preferred to the more popular difference-in-difference (DiD) estimation when outcomes are only weakly correlated over time (β_3). In these cases, the ANCOVA model can lead to improvements in statistical power (the lower the autocorrelation, the larger the improvements) whereas it becomes inefficient to fully correct for baseline imbalances between treatment and control with DiD[35] (McKenzie, 2012). Outcomes are only weakly correlated in my samples with the highest auto-correlation value equal to 0.41. I therefore

[33] Angrist and Pischke (2009) and (Hellevik, 2009) suggest that the linear probability model and the probit provide very similar results when the aim is to capture the average effect.
[34] The amount spent on any activity is zero for individuals who do not engage in such activity. OLS estimations are run on all individuals, whether or not undertaking the activity. Hours are logged using log (1+time spent on activity).
[35] Intuitively, when the auto-correlation is low, a relatively small baseline imbalance between two treatment arms is not predictive of the difference between treatment and control at follow-up.

present the ANCOVA results as the preferred estimates even though alternative estimations are reported in the Appendix as robustness checks as discussed further in Section 2.8.

To counteract the problem of multiple hypothesis testing – as I am estimating impacts on a high number of outcome indicators – I report the significance of results based on corrected p-values obtained using the Sidak-Bonferroni adjustment[36] (Abdi, 2007) per family of outcomes; this approach allows to control the family-wise error rate (FWER), or the probability of making one or more false rejections. I present both unadjusted p-values as well as p-values adjusted for multiple comparisons; impact estimates that remain significant (at the 5 per cent level) even after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.

When the dependent variable is measured in logarithms – namely logged hours, I take the exponential of the effect estimate and subtract one to derive the impact size. To convert the impact estimates back to the original metric (i.e. hours), I start from the geometric mean of the dependent variable in the data scale when all independent variables are zero (as discussed in Newson, 2003).

2.6 Results

In discussing the results, I will focus primarily on cash effects. Null results for food transfers might be driven either by a lack of enforcement or by the ineffectiveness of food transfers, and it is therefore difficult to draw conclusions. Moreover, focusing on cash impacts facilitates examination of the unintended or unforeseen consequences of the programme.

[36] $adjusted\ \alpha = 1 - (1 - unadjusted\ \alpha)^F$, where F indicates the number of family tests, namely the number of family outcomes within the domain.

2.6.1 Very young children (aged 3–5)

Previous studies have explored in detail the positive impacts of the cash transfer on primary outcomes including ECD participation, cognitive and non-cognitive development in early childhood (Gilligan et al., 2015). ANCOVA impact estimates on ECD attendance for preschool aged children 3–5 at baseline are consistent with this existing evidence[37] and are reported for completeness in Table 2.10. Having a 3–5 year-old child enrolled in an ECD centre is part of the eligibility criteria for the household; the positive cash impact on ECD enrollment (column 1) therefore indicates that the intervention has enabled households to register other young children – previously out of school – in ECD centres.

In Table 2.11, I report the impact estimates on economic activities and household chores of very young children aged 3–5 at baseline which have not been studied elsewhere. I find no significant impact of either the cash or the food arm on young children's activities and chores, both in terms of extensive and intensive margins, once I control for multiple testing.

Table 2.10: Impact of cash transfers on ECD outcomes for children aged 3–5 at baseline

	Currently enrolled in an ECD centre (1)	*Attended any time during this school year* (2)
Food impact	0.01	0.05
	(0.02)	(0.03)
Cash impact	0.04**	0.09***
	(0.02)	(0.03)
R-squared	0.06	0.07
N	2,773	2,760
Baseline control mean	0.906	0.854
P-value: Food=Cash	0.05	0.02

Notes: Estimations use ANCOVA modelling among panel children aged 3–5 at baseline. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable.

[37] The sample I focus on is larger than in Gilligan et al. (2015) who focused only on BICs who had been administered the child assessment questionnaire. I also use a different methodology (ANCOVA rather than single difference), however the significance and direction of impacts is consistent with previous findings.

Table 2.11: Impact of cash transfers on labour outcomes for young children (age 3–5 at baseline)

	Any work		Productive work		Reproductive work		Looking after younger children and babies		Doing other chores outside or far from the home		Doing other chores near or in the home	
	Part. (1/0) (1)	Hours (logged) (2)	Part. (1/0) (3)	Hours (logged) (4)	Part. (1/0) (5)	Hours (logged) (6)	Part. (1/0) (7)	Hours (logged) (8)	Part. (1/0) (11)	Hours (logged) (12)	Part. (1/0) (13)	Hours (logged) (14)
Food impact	-0.03	-0.02	0.01	0.01	-0.03	-0.03	*-0.06**	*-0.07**	0.04	0.03	-0.01	0.01
	(0.03)	(0.05)	(0.01)	(0.01)	(0.03)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
Cash impact	0.02	0.02	*0.01**	0.01	0.02	0.02	-0.01	-0.03	0.04	0.02	*0.06**	*0.05***
	(0.03)	(0.04)	(0.01)	(0.01)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)
R-squared	0.06	0.07	0.01	0.01	0.06	0.08	0.06	0.06	0.05	0.06	0.05	0.05
N	2,230	2,213	2,223	2,223	2,231	2,217	2,224	2,224	2,230	2,230	2,225	2,225
Baseline control mean	0.459	0.633	0.041	0.037	0.457	0.619	0.358	0.393	0.257	0.223	0.225	0.201
P-value: Food=Cash	0.11	0.33	0.55	0.71	0.09	0.31	0.19	0.36	0.80	0.79	0.03	0.15
Unadj. p-value: Food impact=0	0.33	0.59	0.27	0.31	0.33	0.54	0.08	0.07	0.19	0.20	0.69	0.66
Adj. p-value: Food impact=0	0.99	0.99	0.98	0.99	0.99	0.99	0.65	0.59	0.92	0.94	0.99	0.99
Unadj. p-value: Cash impact=0	0.51	0.59	0.07	0.15	0.45	0.62	0.72	0.41	0.15	0.38	0.07	0.04
Adj. p-value: Cash impact=0	0.99	0.99	0.60	0.86	0.99	0.99	0.99	0.99	0.86	0.99	0.56	0.39

Notes: Estimations use ANCOVA modelling among panel children aged 3–5 at baseline. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable. Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Productive work captures one activity only: looking after livestock. Reproductive work includes looking after younger children and babies, doing other chores outside or far from home (such as gathering greens, collecting firewood, or carrying water) and doing other chores near or in the home (such as cooking, cleaning, washing clothes, or doing other domestic work). 'Any work' combines productive and reproductive work. The reference period for all activities is the last seven days. Hours are meant per day.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

2.6.2 Primary school-age children: schooling and work impacts

In Table 2.12, I report the impact of transfers on school enrolment, attendance and investment in schooling among primary school-age children. There are no significant impacts on any of these indicators for either transfer modality: there is neither a positive nor a detrimental effect on schooling outcomes for this cohort of children.[38]

Table 2.12: Impact of cash transfers on schooling outcomes for primary school-age children (age 7–12 at baseline)

	Currently enrolled (1)	N. days attended school out of n. days school was open (2)	Amount paid for schooling this year, logged (3)
Food impact	-0.02	-0.03	-0.49
	(0.04)	(0.04)	(0.32)
Cash impact	-0.02	-0.01	-0.08
	(0.03)	(0.03)	(0.32)
R-squared	0.22	0.20	0.22
N	2,478	2,432	2,448
Baseline Control mean	0.538	0.495	4.510
P-value: Food=Cash	0.99	0.59	0.18
Unadj. p-value: Food impact=0	0.61	0.47	0.14
Adj. p-value: Food impact=0	0.94	0.85	0.36
Unadj. p-value: Cash impact=0	0.58	0.85	0.80
Adj. p-value: Cash impact=0	0.93	0.99	0.99

Notes: Estimations use ANCOVA modelling among panel children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Unadj.=Unadjusted; Adj.=Adjusted

To understand and unpack the lack of impacts on schooling – which seems in contrast with existing cash transfer literature – I explore the main reasons why children in my sample were not enrolled in school at baseline (see Table A.4 in Appendix). Lack of funds was reported only for 11.6 per cent of children.

The main constraint to schooling, reported for the vast majority (61.8 per cent), is competition with work, whether domestic (51 per cent), agricultural (3.8 per cent) or looking after

[38] I also considered 'ever enrolled' but no significant impacts were found.

livestock (7 per cent). These descriptive statistics suggest that the cash transfer might not be high enough to offset the opportunity cost of going to school.[39]

Finally, the lack of or inadequate infrastructure in these areas might be acting as a barrier to enrolment and participation; however, distance to school is reported as the main reason only for 2.1 per cent of children. As a further check, I estimated heterogeneous impacts on enrolment by distance to school at baseline, where distance is defined in minutes (above or below the median) to reach school and is collected for all children whether enrolled or not;[40] however, I find there is no differential impact.[41]

Table 2.13: Impact of cash transfers on labour outcomes for children aged 7–12 at baseline

	Any work		Productive work		Reproductive work	
	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)
	(1)	(2)	(3)	(4)	(5)	(6)
Food impact	-0.02	-0.03	-0.03	-0.07	-0.02	-0.01
	(0.02)	(0.06)	(0.05)	(0.07)	(0.02)	(0.05)
Cash impact	*0.03**	**0.17***	**0.27***	**0.31***	0.02	0.07
	(0.02)	(0.05)	(0.05)	(0.06)	(0.02)	(0.05)
R-squared	0.04	0.13	0.14	0.14	0.05	0.12
N	2,273	2,207	2,244	2,213	2,273	2,258
Baseline control mean	0.929	1.858	0.613	0.712	0.896	1.635
P-value: Food=Cash	0.01	0.00	0.00	0.00	0.05	0.12
Unadj. p-value: Food impact=0	0.32	0.56	0.50	0.30	0.37	0.86
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.07	0.00	0.00	0.00	0.25	0.14
Adj. p-value: Cash impact=0	0.75	0.04	0.00	0.00	0.99	0.95

Notes: Estimations use ANCOVA modelling among panel children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable. Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Productive work includes: looking after livestock, helping with other agricultural work on own land and doing wage work. Reproductive work includes: doing other chores outside or far from the home (such as gathering greens, collecting firewood, or carrying water), doing other chores near or in the home (such as cooking, cleaning, washing clothes, or doing other domestic work), looking after younger children and babies and looking after or caring for sick household members. 'Any work' combines productive and reproductive work. The reference period for all activities is the last seven days. Hours are meant per day.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table 2.13 reports impacts on participation and time spent in productive and reproductive activities for primary school-aged children (7–12 years) in the last 7 days. In the cash arm, overall,

[39] I also tested whether impacts on schooling vary depending on whether a household was initially labour constrained (not shown), which I defined as a household where at baseline working-age adults are less than a third (the median) of all household members. I find no evidence of significant differential impacts.
[40] The question used is "How many minutes does/would it take [NAME] to reach school by normal means?"; the average is around 40 minutes at baseline and it does not differ significantly between enrolled and not enrolled children.
[41] As reported in the robustness checks section, fixed effects at the cluster level – which inherently take care of time invariant differences such as the availability of and access to schools – confirm the lack of significant impacts on schooling.

there is no significant impact on the proportion of children involved in any type of work: the significance of the cash transfer estimate ($p<0.1$) does not survive the multiple hypothesis testing. However, children are spending significantly more time (18.5 per cent[42]) – an additional hour per day – on work, be it productive or reproductive, due to the cash grant; by endline, children in the cash arm spend on average 7 hours and 20 minutes on any work activity.

Once I disaggregate, it seems that such an impact is driven by a significant effect on productive rather than reproductive work. There is a large and significant positive impact on the proportion of children doing any productive activity of 27 percentage points accompanied by a significant positive impact in terms of time spent on these activities: children in cash beneficiary households spend around 30 per cent more time in productive activities compared to their control peers which translates to an additional 30 minutes per day compared to the baseline hour and a half. There is no significant impact on any of these activities for the food arm.

In Table 2.14 I further disaggregate productive and reproductive work by type of activity. This exercise shows that the positive impact found on children's productive work for the cash arm is driven by agricultural work[43] (specifications 3 and 4). Whereas in the control group the proportion of children helping with agricultural work has been decreasing over time – reflecting the seasonality in the data – in the cash arm it has increased (not shown). As a result, for children in cash beneficiary households only, I find a strong positive and highly significant impact of 29 percentage points in terms of extensive margin and a 31 per cent (26 minutes) impact in terms of intensive margin.

Specifications 7 and 8 (Table 2.14) also highlight the positive impact on chores outside the household in the cash arm. In terms of extensive margins, I find a positive and significant impact of 8.3 percentage points; children in cash beneficiary households also increased the time spent on these activities more than their control peers; however, the impact is relatively small in

[42] $e^{0.17}$ - 1=0.185 (or 18.5 %).
[43] Agricultural land is relatively close to the household residence/manyatas. Approximately 81 per cent of households owning land can reach the parcel within an hour (walking time) and 55 per cent have a parcel within 30 minutes from their residence/manyatas.

magnitude translating to 11 additional minutes per day. Chores outside or far from home were classified as reproductive work as it includes collecting firewood and carrying water which are commonly categorized under domestic chores; however outside the household also include gathering greens which could be assimilated with agricultural work making this finding consistent with the increase in children's productive work. Findings in Table 2.14 confirm the lack of overall impacts for the food arm.

To explore potential detrimental impacts of child work, I also investigated the impact of transfers on long working hours, here defined as working in either economic or household chores for more than 14 or 28 hours per week.[44] Long working hours are considered as hazardous work and are therefore used as a proxy for 'worst forms of child labour'. I find no significant impacts using either threshold (not shown).

The magnitude of the estimated effect of the cash transfer on the incidence of productive work is sizeable (29 percentage points or 44 percent). Virtually all existing literature on the impacts of cash transfers on child work have found no significant effects or a decrease, rather than an increase, in the incidence of work for children (de Hoop and Rosati, 2014a). The main study for comparison − also finding an increase in child work as a result of two unconditional cash transfers − is de Hoop et al. (2017). The authors show a significant impact of 5.5 percentage points (or 31 percent) on economic activities in Zambia and a 6.3 percentage points (or 44 percent) impact on agricultural work for the household in Malawi. Such comparisons place my results at the higher end of these absolute impact effects; however, as the initial incidence of child work is higher in the Karamoja sample, the impact relative to the baseline value in my analysis is the same as the one in Malawi (44 percent).

[44] In Multiple Indicator Cluster Surveys (MICS) run by UNICEF, child labour is defined as engagement in productive work for an hour or more for children 5 to 11 years old, for 14 hours or more for children aged 12−14 years, or as engagement in unpaid domestic chores for 28 hours or more per week (Dayıoğlu 2013). According to the Ugandan legislation, children under 14 years old are not allowed to work except for light work carried out under the supervision of an adult and for no longer than 14 hours per week.

Table 2.14: Impact of cash transfers on labour outcomes for children aged 7–12 at baseline, disaggregated activities

	PRODUCTIVE WORK *Last seven days*						REPRODUCTIVE WORK *Last seven days*							
	Looking after livestock		Helping with other agricultural work on own land		Doing wage work		Doing other chores outside or far from the home		Doing other chores near or in the home		Looking after younger children and babies		Looking after or caring for sick household members	
	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Food impact	-0.02	-0.03	-0.02	-0.04	-0.00	-0.01	0.01	0.04	-0.04	-0.02	0.01	0.02	-0.04	*-0.05**
	(0.02)	(0.03)	(0.05)	(0.06)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
Cash impact	0.02	0.02	**0.29*** **	**0.31*** **	0.01	-0.01	**0.08*** **	**0.10*** **	0.03	0.05	0.05	0.04	*-0.06**	*-0.06**
	(0.02)	(0.03)	(0.05)	(0.06)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)	(0.03)
R-squared	0.10	0.11	0.15	0.15	0.03	0.03	0.05	0.09	0.07	0.11	0.08	0.10	0.08	0.08
N	2,256	2,256	2,267	2,267	2,224	2,224	2,272	2,272	2,270	2,270	2,269	2,269	2,265	2,265
Baseline control mean	0.104	0.137	0.575	0.607	0.044	0.045	0.727	0.752	0.734	0.747	0.742	0.915	0.310	0.295
P-value: Food=Cash	0.01	0.06	0.00	0.00	0.71	0.66	0.01	0.08	0.03	0.06	0.31	0.62	0.42	0.63
Unadj. p-value: Food impact=0	0.35	0.29	0.66	0.53	0.94	0.52	0.59	0.26	0.19	0.56	0.87	0.72	0.22	0.10
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.99	0.99	0.99	0.86
Unadj. p-value: Cash impact=0	0.23	0.57	0.00	0.00	0.77	0.82	0.00	0.00	0.38	0.21	0.23	0.41	0.08	0.06
Adj. p-value: Cash impact=0	0.99	0.99	0.00	0.00	0.99	0.99	0.04	0.01	0.99	0.99	0.99	0.99	0.79	0.71

Notes: Estimations use ANCOVA modelling among panel children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable. Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics. The reference period for all activities is the last seven days. Hours are meant per day.

Doing other chores outside or far from the home includes activities such as gathering greens, collecting firewood, or carrying water; doing other chores near or in the home includes activities such as cooking, cleaning, washing clothes, or doing other domestic work.

Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

I also estimate impacts of the programme on the eight (mutually exclusive) combinations of school and work used in Table 2.5. Results are reported in Table 2.15. In the cash arm, there is a significant negative impact (-15 pp) on the proportion of children who combine school with some reproductive domestic work and a positive significant impact of almost the same magnitude (+16 pp) on the proportion of children who combine school with any work (both productive and reproductive); similarly, there is a significant negative impact (-9 pp) on the proportion of children who do chores only and a positive significant impact of almost the same magnitude (+10 pp) on the proportion of children who combine productive and reproductive work. Table 2.15 indicates null impacts for the food arm.

Overall cash impact estimates suggest a shift from helping at home with domestic work only to combining chores with economic work, whether accompanied or not with schooling. The increase in time spent working has not come at the cost of schooling conditional on being enrolled; results suggest instead that it might have been accompanied by a decrease in leisure time.

Due to lack of leisure data for children, I cannot explicitly test this hypothesis. As a second-best option, I calculated leisure time artificially. I deducted from total waking hours per day – assumed to be 16 – total hours worked in productive or reproductive activities and hours spent at school for children attending. I report the results on the impact of cash transfers on leisure. I use four definitions which vary depending on how the duration of the typical school day has been set (range from 5 to 8 hours per day). Findings in Table 2.16 suggest a reduction in leisure time for primary school-age children although the significance of cash impacts varies between a 5 and 10 per cent level depending on the specification or leisure definition.

Table 2.15: ANCOVA impact estimates on mutually exclusive combinations of school and work for children 7–12 at baseline

	SCHOOL ONLY	SCHOOL and WORK			WORK ONLY			IDLE
	School only	*School and chores (only)*	*School and work (only)*	*School and chores + work*	*Work only*	*Chores only*	*Work + chores only*	*Idle*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Food impact	0.00	0.02	0.00	-0.04	0.00	-0.00	0.01	0.01
	(0.01)	(0.04)	(0.00)	(0.04)	(0.01)	(0.03)	(0.04)	(0.01)
Cash impact	*-0.02**	**-0.15***	0.00	**0.16***	0.01	**-0.09***	**0.10***	-0.00
	(0.01)	(0.03)	(0.00)	(0.04)	(0.01)	(0.02)	(0.04)	(0.01)
R-squared	0.05	0.12	0.01	0.09	0.07	0.04	0.11	0.01
N	2,130	2,130	2,130	2,130	2,130	2,130	2,130	2,130
Baseline control mean	0.052	0.176	0.013	0.300	0.021	0.143	0.271	0.024
P-value: Food=Cash	0.03	0.00	0.85	0.00	0.66	0.00	0.01	0.39
Unadj. p-value: Food impact=0	0.96	0.62	0.62	0.32	0.69	0.92	0.81	0.49
Adj. p-value: Food impact=0	0.99	0.98	0.98	0.78	0.99	0.99	0.99	0.93
Unadj. p-value: Cash impact=0	0.07	0.00	0.77	0.00	0.43	0.00	0.01	0.87
Adj. p-value: Cash impact=0	0.26	0.00	0.99	0.00	0.89	0.00	0.04	0.99

Notes: Estimations use ANCOVA modelling among panel children 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable. Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.

'School only' refers to children who are currently enrolled in school and do not have any other work activity. 'Chores' refer to reproductive work only, whereas 'work' refers to productive work only.

Unadj.=Unadjusted; Adj.=Adjusted

Table 2.16: ANCOVA impact estimates on (artificial) leisure time for children aged 7–12 at baseline

	Leisure time *assuming: for children attending*			
	5 school hours per day	*6 school hours per day*	*7 school hours per day*	*8 school hours per day*
	Hours per day (logged)	Hours per day (logged)	Hours per day (logged)	Hours per day (logged)
	(1)	(2)	(3)	(4)
Food impact	0.072	0.060	0.034	0.017
	(0.063)	(0.066)	(0.068)	(0.071)
Cash impact	-0.104*	-0.122*	-0.141**	-0.155**
	(0.062)	(0.066)	(0.068)	(0.070)
R-squared	0.061	0.057	0.052	0.049
N	2,207	2,207	2,207	2,207
Baseline control mean	1.802	1.706	1.611	1.517

Notes: Estimations use ANCOVA modelling among panel children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable.
Leisure time is computed as total waking hours per day (assumed to be 16) minus total working hours – whether productive or reproductive – and hours spent at school for children attending. Hours spent at school per day are considered on average to be 5 (specification 1), 6 (specification 2), 7 (specification 3), or 8 (specification 4).
Unadj.=Unadjusted; Adj.=Adjusted

2.6.3 Prime-age adults: work and leisure impacts

Table 2.17 reports the results in terms of impacts on adult time use. There is a highly significant 9 percentage points impact of the cash transfer on the proportion of adults working in agriculture on land operated by the household in the last six months. While participation in agriculture decreased sharply between baseline and endline in the control group (from 93 to 79 per cent), it remained relatively stable in the cash arm (from 93 to 89 per cent, not shown). This finding provides some indication that the cash might have enabled households to work at a time of the year when they would not normally have done so.

For adults in cash beneficiary households, I also find a 25.3 per cent increase in the time spent on income-earning activities in the past two weeks (column 6, Table 2.17) – roughly an additional hour. Income-earning activities include agriculture but also looking after livestock, wage work, and non–agricultural self-employment. There is no sign of a compositional shift over time and agriculture at endline is still the activity households spend most time on (not shown).[45] In this context, the positive impact on hours in income-earning activities seems to strengthen the

[45] I find no evidence of a shift across labour activities. Overall, agriculture was the activity that the vast majority of working–age adults spent most time on at baseline and still is by endline. Reflecting seasonality however, the proportion of adults reporting agriculture as the activity they spent most time on decreased over time (from 83 per cent to 67 per cent).

former effect on the positive impact on participation in agriculture rather than suggesting that adults are engaging in newer activities after receiving cash transfers.

The significant increase in time spent in income-earning activities is not accompanied however by a parallel significant reduction in either domestic work or leisure time (columns 5 and 7). This could be driven by the fact that the instrument I use is not a detailed time-use survey and therefore might not comprehensively pick up all the activities that individuals spend their time on. It could indeed be that individuals reduce time spent in activities that I do not capture with the instrument, such as eating, sleeping, or personal care.

Overall, results provide consistent evidence that cash transfers did not create any perverse incentive that discouraged beneficiaries from working. On the contrary, more adults in cash beneficiary households work in agriculture and spend more time on income-generating activities compared to the control group.

Finally, the cash transfer has no impact on the proportion of adults who looked after livestock, did work that paid a salary or wages, or worked at non-agricultural self-employment in the last 6 months. Also, food transfers do not lead to any significant impact on any of the outcomes.

Table 2.17: Impact of cash transfers on time use outcomes for adults aged 18–59 at baseline

	In last six months…				*In last two weeks…*		
	Worked in agriculture	Spent time looking after livestock	Did work that paid a salary or wages	Non-agric. self-employed work	Domestic work	Income-earning activities †	Leisure activities†
	Part. (1/0)	Part. (1/0)	Part. (1/0)	Part. (1/0)	Hours per day (logged)	Hours per day (logged)	Hours per day (logged)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Food impact	0.01	0.02	0.00	-0.00	-0.00	-0.02	-0.04
	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)
Cash impact	**0.09*** **	-0.01	0.00	-0.00	0.03	**0.25*** **	-0.02
	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
R-squared	0.06	0.08	0.03	0.07	0.38	0.11	0.15
N	4,401	4,387	4,391	4,395	4,402	4,399	4,371
Baseline (•) control mean	0.929	0.161	0.380	0.278	1.363	1.266•	1.165•
P-value: Food=Cash	0.00	0.04	0.96	0.99	0.32	0.00	0.64
Unadj. p-value: Food	0.81	0.27	0.87	0.92	0.92	0.59	0.32
Adj. p-value: Food impact=0	0.99	0.97	0.99	0.99	0.99	0.99	0.99
Unadj. p-value: Cash	0.00	0.46	0.91	0.91	0.35	0.00	0.56
Adj. p-value: Cash impact=0	0.01	0.99	0.99	0.99	0.99	0.00	0.99

Notes: Estimations use ANCOVA modelling (†single-difference modelling for time spent on income-earning activities and leisure) among panel adults aged 18–59 at baseline. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, age and gender; ANCOVA estimations also control for the baseline value of the dependent variable. Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
•Control mean refers to endline rather than baseline in specifications 6 and 7.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

2.7 Potential mechanisms

To explain how cash transfers might lead to the impacts reported in previous sections, I hypothesize that the regular grant may be relaxing the liquidity constraints of poor households enabling them to invest more in productive activities.

Table 2.18 reports impacts related to investments in land in the last 12 months to explore this hypothesis. Almost all households in the sample own at least a parcel (1.6 on average at baseline)[46] and the vast majority (81 per cent) of these households can reach their parcel/s within an hour's walking distance.[47] There is a positive strong and significant impact on the proportion of households that report having invested in any of the parcels owned. Households in the cash arm are 11.7 percentage points more likely to invest in their land with respect to the control group, where investments include, for example, clearing, irrigating, fertilizing, fencing and so on. The proportion of households that invest in land decreases less in the cash group with respect to the control arm (not shown).

Unfortunately, I cannot distinguish what specific type of investment drives this impact as information on all investments in land is captured through a single question. Given the specific context of Karamoja, households might simply try to protect – for instance by fencing – their parcels and crops from being uprooted and stolen, or from livestock eating them. It is also possible that cash transfers might have enabled households to make small investments, for instance in agricultural inputs.[48] Endline data was collected during the lean season when typically, little agricultural work is done. However, cash transfers seem to allow farmers to use this time productively, perhaps in investments. There is no possibility of confirming this hypothesis due to the lack of information on timing of the investment. Table 2.18 also indicates no impact on land irrigated, which could be expected, given the relatively high fixed cost of this type of investment.

[46] Only 0.5 per cent at baseline and 5 per cent at endline do not own a parcel. Around 57−59 per cent owns one parcel only, 27−28 per cent owns 2 parcels, whereas 11−12 per cent owns more than 2 parcels.
[47] 55 per cent have a parcel within 30 minutes' walk from their residence/manyatas.
[48] As shown for instance in Handa et al. (2018); Daidone et al. (2014a; 2014b), Karlan et al. (2014); Seidenfeld and Handa (2011).

Table 2.18: Impact of cash transfers on investments in land in the last 12 months

	Any parcel irrigated	Any investment on land
	(1/0)	(1/0)
	(1)	(2)
Food impact	0.001	0.006
	(0.005)	(0.044)
Cash impact	-0.002	0.117***
	(0.005)	(0.044)
R-squared	0.00	0.06
N	2,357	2,357
Baseline control mean	0.012	0.548
P-value: Food=Cash	0.48	0.01

Notes: Estimations use ANCOVA modelling among panel households. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies and for the baseline value of the dependent variable. Both dependent variables refer to the last 12 months and any investment in land includes clearing, irrigating, fertilizing, fencing and so on.

I also investigated whether the intervention led to any change in terms of crop patterns. Results (not shown) suggest households continue to cultivate the same crops as before – mainly sorghum and maize – with no sign of moving from subsistence to cash or more profitable crops.[49] In the Karamoja context, it is more likely households might simply be trying to improve their food security, increasing agricultural production for home consumption, rather than actually trying to sell their crops for profit, however this cannot be verified in the data due to the lack of harvest sales information. To understand whether the cash transfer had any impact on household livelihood strategies – and in particular agriculture – I also estimated the impact on livestock, especially large animals that could be used to plough and improve tillage – and productive assets (agricultural tools); after controlling for multiple comparisons, I find no significant impacts on any of these indicators that could help explain the significant impact in participation in agricultural work.

2.8 Robustness checks

First, I make sure that any baseline imbalance across treatment and control arm does not bias my findings. I re-estimate all main specifications including those pre-program characteristics which were statistically significantly different at the 5 per cent level between the control group and the

[49] Sorghum might however be seen as a cash crop as it is used primarily for brewing local alcohol which can then be sold.

intervention groups in Tables 2.1 and 2.2; I also included non-food consumption per capita which resulted as lower across control attritors in Tables 2.8 and 2.9. Results remain unaltered (not shown).

Second, I report the results based on single differences using the endline cross-section for child work and schooling outcomes; this allows use of a larger sample and inclusion also of older children who are at higher risk of dropping out and more likely to work (ages 6 to 17 for schooling and 7 to 14 for activities and chores).[50] Results are reported in the Appendix in Tables A.5-A.7 and confirm my previous findings; however, the impact on chores outside or far from home does not survive the multiple hypothesis testing. I also use a different definition of work categories by including chores outside or far from home in productive rather than reproductive work; the main message from Table 2.13 remains basically unchanged with a significant impact on productive work even though the magnitude of the impact for participation is smaller at 9 rather than 27 percentage points (not shown).

Third, I try including the ECD cluster rather than strata fixed effects. Tables A.8-A11 in the Appendix report results for primary school-aged children and working-age adults: all results hold with the exception of child engagement in chores outside or far from home which loses its significance. The same pattern of results is found when including individual fixed effects (see Appendix Tables A.12-A.15). Moreover, several alternative methods – including Bonferroni, Sidak (1967); Holm (1979); Benjamini and Hochberg (1995) – were used to adjust the estimates of p-values as a robustness check (not shown), and results are consistent.

Fourth, I re-ran all estimations controlling for the number of children aged 3–5 at baseline, given households could in theory be receiving multiple transfers depending on the number of eligible children (aged 3–5) at baseline. Results (not shown) are robust.

[50] Given the experimental design of the study and the successful randomization that led to very similar control, cash and food households at baseline, the use of single-differences that basically compare mean outcomes of programme participants to mean outcomes of non-participants can be considered a rigorous approach. As a further check I compared ANCOVA and single-difference estimates for outcomes that were collected both at baseline and endline, and found consistent results.

Fifth, I re-estimate main specifications using probit and tobit modelling for binary and continuous variables respectively rather than ordinary least squares (OLS). Main findings (not shown) hold, although tobit impact estimates tend to be larger.

Sixth, I also compute Lee bounds (Lee, 2009) to further prove that individual-level differential attrition is not an issue and that results hold. Lee bounds rest on the assumptions of successful randomization of treatment and monotonicity.[51] Results are reported in Tables A.16-A.20 and show the impact estimates for outcomes for the primary school-aged children and the adults' samples together with their upper and lower bound. Results are largely robust.

Seventh, I test differential impacts by gender. As Ilahi (2000) highlights, time within the household is often not equally distributed by gender; indeed, as shown in Section 24.3, there is some gender specialization/division of labour.[52] The heterogeneous impact analysis by gender (not shown) indicates that the impact of the transfers on my primary outcomes does not significantly differ between boys and girls, men and women. I therefore conclude there is no evidence of cross substitution effects between females and males for the activities analyzed, either in terms of extensive or intensive margins.

I also investigate heterogeneous impacts by access to other assistance programmes in the area in order to further ensure that the estimated impacts are causally driven by the cash intervention and not by a cumulative impact of the cash transfer with other interventions. I investigate the heterogeneous impacts depending on whether the household at baseline was a beneficiary of any of the three major types of programmes in the area (namely, KPAP, the WFP general food distribution, or other food transfers); only around 30 per cent was not receiving any of these three programmes at baseline. I find no heterogeneous impacts on any of the outcomes studied, either for the food or for the cash intervention, suggesting there is no combined impact

[51] Monotonicity means that the treatment assignment affects sample inclusion in just one direction; this would mean that receiving the cash transfer makes inclusion either more or less likely for any observation.

[52] In Karamoja, girls and women tend to be disproportionately more responsible for domestic tasks and care activities (Czuba, 2012); women are also traditionally responsible for agricultural activities, even though some practices are shared (Stites et al., 2007). Boys and men are in charge of livestock: boys herd livestock while men guard them and protect them from raids (Stefansky Huisman, 2011; Stites et al., 2007).

of the transfer intervention under study and the cocktail of different assistance programmes already in place (not shown).

Finally, I estimated heterogenous impacts by intensity of treatment. As highlighted in the programme description section, households can receive as many transfers as the number of eligible children. At baseline, almost three quarters of my sample of households (72 percent) have only one child in the age range 3 to 5 years old enrolled in an ECD centre and are therefore expected to receive only one transfer, whereas the remainder of the households should be eligible for multiple transfers (24 percent of households have two enrolled children; 2 percent have three or four children whereas 2 percent have missing ECD enrollment data).

Due to this design feature of the program, the transfer level per household will vary according to the number of eligible children per household (and the per person transfer level will vary also according to the household size). As a result, and assuming these poor households pool resources, the impacts of the program may change according to the intensity of the treatment. Indeed, smaller households with a higher number of eligible children will effectively receive a larger transfer on a per capita basis potentially leading to larger impacts.

I estimated heterogenous impacts by intensity of treatment which is measured either as whether the household had more than one eligible child at baseline or by a dummy equal to one if the share of eligible children to household size is above the median. There are no significant heterogeneous impacts on primary school-age children and prime-age adults.

Overall, sensitivity analyses confirm results are robust with the exception of participation and time spent in chores outside the household which, depending on the specification used, are or are not significant. All the robustness checks run also confirm the lack of impacts on key outcomes of interest for the food intervention.

2.9 Discussion and conclusions

I use longitudinal experimental data to investigate whether transfers targeted to households with children aged 3–5 and linked to their participation in ECD centres, have an impact on intra-household allocation of time for other household members.

My findings indicate that after receiving cash transfers for approximately one year, there is a positive impact on adults' participation in agriculture, and on time spent on income-earning activities. The cash transfer has enabled households to invest more in productive activities, and in agriculture in particular. As endline data was collected during the lean season, these results suggest that the cash transfer relaxed constraints related to agricultural activity enabling adults to cultivate at times of the year when they might not normally have done so before.

I also find a positive and significant impact of the cash transfer on children's involvement in productive activities – mainly agricultural – of around 30 percentage points. The increase in child work does not seem to come at the expense of schooling; indeed, there is no significant impact on schooling outcomes, whether enrolment, attendance or schooling expenditures. There seems to be a shift from helping at home with domestic work to combining household chores with economic work, which suggests an increase in time spent on work at the expense of leisure time. The impact on time spent working in productive activities is relatively small however, as children spend on average half an hour more per day on these activities compared to their peers. There is no impact on hazardous work, proxied by long working hours.

My findings seem to be in contrast with most cash transfer literature which has usually found a positive impact on schooling (Baird et al., 2014) and no perverse impact on child work (de Hoop and Rosati, 2014a). So how do we explain the lack of schooling impacts and the increase in child work? Households in the sample are extremely poor. On one hand, the opportunity cost of going to school might be higher than the transfer the household receives, so that the transfer does not entirely compensate for the costs of schooling (de Hoop and Rosati, 2014a for further discussion). On the other hand, if the cash enables households to invest in productive activities,

then the opportunity cost of going to school for children might go up – as the returns to children's work increase (see Dammert et al., 2018).

For demand-side interventions to have an impact on schooling, there have to be enough primary schools in catchment areas and sufficient excess capacity to accommodate additional students (Orazem et al., 2008). However, school coverage is still a constraint, especially in remote areas of Uganda (UBOS and UCW, 2014) and I lack data on the excess capacity of primary schools. Households make decisions about their children's education on the basis of their perceptions of the returns to education, and as a result might decide to under–invest in schooling. On the other hand, parents might value child work as building experience and acquiring useful skills (on-the-job-learning).

Overall, there is no evidence of cross-substitution effects between child and adult labour supply. Findings rather suggest that older children may be helping their parents with the agricultural activities the household has been investing in. This is consistent with anecdotal evidence that children in Karamoja, as elsewhere in Uganda, accompany their parents during their daily activities, learning agricultural practices and livestock-rearing at an early age, such that "By the age of eight, they are fully tied into the family farming enterprise, and from the age of fourteen onwards they are responsible for their own cultivation and livestock" (Löwe and Phiona, 2017:13).

My findings of a positive impact on productive activities and child work are also consistent with those from de Hoop et al. (2017) who find that two government–run unconditional cash transfer programmes in Malawi and Zambia not only increased adult labour supply as a result of new productive opportunities, but also increased children's participation in domestic and economic work. Unlike my results, de Hoop et al. (2017) find these transfers still increased school attendance as well as a number of other child well-being indicators.

Results relating to adults' time allocation are consistent with Banerjee et al. (2017) and Handa et al. (2017). The cash transfer did not cause 'laziness' or an increase in welfare

dependency. Adults do not use the benefits to work less and increase their leisure consumption, suggesting that, for individuals in poor households, leisure might not be – as typically assumed – a normal good. I find no significant impact for adults on time spent on leisure. On the contrary, the cash transfer has had incentive effects on adult labour supply.

Overall, I find no impact on intra-household allocation for the food treatment arm. It cannot be completely excluded that results for food transfers might be driven by a lack of enforcement, rather than by the ineffectiveness of food transfers. I cannot therefore draw any firm conclusion on the relative merits of food versus cash-based assistance.

There are a number of limitations to this analysis. The survey instrument was not developed as a detailed time-use survey and does not comprehensively pick up all the activities that individuals spend their time on. Moreover, it lacks leisure data for children and agricultural production data. The timing of the surveys only allows investigation of relatively short-term impacts; however, effects can vary over the duration of the programme (King and Behrman, 2009). Finally, I do not know what specific activities children are doing in agricultural work which – according to ILO – is one of the most hazardous sectors in which to engage in at any age. Although it is usually assumed that working on the family farm does not entail the risks faced by children working on larger commercial firms, and might even be beneficial, I cannot completely rule out the possibility of hazardous work.

Despite these limitations, my empirical findings hold under a number of robustness checks. To sum up, I find that the cash transfer – although not framed as a livelihood programme – improved household productive investments and the labour supply of poor adults. There is also a positive impact on children's workloads as children help their parents with agricultural activities. This effect however does not affect their school enrolment or attendance but rather seems to come at the expense of leisure time. To explore intended as well as unintended impacts of interventions, it is important to provide a complete picture at the household level.

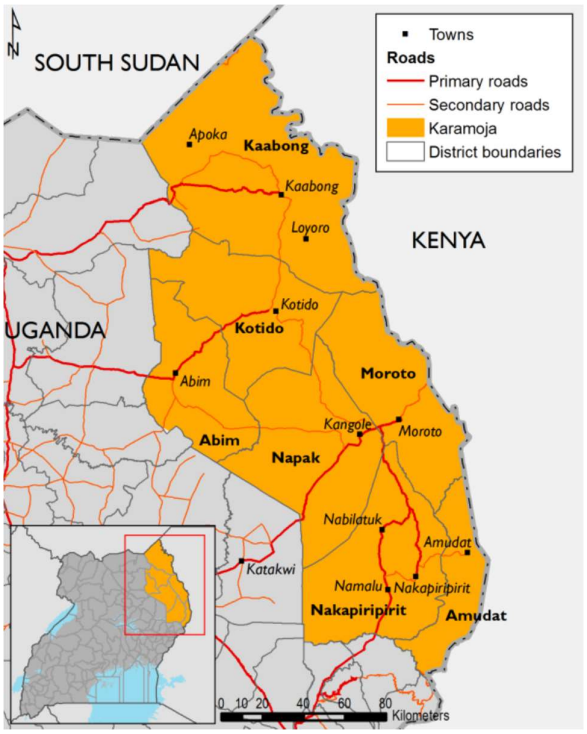
This study highlights that we do not know enough to draw clear policy conclusions; more research is necessary to understand the level of cash transfer needed to improve schooling and work outcomes among the most vulnerable and disadvantaged children. Household income should be raised above the subsistence threshold (Basu and Van, 1998). More efforts are also needed to understand how to better design programmes – besides using conditionalities and larger transfers – such that they encourage human capital investment overall for the household, without children engaging in hazardous labour.

If constraints to schooling beyond income are clearly identified, then 'cash plus' interventions could be considered to achieve more successful outcomes. Indeed, 'cash plus' interventions provide beneficiary households with regular cash transfers linked to other health or educational services and/or including complementary activities to influence beneficiary behaviour or knowledge (Roelen et al., 2017). Moreover, for a comprehensive child impact assessment, and to understand what is in children's best interests in the long term, broader indicators of child well-being should be collected and analysed together with further information on the type of work carried out in order to be able to distinguish the worst forms of child labour.

To conclude, it is essential from a policy perspective to be aware of these unexpected impacts and explore intra-household spillover effects; in this study the impacts are mostly benign but this might not always be the case. Notwithstanding the primary goal of the programme, they are likely to influence the long-term impacts and success of the programme, as well as to inform the design of future programmes.

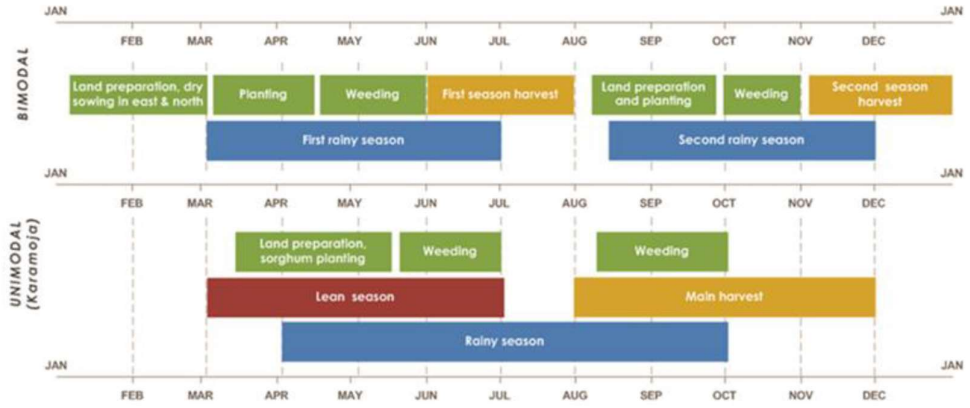
Appendix A

Figure A.1: Uganda and Karamoja sub-region maps



Source: FEWS NET (2016)

Figure A.2: Seasonal calendar for bimodal and unimodal seasons, Uganda.



Source: FEWS NET (2016)

Table A.1: Dates for distribution of food and cash transfers, 2011−2012

	Kaabong		Kotido		Napak	
	Food	**Cash**	**Food**	**Cash**	**Food**	**Cash**
Cycle 1	3 April - 9 May 2011	13 - 22 Aug 2011	19 - 20 April 2011	7 - 12 June 2011	8 - 15 April 2011	15 - 23 June2011
Cycle 2	7 June - 14 July 2011	5 - 28 Sept 2011	6 - 15 June 2011	23 June - 8 July 2011	11 - 16 June 2011	6 -18 July 2011
Cycle 3	5 Aug - 20 Sept 2011	2 Nov - 22 Dec 2011	9 - 10 Aug 2011	7 - 9 Sept 2011	15 -17 Aug 2011	8 -15 Sep 2011
Cycle 4	28 Nov - 18 Dec 2011	2 Nov - 22 Dec 2011	8 - 10 Nov 2011	22 - 25 Nov 2011	7 - 18 Oct 2011	22 - 30 Nov 2011
Cycle 5	28 Nov - 18 Dec 2011	18 Feb - 8 March 2011	12 - 13 Dec 2011	17 - 30 Jan 2012	13 - 14 Dec 2011	12 -17 April 2012
Cycle 6	31 Jan - 21 Feb 2012	17 - 30 April 2012	29 Feb 2012	11 - 13 April 2012	9 - 14 Feb 2012	12 -17 April 2012
Cycle 7	12 - 29 Aug 2012	26 July - 16 Aug 2012	30 July - 2 Aug 2012	18 - 24 July 2012	6 - 9 Sept 2012	20 - 27 Aug 2012

Source: Gilligan et al. (2013).

Table A.2: Household survey modules administered at baseline and/or endline

HOUSEHOLD SURVEY TOPICS	**Baseline**	**Endline**
Household identification, location, interview details	X	X
Consent	X	X
Household roster and demographics	X	X
Participation in ECD centres [children aged 3−6[53]]	X	X
Experience with transfers		X
Schooling [children aged 6−17]	X	X
Activities and labour force participation [household members aged 15 and older]	X	X
Activities and labour force participation [children aged 7−14]	X	X
Activities and labour force participation [children aged 3−6]	X	X
Dwelling characteristics	X	X
Health	X	X
Child health and development [children aged 3−6][53]	X	X
Health knowledge	X	X
Consumption habits	X	X
Food consumption and expenditures	X	X
Food consumption of young children [children aged 1−6][54]	X	X
Markets and purchasing behaviour	X	X
Non-food consumption and expenditures	X	X
Assets: Land, livestock, durables, savings and credit	X	X
Other transfers	X	X
Shocks	X	X
Budgeting behaviour	X	
Women's status / decision-making	X	X

Source: Modified from Gilligan et al (2013).

[53]The same module at endline collects data for children 3 to 7.
[54]The same module at endline collects data for children 1 to 7.

Table A.3: List of indicators by age group

Children 6 to 12/13 at baseline	
School outcomes	
Currently enrolled	
N. days attended school out of n days school was open in the last 4 weeks	
Amount paid for schooling this year (logged)	
Activities and chores outcomes [last 7 days]	
Looking after younger children and babies:	Participation (1/0)
	Average hours per day (logged)
Looking after or caring for sick household members:	Participation (1/0)
	Average hours per day (logged)
Looking after livestock:	Participation (1/0)
	Average hours per day (logged)
Helping with other agricultural work on own land:	Participation (1/0)
	Average hours per day (logged)
Doing other chores outside or far from the home:	Participation (1/0)
	Average hours per day (logged)
Doing other chores near or in the home:	Participation (1/0)
	Average hours per day (logged)
Doing wage work:	Participation (1/0)
	Average hours per day (logged)
Any work (productive or reproductive):	Average hours per day (logged)
Productive work:	Participation (1/0)
	Average hours per day (logged)
Reproductive work:	Participation (1/0)
	Average hours per day (logged)
Adults 18 to 59 at baseline	
Time use outcomes	
In the last six months…	
Worked in agriculture on land operated by household (1/0)	
Spent time looking after livestock for the household (1/0)	
Did work that paid a salary or wages (1/0)	
Did work as non-agricultural self-employment (1/0)	
In the last two weeks…	
Domestic work: Average hours per day (logged)	
Income-earning activities: Average hours per day (logged)#	
Leisure activities: Average hours per day (logged)#	
Children 3–5 at baseline	
Preschool outcomes	
Currently enrolled in an ECD centre	
Attended any time during this school year	
Activities and chores outcomes [last 7 days]	
Looking after younger children and babies:	Participation (1/0)
	Average hours per day (logged)
Looking after livestock:	Participation (1/0)
	Average hours per day (logged)
Doing other chores outside or far from the home:	Participation (1/0)
	Average hours per day (logged)
Doing other chores near or in the home:	Participation (1/0)
	Average hours per day (logged)
Any work (productive or reproductive):	Average hours per day (logged)
Productive work:	Participation (1/0)
	Average hours per day (logged)
Reproductive work:	Participation (1/0)
	Average hours per day (logged)

Indicators collected only at endline.

Table A.4: Main reason reported for not being enrolled in school for children aged 7–12 at baseline

	%
Work:	
Domestic work	51.0
Looking after livestock	7.0
Agricultural work	3.8
Lack of funds	11.6
Lack of motivation / don't want to go	9.8
Child not ready, not mature enough	6.1
Parent sees no value in education / no motivation to send	2.2
School is too far	2.1
Other	6.4
N	1,203

Note: The 'other' option also includes responses below 1 per cent and not reported (1.4%).

Table A.5: Impact estimates on schooling outcomes for children aged 6−17 at endline

	Currently enrolled (1)	N. days attended school out of n days school open (2)	Amount paid for schooling this year, logged (3)
Food impact	-0.00	-0.01	-0.26
	(0.03)	(0.03)	(0.28)
Cash impact	-0.02	-0.00	0.07
	(0.03)	(0.03)	(0.26)
R-squared	0.16	0.16	0.17
N	5,297	5,265	5,286
Endline control mean	0.471	0.451	3.660

Notes: Estimations use single-difference modelling among children aged 6-17 at endline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender.

Table A.6: Impact estimates on labour outcomes for children aged 7−14 at endline

	Any work		Productive work		Reproductive work	
	Part. (1/0) (1)	Hours (logged) (2)	Part. (1/0) (3)	Hours (logged) (4)	Part. (1/0) (5)	Hours (logged) (6)
Food impact	-0.01	-0.00	-0.02	-0.05	-0.01	0.01
	(0.02)	(0.05)	(0.04)	(0.05)	(0.02)	(0.04)
Cash impact	0.02	*0.12***	**0.22****	**0.24****	0.01	0.03
	(0.01)	(0.05)	(0.04)	(0.05)	(0.02)	(0.04)
R-squared	0.04	0.15	0.15	0.17	0.03	0.11
N	3,758	3,710	3,731	3,716	3,758	3,749
Endline control mean	0.906	1.711	0.379	0.480	0.889	1.561
Unadj. p-value: Food impact=0	0.75	0.99	0.67	0.37	0.53	0.76
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.11	0.01	0.00	0.00	0.45	0.44
Adj. p-value: Cash impact=0	0.89	0.23	0.00	0.00	0.99	0.99

Notes: Estimations use single-difference modelling among children aged 7−14 at endline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender. Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level even after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Productive work includes: looking after livestock, helping with other agricultural work on own land and doing wage work. Reproductive work includes: doing other chores outside or far from the home (such as gathering greens, collecting firewood, or carrying water), doing other chores near or in the home (such as cooking, cleaning, washing clothes, or doing other domestic work), looking after younger children and babies and looking after or caring for sick household members. 'Any work' combines productive and reproductive work. The reference period for all activities is the last seven days. Hours are meant per day. Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.7: Impact estimates on labour outcomes for children aged 7−14 at endline, disaggregated activities

	PRODUCTIVE WORK *Last seven days*						REPRODUCTIVE WORK *Last seven days*							
	Looking after livestock		Helping with other agricultural work on own land		Doing wage work		Doing other chores outside or far from the home		Doing other chores near or in the home		Looking after younger children and babies		Looking after or caring for sick household members	
	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Food impact	-0.02	-0.03	-0.01	-0.02	0.00	-0.01	0.03	*0.05**	-0.02	-0.00	-0.00	0.02	-0.02	-0.03
	(0.02)	(0.03)	(0.05)	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)
Cash impact	0.01	0.00	**0.24*** **	**0.25*** **	0.01	0.00	*0.07****	*0.06***	0.02	0.02	0.03	0.03	-0.03	-0.04
	(0.02)	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
R-squared	3,750	3,750	3,755	3,755	3,721	3,721	3,756	3,756	3,757	3,757	3,751	3,751	3,756	3,756
N	0.071	0.078	0.155	0.170	0.038	0.033	0.050	0.091	0.059	0.103	0.063	0.079	0.078	0.078
Endline control mean	0.076	0.124	0.323	0.356	0.044	0.054	0.724	0.746	0.745	0.786	0.672	0.801	0.196	0.197
Unadj. p-value: Food impact=0	0.21	0.20	0.78	0.67	0.86	0.73	0.28	0.08	0.58	0.94	0.98	0.63	0.32	0.17
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.79	0.99	0.99	0.99	0.99	0.99	0.97
Unadj. p-value: Cash impact=0	0.65	0.90	0.00	0.00	0.55	0.88	0.01	0.02	0.56	0.48	0.33	0.55	0.21	0.11
Adj. p-value: Cash impact=0	0.99	0.99	0.00	0.00	0.99	0.99	0.11	0.28	0.99	0.99	0.99	0.99	0.99	0.89

Notes: Estimations use single-difference modelling among children aged 7−14 at endline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for stratified design and clustering at the ECD level. All estimations control for stratum dummies, child's age and gender. Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level even after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
The reference period for all activities is the last seven days, hours are meant per day.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.8: Impact of cash transfers on schooling outcomes for primary school−age children (aged 7–12 at baseline), cluster (ECD) level fixed effects

	Currently enrolled (1)	N. days attended school out of n. days school was open (2)	Amount paid for schooling this year, logged (3)
Food*Follow-up	0.01	-0.01	-0.29
	(0.04)	(0.04)	(0.33)
Cash*Follow-up	0.02	0.02	0.32
	(0.04)	(0.04)	(0.34)
Follow-up	0.08***	0.10***	0.29
	(0.03)	(0.03)	(0.25)
R-squared	0.01	0.01	0.00
N	5,808	5,758	5,776
Baseline control mean	0.517	0.478	4.381
P-value: Food=Cash	0.68	0.37	0.06
Unadj. p-value: Food impact=0	0.88	0.72	0.39
Adj. p-value: Food impact=0	0.99	0.98	0.77
Unadj. p-value: Cash impact=0	0.59	0.59	0.35
Adj. p-value: Cash impact=0	0.93	0.93	0.73

Notes: Estimations use ECD level fixed effects (98 clusters) among children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Unadj.=Unadjusted; Adj.=Adjusted

Table A.9: Impact of cash transfers on labour outcomes for children aged 7–12 at baseline, cluster (ECD) level fixed effects

	Any work		Productive work		Reproductive work	
	Part. (1/0) (1)	Hours (logged) (2)	Part. (1/0) (3)	Hours (logged) (4)	Part. (1/0) (5)	Hours (logged) (6)
Food*Follow-up	0.01	0.04	0.04	-0.01	0.01	0.06
	(0.03)	(0.11)	(0.09)	(0.11)	(0.04)	(0.09)
Cash*Follow-up	0.05*	0.27***	**0.31*****	**0.37*****	0.06*	0.18**
	(0.03)	(0.10)	(0.08)	(0.10)	(0.04)	(0.08)
Follow-up	-0.02	-0.10	-0.20***	-0.17**	-0.01	-0.05
	(0.02)	(0.08)	(0.06)	(0.08)	(0.02)	(0.06)
R-squared	0.00	0.01	0.03	0.02	0.00	0.00
N	5,594	5,503	5,558	5,510	5,595	5,576
Baseline control mean	0.938	1.880	0.616	0.719	0.904	1.648
P-value: Food=Cash	0.14	0.01	0.00	0.00	0.21	0.11
Unadj. p-value: Food impact=0	0.77	0.73	0.68	0.91	0.70	0.53
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.08	0.01	0.00	0.00	0.10	0.02
Adj. p-value: Cash impact=0	0.80	0.11	0.01	0.01	0.86	0.33

Notes: Estimations use ECD level fixed effects (98 clusters) among children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Productive work includes: looking after livestock, helping with other agricultural work on own land and doing wage work. Reproductive work includes: doing other chores outside or far from the home (such as gathering greens, collecting firewood, or carrying water), doing other chores near or in the home (such as cooking, cleaning, washing clothes, or doing other domestic work), looking after younger children and babies and looking after or caring for sick household members. 'Any work' combines productive and reproductive work. The reference period for all activities is the last seven days. Hours are meant per day.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.10: Impact of cash transfers on labour outcomes for children aged 7–12 at baseline, cluster (ECD) level fixed effects

	PRODUCTIVE WORK *Last seven days*						REPRODUCTIVE WORK *Last seven days*							
	Looking after livestock		Helping with other agricultural work on own land		Doing wage work		Doing other chores outside or far from the home		Doing other chores near or in the home		Looking after younger children and babies		Looking after or caring for sick household members	
	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Food*Follow-up	0.01	-0.01	0.05	0.02	-0.03	-0.05	0.03	0.05	0.00	-0.02	0.05	0.04	0.05	0.03
	(0.02)	(0.04)	(0.09)	(0.10)	(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.07)	(0.04)	(0.04)
Cash*Follow-up	0.03	0.01	**0.34*** **	**0.39*** **	0.01	-0.01	0.11**	0.15***	0.04	0.05	0.10*	0.12*	0.01	0.00
	(0.03)	(0.04)	(0.08)	(0.09)	(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)
Follow-up	-0.02	0.01	-0.22***	-0.21***	0.02	0.03	0.01	0.01	0.03	0.06	-0.06	-0.10*	-0.09***	-0.07***
	(0.02)	(0.03)	(0.06)	(0.07)	(0.02)	(0.02)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.03)	(0.03)
R-squared	0.00	0.00	0.03	0.03	0.00	0.00	0.01	0.01	0.00	0.01	0.00	0.00	0.01	0.01
N	5,575	5,575	5,586	5,586	5,524	5,524	5,592	5,592	5,590	5,590	5,589	5,589	5,585	5,585
Baseline control mean	0.101	0.139	0.575	0.607	0.042	0.043	0.733	0.759	0.743	0.761	0.744	0.906	0.316	0.301
P-value: Food=Cash	0.35	0.39	0.00	0.00	0.18	0.17	0.10	0.07	0.39	0.24	0.32	0.18	0.29	0.55
Unadj. p-value: Food impact=0	0.74	0.71	0.61	0.82	0.28	0.15	0.62	0.38	0.98	0.78	0.39	0.56	0.20	0.47
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.95	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.30	0.73	0.00	0.00	0.80	0.84	0.04	0.01	0.36	0.39	0.07	0.06	0.90	0.92
Adj. p-value: Cash impact=0	0.99	0.99	0.00	0.00	0.99	0.99	0.52	0.16	0.99	0.99	0.76	0.69	0.99	0.99

Notes: Estimations use ECD level fixed effects (98 clusters) among children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
The reference period for all activities is the last seven days. Hours are meant per day.
Doing other chores outside or far from the home includes activities such as gathering greens, collecting firewood, or carrying water; doing other chores near or in the home includes activities such as cooking, cleaning, washing clothes, or doing other domestic work.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.11: Impact of cash transfers on time use outcomes for adults aged 18–59 at baseline, cluster (ECD) level fixed effects

	In last six months…				*In last two weeks…*
	Worked in agriculture	Spent time looking after livestock	Did work that paid a salary or wages	Non-agric. self-employed work	Domestic work
	Part. (1/0)	Part. (1/0)	Part. (1/0)	Part. (1/0)	Hours per day (logged)
	(1)	(2)	(3)	(4)	(5)
Food*Follow-up	0.01	*0.07**	0.03	-0.00	0.02
	(0.03)	(0.04)	(0.06)	(0.04)	(0.05)
Cash*Follow-up	**0.10*** **	-0.01	-0.03	-0.02	0.03
	(0.03)	(0.04)	(0.07)	(0.04)	(0.05)
Follow-up	-0.14***	-0.07**	-0.09*	-0.03	-0.25***
	(0.02)	(0.03)	(0.05)	(0.03)	(0.04)
R-squared	0.03	0.01	0.01	0.00	0.02
N	9,660	9,644	9,650	9,654	9,674
Baseline control mean	0.930	0.159	0.380	0.272	1.365
P-value: Food=Cash	0.00	0.02	0.24	0.59	0.81
Unadj. p-value: Food impact=0	0.82	0.07	0.65	0.97	0.67
Adj. p-value: Food impact=0	0.99	0.57	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.00	0.78	0.61	0.60	0.55
Adj. p-value: Cash impact=0	0.02	0.99	0.99	0.99	0.99

Notes: Estimations use ECD level fixed effects (98 clusters) among adults aged 18–59 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Time spent on income-earning activities and leisure are not reported as data was collected only at endline.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.12: Impact of cash transfers on schooling outcomes for primary school−age children (aged 7–12 at baseline), individual level fixed effects

	Currently enrolled	N. days attended school out of n. days school was open	Amount paid for schooling this year, logged
	(1)	(2)	(3)
Food*Follow-up	0.00	-0.02	-0.32
	(0.04)	(0.04)	(0.37)
Cash*Follow-up	0.02	0.02	0.23
	(0.04)	(0.04)	(0.34)
Follow-up	0.07**	0.09***	0.25
	(0.03)	(0.03)	(0.25)
R-squared	0.02	0.03	0.00
N	4,956	4,864	4,896
N. of children	2,478	2,432	2,448
Baseline control mean	0.538	0.495	4.510
P-value: Food=Cash	0.65	0.37	0.12
Unadj. p-value: Food impact=0	0.99	0.71	0.38
Adj. p-value: Food impact=0	0.99	0.98	0.76
Unadj. p-value: Cash impact=0	0.63	0.58	0.49
Adj. p-value: Cash impact=0	0.95	0.92	0.86

Notes: Estimations use individual-level fixed effects among panel children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
Unadj.=Unadjusted; Adj.=Adjusted

Table A.13: Impact of cash transfers on labour outcomes for children aged 7–12 at baseline, individual level fixed effects

	Any work		Productive work		Reproductive work	
	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)
	(1)	(2)	(3)	(4)	(5)	(6)
Food*Follow-up	0.00	0.04	0.04	0.01	0.00	0.05
	(0.03)	(0.12)	(0.09)	(0.11)	(0.04)	(0.10)
Cash*Follow-up	0.05	0.28***	**0.31*** **	**0.38*** **	0.07*	0.20**
	(0.03)	(0.10)	(0.08)	(0.11)	(0.04)	(0.08)
Follow-up	-0.01	-0.08	-0.19***	-0.17**	-0.00	-0.04
	(0.03)	(0.08)	(0.06)	(0.08)	(0.03)	(0.07)
R-squared	0.01	0.01	0.05	0.04	0.01	0.01
N	4,546	4,414	4,488	4,426	4,546	4,516
N. of children	2,273	2,207	2,244	2,213	2,273	2,258
Baseline control mean	0.929	1.858	0.613	0.712	0.896	1.635
P-value: Food=Cash	0.12	0.02	0.00	0.00	0.12	0.09
Unadj. p-value: Food impact=0	0.95	0.73	0.64	0.95	0.92	0.65
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.11	0.01	0.00	0.00	0.09	0.02
Adj. p-value: Cash impact=0	0.90	0.14	0.01	0.01	0.83	0.29

Notes: Estimations use individual-level fixed effects among panel children aged 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics. Productive work includes: looking after livestock, helping with other agricultural work on own land and doing wage work. Reproductive work includes: doing other chores outside or far from the home (such as gathering greens, collecting firewood, or carrying water), doing other chores near or in the home (such as cooking, cleaning, washing clothes, or doing other domestic work), looking after younger children and babies and looking after or caring for sick household members. 'Any work' combines productive and reproductive work. The reference period for all activities is the last seven days. Hours are meant per day.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.14: Impact of cash transfers on labour outcomes for children aged 7–12 at baseline, individual-level fixed effects

	PRODUCTIVE WORK *Last seven days*						REPRODUCTIVE WORK *Last seven days*							
	Looking after livestock		Helping with other agricultural work on own land		Doing wage work		Doing other chores outside or far from the home		Doing other chores near or in the home		Looking after younger children and babies		Looking after or caring for sick household members	
	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)	Part. (1/0)	Hours (logged)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Food*Follow-up	0.03	0.00	0.05	0.03	-0.02	-0.03	0.02	0.05	0.00	-0.03	0.04	0.04	0.06	0.04
	(0.02)	(0.04)	(0.09)	(0.10)	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.08)	(0.05)	(0.05)
Cash*Follow-up	0.04	0.01	**0.35*** **	**0.41*** **	0.01	0.00	0.12**	0.16***	0.04	0.04	0.11*	0.15**	0.01	0.01
	(0.03)	(0.04)	(0.08)	(0.09)	(0.03)	(0.03)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)
Follow-up	-0.02	0.01	-0.22***	-0.21***	0.01	0.02	0.02	0.02	0.04	0.08*	-0.07	-0.11**	-0.09**	-0.07**
	(0.02)	(0.03)	(0.07)	(0.07)	(0.02)	(0.02)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)
R-squared	0.00	0.00	0.06	0.06	0.00	0.00	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.01
N	4,512	4,512	4,534	4,534	4,448	4,448	4,544	4,544	4,540	4,540	4,538	4,538	4,530	4,530
N. of children	0.0257	0.00210	2,267	2,267	2,224	2,224	2,272	2,272	2,270	2,270	2,269	2,269	2,265	2,265
Baseline control mean	0.104	0.137	0.575	0.607	0.044	0.045	0.727	0.752	0.734	0.747	0.742	0.915	0.310	0.295
P-value: Food=Cash	0.59	0.77	0.00	0.00	0.25	0.24	0.07	0.06	0.43	0.27	0.21	0.12	0.27	0.53
Unadj. p-value: Food impact=0	0.25	0.95	0.58	0.76	0.51	0.30	0.74	0.40	0.99	0.69	0.52	0.60	0.21	0.41
Adj. p-value: Food impact=0	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.19	0.78	0.00	0.00	0.61	0.94	0.04	0.01	0.39	0.48	0.06	0.03	0.89	0.85
Adj. p-value: Cash impact=0	0.98	0.99	0.00	0.00	0.99	0.99	0.50	0.13	0.99	0.99	0.68	0.43	0.99	0.99

Notes: Estimations use individual-level fixed effects among panel children age 7–12 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.
The reference period for all activities is the last seven days. Hours are meant per day. Doing other chores outside or far from the home includes activities such as gathering greens, collecting firewood, or carrying water; doing other chores near or in the home includes activities such as cooking, cleaning, washing clothes, or doing other domestic work.
Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.15: Impact of cash transfers on time use outcomes for adults aged 18–59 at baseline, individual-level fixed effects

	In last six months…				*In last two weeks…*
	Worked in agriculture	Spent time looking after livestock	Did work that paid a salary or wages	Non-agric. self-employed work	Domestic work
	Part. (1/0)	Part. (1/0)	Part. (1/0)	Part. (1/0)	Hours per day (logged)
	(1)	(2)	(3)	(4)	(5)
Food*Follow-up	-0.00	*0.07**	0.02	-0.01	0.03
	(0.03)	(0.04)	(0.06)	(0.04)	(0.04)
Cash*Follow-up	**0.10*****	0.00	-0.04	-0.02	0.03
	(0.03)	(0.04)	(0.07)	(0.04)	(0.05)
Follow-up	-0.14***	-0.08**	-0.09*	-0.04	-0.25***
	(0.02)	(0.03)	(0.05)	(0.03)	(0.03)
R-squared	0.07	0.02	0.02	0.01	0.08
N	8,802	8,774	8,782	8,790	8,804
N of adults	4,401	4,387	4,391	4,395	4,402
Baseline control mean	0.929	0.161	0.380	0.278	1.363
P-value: Food=Cash	0.00	0.03	0.29	0.70	0.99
Unadj. p-value: Food impact=0	0.99	0.06	0.77	0.89	0.50
Adj. p-value: Food impact=0	0.99	0.49	0.99	0.99	0.99
Unadj. p-value: Cash impact=0	0.00	0.99	0.55	0.64	0.55
Adj. p-value: Cash impact=0	0.02	0.99	0.99	0.99	0.99

Notes: Estimations use individual-level fixed effects among panel adults aged 18–59 at baseline. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses corrected for clustering at the ECD level. Specifications are estimated on all children (whether enrolled or not). Adjusted p-values are Sidak-Bonferroni corrected p-values. Impact estimates that remain significant at the 5 per cent level after adjusting for multiple inference are reported in bold; those that do not survive the multiple hypothesis testing are reported in italics.

Time spent on income-earning activities and leisure are not reported as data was collected only at endline.

Part.=Participation; Unadj.=Unadjusted; Adj.=Adjusted

Table A.16: Lee bounds, schooling outcomes for primary school-age children (aged 7–12 at baseline)

	Currently enrolled			N. days attended school out of n. days school was open			Amount paid for schooling this year, logged		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	-0.02	-0.02	-0.02	-0.01	-0.00	-0.01	-0.08	-0.06	-0.11
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.33)	(0.33)	(0.33)
N	2,478	2,473	2,473	2,432	2,422	2,422	2,448	2,443	2,443
R-squared	0.22	0.22	0.22	0.20	0.20	0.20	0.22	0.22	0.21

Notes: Standard errors in parentheses clustered at the ECD level. * p<0.1, ** p<0.05, *** p<0.01. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable; the food dummy is included but not shown.

Table A.17: Lee bounds, labour outcomes for primary school-age children (aged 7–12 at baseline)

	Any work, participation (1/0)			Any work, hours (logged)			Productive work, participation (1/0)		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.03*	0.05***	0.03*	0.17***	0.21***	0.14**	0.27***	0.28***	0.26***
	(0.02)	(0.01)	(0.02)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
N	2,273	2,257	2,257	2,207	2,190	2,190	2,244	2,230	2,230
R-squared	0.04	0.05	0.04	0.13	0.13	0.12	0.14	0.15	0.14
	Productive work, hours (logged)			Reproductive work, participation (1/0)			Reproductive work, hours (logged)		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.31***	0.32***	0.28***	0.02	0.04**	0.02	0.07	0.11**	0.04
	(0.07)	(0.07)	(0.06)	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.05)
N	2,213	2,200	2,200	2,273	2,257	2,257	2,258	2,242	2,242
R-squared	0.14	0.15	0.13	0.05	0.05	0.05	0.12	0.12	0.11

Notes: Standard errors in parenthesis clustered at the ECD level. * p<0.1, ** p<0.05, *** p<0.01. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable; the food dummy is included but not shown.

Table A.18: Lee bounds, labour outcomes for primary school-age children (aged 7–12 at baseline) – disaggregated activities

	Looking after livestock, participation (1/0)			Looking after livestock, hours (logged)			Helping with other agricultural work on own land, participation (1/0)		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.02	0.03	0.01	0.02	0.02	-0.01	0.29***	0.30***	0.28***
	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.05)	(0.05)	(0.05)
N	2,256	2,244	2,244	2,256	2,244	2,244	2,267	2,254	2,254
R-squared	0.10	0.10	0.09	0.11	0.11	0.09	0.15	0.15	0.14
	Helping with other agricultural work, hours (logged)			Doing wage work, participation (1/0)			Doing wage work, hours (logged)		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.31***	0.33***	0.29***	0.01	0.01	-0.01	-0.01	-0.00	-0.03
	(0.06)	(0.06)	(0.06)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	2,267	2,254	2,255	2,224	2,210	2,210	2,224	2,210	2,210
R-squared	0.15	0.15	0.14	0.03	0.03	0.03	0.03	0.03	0.03
	Doing other chores outside or far from home, participation (1/0)			Doing other chores outside or far from the home, hours (logged)			Doing other chores near or in the home, participation (1/0)		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.08***	0.10***	0.08***	0.10***	0.12***	0.07**	0.03	0.05	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
N	2,272	2,256	2,256	2,272	2,256	2,256	2,270	2,253	2,253
R-squared	0.05	0.05	0.05	0.09	0.09	0.09	0.07	0.07	0.07
	Doing other chores near or in the home, hours (logged)			Looking after younger children and babies, participation (1/0)			Looking after younger children, hours (logged)		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.05	0.06*	0.02	0.05	0.06	0.04	0.04	0.06	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
N	2,270	2,253	2,253	2,269	2,251	2,251	2,269	2,251	2,251
R-squared	0.11	0.11	0.10	0.08	0.08	0.08	0.10	0.10	0.09
	Looking after or caring for sick household members, participation (1/0)			Time spent looking after sick household members, hours (logged)					
	Beta	Upper	Lower	Beta	Upper	Lower			
Cash impact	-0.06*	-0.05	-0.07**	-0.06*	-0.06*	-0.09***			
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)			
N	2,265	2,252	2,252	2,265	2,252	2,252			
R-squared	0.08	0.08	0.08	0.08	0.08	0.08			

Notes: Standard errors in parentheses clustered at the ECD level. * p<0.1, ** p<0.05, *** p<0.01. All estimations control for stratum dummies, child's age and gender as well as the baseline value of the dependent variable; the food dummy is included but not shown.

Table A.19: Lee bounds, time use outcomes for adults (aged 18–59 at baseline)

	Worked in agriculture, participation (1/0)			Spent time looking after livestock, participation (1/0)			Did work that paid a salary or wages, participation (1/0)		
	Beta	Upper	Lower	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.09***	0.12***	0.09***	-0.01	-0.01	-0.04***	0.00	0.01	-0.02
	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.03)
N	4,401	4,346	4,347	4,387	4,340	4,341	4,391	4,341	4,342
R-squared	0.06	0.07	0.06	0.08	0.08	0.08	0.03	0.03	0.03

	Did work at non-ag. self-employment, participation (1/0)			Domestic work, hours (logged)		
	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	-0.00	0.00	-0.03	0.03	0.05*	0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
N	4,395	4,346	4,347	4,402	4,355	4,355
R-squared	0.07	0.07	0.06	0.38	0.38	0.38

Notes: Standard errors in parenthesis clustered at the ECD level. * p<0.1, ** p<0.05, *** p<0.01. All estimations control for stratum dummies, age and gender as well as the baseline value of the dependent variable; the food dummy is included but not shown.

Table A.20: Lee bounds, time use outcomes for adults (aged 18–59 at baseline)

	Income–earning activities,† hours (logged)			Leisure activities,† hours (logged)		
	Beta	Upper	Lower	Beta	Upper	Lower
Cash impact	0.25***	0.30***	0.23***	-0.02	0.01	-0.05
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
N	4,399	4,353	4,353	4,371	4,330	4,330
R-squared	0.11	0.12	0.11	0.15	0.14	0.15

Notes: Standard errors in parenthesis clustered at the ECD level. * p<0.1, ** p<0.05, *** p<0.01. All estimations control for stratum dummies, age and gender as well as the baseline value of the dependent variable; the food dummy is included but not shown.

3 The Impact of the Child Grant Programme on Women's Savings: Experimental Evidence from Rural Zambia[55]

3.1 Introduction

In low-income countries where access to financial services remains limited, savings are an important mechanism for smoothing consumption and financing productive investment. On one hand, savings fulfil precautionary motives in situations of income uncertainty: poor households can use savings to provide a buffer during hard times, smooth income fluctuations and avoid negative coping mechanisms to shocks. On the other hand, savings can also provide capital to finance investment opportunities not only in human capital but also in the diversification of income sources and asset stocks, thus mitigating further against income fluctuation.

In sub-Saharan Africa (SSA), poor households – with low and unreliable incomes – are often constrained in their ability to save as they face several challenges. They lack a safe place to keep their savings and earn a return (Hulme and Arun, 2009); indeed, poor households rarely have access to adequate financial services. Moreover, it can be difficult to protect savings from claims made by relatives, neighbours, alcoholic husbands and so on (Hulme and Arun, 2009; Platteau, 2000; Ashraf, 2009). Last but not least, there might be problems resisting the personal temptation to spend the money at hand (Hulme and Arun, 2009; Ashraf et al., 2006 in the Philippinnes; Duflo

[55] This chapter is a substantially developed version of the following joint work: Natali, L., S. Handa, A. Peterman, D. Seidenfeld, G. Tembo, Zambia Cash Transfer Evaluation Team (2016). Making Money Work: Unconditional cash transfers allow women to save and re-invest in rural Zambia, *Innocenti Working Paper* No.2016-02, UNICEF Office of Research, Florence.

et al., 2006 in Kenya; Laibson, 1997; Gul and Pesendorfer, 2001; Gul and Pesendorfer, 2004). Notwithstanding the existing barriers, empirical evidence has shown that even the poorest people and individuals in SSA do save (Chowa et al., 2012; Collins et al., 2009; Dupas and Robinson, 2009; Karlan et al., 2014).

There is little evidence of successful programming efforts to facilitate cash savings, including those which specifically target women, and are cost effective and scaleable. Unlike other forms of saving, cash savings are liquid; they improve households' ability to manage unexpected adverse events, such as health shocks, death of a family member, crop failure etc. (Collins et al., 2009), re-invest in productive or human capital investment (Rutherford, 1999) and finally, to fund recurring/day-to-day expenses and satisfy households' cash requirements, such as school fees or the purchase of consumption goods (i.e., to engage in the cash economy).

Enabling women's, rather than more broadly household, cash savings might also be good for women's empowerment. Existing literature, particularly from conditional cash transfers in Latin America, has shown that the transfer might increase women's control over resources within the household (Fiszbein and Schady, 2009; van den Bold et al., 2013, among others) that may, in turn, impact spending patterns (in particular, it transforms into greater investment in children's health, nutrition and education as shown in Haddad et al., 1994; Hoddinott and Haddad, 1995; Schady and Rosero, 2007, among others).

However, less evidence exists overall on the effects of cash-based interventions – conditional or unconditional – on women's savings in Africa as in other regions. This paper investigates the impact of a cash transfer intervention, in the form of the Government of Zambia's Child Grant Programme (CGP), on women's cash savings behaviour primarily. The CGP is an unconditional cash transfer (UCT) targeted to all households with a child under age five in three of the poorest districts of rural Zambia that provided a flat transfer of approximately USD 12 (USD 24 PPP) per month (paid bi-monthly) irrespective of household size. The overall stated goal of the CGP is to reduce extreme poverty and break the inter-generational cycle of poverty. Specific objectives of the programme mainly focused on food security, young child nutrition and

health.

While the beneficiary in the CGP is the household, the transfer is paid directly to the child's primary caregiver, 99 per cent of whom are women. This provides potential to investigate the relatively understudied question of whether UCTs can impact female-level outcomes: in particular, does the CGP incentivize women's cash savings? The potential impact of a cash transfer on women's cash savings in this context is particularly relevant for two main reasons.

First, women have little access to cash in these remote rural areas of Zambia. At baseline, a third of women were involved in farming – mainly subsistence agriculture; an additional 10 per cent did not carry out economic activities (homemakers). Another 30 per cent of women were engaged in piece-work which represents short-term casual work and by definition would only provide access to small and irregular amounts of cash. Less than a fifth of women ran a small non-farm business and therefore had somewhat more regular access to cash. The liquidity constraint is also highlighted by the low initial cash saving rates among women (16−18 per cent).

Second, women in this context also had relatively low control over resources to start with. Qualitative research from the same programme (Bonilla et al., 2017) also suggests that women relate empowerment within their own communities to the availability of financial resources to spend how they wish. However, over forty per cent of women reported no cash income at all at baseline and many of those who did earn some money reported only very small amounts.

Given the paucity of formal financial institutions in these remote districts of rural Zambia and the many barriers to accessing savings accounts in financial facilities typical in developing countries more generally, low-income women are more likely to save informally. The dominant form of saving in the sample is indeed informal with women either keeping their money at home (93 percent) or in other informal saving schemes (roughly 2 percent). The proportion of women holding some formal savings (at a bank or post office) is almost null (below 5 percent). Informal savings are typically less lucrative, with women saving in a low-yielding form, but are still desirable and can be potentially transformative when providing women with the instruments to invest.

This Chapter goes beyond the effects on basic short-term subsistence requirements and studies a medium-term impact of the programme. Savings could be thought of as an additional effect of the programme facilitated by its unconditional nature; although UCTs are not traditionally thought of as savings instruments, since households receiving UCTs are free to use money according to their needs and do not have to comply with any condition to receive the transfer, broad impacts across multiple domains are possible.[56]

Ideally, we would like to explore other forms of women's savings; indeed, the savings of poor households are often in non-currency form: buffer assets, such as the acquisition of livestock, productive assets or land, as well as crop stocks; moreover, even though cash savings are good for liquidity, they might also be unstable, so 'good' investors would tend to diversify (savings diversification). Unfortunately, due to a data constraint, information on other forms of savings is available only at the household level.[57]

Apart from official impact evaluation reports (AIR, 2011; 2013; 2014; Daidone et al., 2013), a number of studies have already been published or are forthcoming on the impact of the CGP on a broad range of outcomes (Handa et al., 2016a; Handa et al., 2018), consumption and food security as well as child growth and malnutrition (Seidenfeld et al., 2014a), schooling and child labour (Handa et al., 2016b), maternal health outcomes (Handa et al., 2016c), the local economy (Thome et al., 2014), the ability of households to avoid adverse coping strategies when facing shocks (Lawlor et al., 2015; Asfaw et al., 2017), women's decision-making and empowerment (Bonilla et al., 2017), early child development (Seidenfeld et al., 2014b), consumption smoothing and productive investments (Bonilla et al., 2015), and fertility (Palermo et al., 2016).

We use data from a large-scale social experiment run to evaluate the impact of the CGP and involving 2,519 households over 90 clusters, that were randomized to an early treatment or a

[56] As highlighted in Davis et al. (2016:6), "Recipients in conditional programmes also have some flexibility in how they spend money, but there are clear incentives to spend on health and education and basic foods since receipt of the transfer is conditional on health and education behaviour. As a result, impact evaluations of CCTs have tended to focus on outcomes in these narrow areas."

[57] Information on cash savings, on the other hand, is available only for women.

delayed-entry control group in three of the poorest rural districts in Zambia. We find that the savings behaviour of women in the intervention arm has changed: the CGP has enabled poor women with limited access to cash to save by significantly increasing the proportion of women with any cash savings, as well as the amount saved in the previous month. The effects are large. For savings rates, the average treatment effect is 15 percentage points after 48 months, which represents almost a 100 per cent increase over baseline; this impact is accompanied by an 80 per cent increase on the amount saved overall. This result is particularly relevant as cash availability for poor women could proxy for women's financial empowerment, representing an improvement in their financial standing within the household. Robustness checks indicate that cash savings are not crowding out traditional forms of savings (e.g. livestock) and are not simply a function of cash on hand from recent payment dates. We show statistics to support that that what is designated as savings is indeed savings as conventionally understood. We provide suggestive evidence of important feedback effects between women's cash savings and increased intra-household resources under women's decision-making control, as well as investment in productive activities through household operation of small non-farm enterprises (NFEs). The types of business activity and the patterns of time-use among men and women in programme households indicate that these small businesses are primarily operated by women. We further explore sources of non-financial constraints and find that social network pressures do not appear to inhibit savings within our sample.

This study highlights four main contributions. First and foremost, the research contributes to the relatively scarce literature on the impacts of cash transfers on savings. Second, we observe the impact on increases in women's cash availability, which proxies for women's financial empowerment. We therefore also contribute to literature on cash transfers and women's economic empowerment. Third, this Chapter adds to the literature from Africa, where dynamics around poverty, women's empowerment and access to services (including financial services), are likely to be distinct from Latin America, which has provided most of the current evidence (Adato et al., 2000; de Braw et al., 2014; Gitter and Barham, 2008). Fourth, we observe outcomes at 24, 36 and 48 months, which allows us to look past the short-term and assess impacts over a sufficient period

for behavioural and economic changes related to our outcomes of interest to take place, and also to provide some partial indication regarding sustainability. In brief, this is the first investigation into the impacts of a UCT in Africa on women's cash savings behaviour over the medium-term.[58] Moreover, the CGP is a large-scale government run programme,[59] thus enhancing considerably the external validity of the results we present.

The Chapter is structured as follows: Section 3.2 presents a conceptual framework and explores the several channels through which the CGP could impact our primary outcome of interest; Section 3.3 briefly reviews the existing empirical evidence on the impact of cash transfers on savings. Section 3.4 describes the CGP and the evaluation design. Section 3.5 introduces the data, sample and analysis methodology. Section 3.6 presents results including robustness checks. Section 3.7 explores linkages between women's cash savings, investments and empowerment while Section 3.8 discusses non-financial saving constraints. Finally, Section 3.9 concludes.

3.2 Conceptual framework: cash transfers and savings

In this section, we focus on and explore the several channels or pathways through which the CGP could affect women's savings behaviour. As already highlighted, the CGP was not designed to specifically impact savings, and the majority of primary objectives were focused on short-term poverty alleviation. However, the CGP is a UCT, thus households had full autonomy over how cash was spent or utilized. The baseline evaluation report (AIR, 2011) provides a conceptual framework for the CGP (replicated as Figure B.1 in Appendix B). Given the programme is implemented in a poor environment[60] where incomes are very low, we might expect that households will spend almost all the additional income provided by the cash transfer on

[58] Impact evaluations over this relatively long-term time span are fairly rare. It should however be highlighted that this research captures medium-term impacts while the programme is still on-going, rather than post-intervention impacts.
[59] The CGP therefore differs also in terms of potential scalability and generalizability relative to smaller pilot NGO programmes such as GiveDirectly. There are also additional contrasting programme design differences as related to the evidence produced in the GiveDirectly trial. For example, the GiveDirectly transfer is paid directly to the recipient's mobile phone which could be considered to act as some form of financial service therefore facilitating savings. Haushofer and Shapiro (2016) find that beneficiary households save an additional USD 3 PPP in M-Pesa relative to control households.
[60] Poverty rates at baseline are exceptionally high in the CGP sample with 95 per cent of individuals living in extreme poverty compared to 69 per cent in rural Zambia and 85 per cent in the three districts (AIR 2011; pp. 25).

immediate needs including food, shelter, clothing, health and other basic needs and that the propensity to consume would be close to one in the short-term. However, once households meet these demands, they may opt to spend the transfer to invest in human capital or other areas.

The cash transfer can exert a direct impact. First, as discussed above, UCTs induce a pure income effect: households have higher incomes that can be spent to address their needs according to their preferences and the constraints faced (Forget et al., 2013). In order to be able to save money or invest, households need to reach a minimum income level. Second, as suggested by the permanent income hypothesis (Friedman, 1957), if households perceive the cash transfer as temporary, they might save most of it rather than consume it; therefore, transitory cash transfers might increase precautionary savings aimed at smoothing income fluctuations (Fiszbein and Schady, 2009; Haushofer and Shapiro, 2013; Stoeffler and Mills, 2014; Gertler et al., 2006).[61]

The impact of the cash may work indirectly through other channels (mediators). Impacts on women's savings could be facilitated by a change in family dynamics such as an increase in women's decision-making, in particular related to control over resources within the household. Literature, particularly from CCTs in Latin America, has shown that the transfer might increase the bargaining power of women or raise proxy measures of women's status within the household (Fiszbein and Schady, 2009; van den Bold et al., 2013, among others). Similarly, the impact might be mediated through changes in aspirations or future-oriented behaviour. For instance, Bernard et al. (2014) report the findings from an experiment in rural Ethiopia aimed at improving aspirations; not only did the programme improve aspirations but treatment effects were also found on, among others, savings behaviour, suggesting that changes in aspirations could lead to changes in forward-looking behaviour.

In addition to the pathways mentioned above, the strength of the direct impact of the programme could be moderated by community or external factors. The impact of the cash transfer might be stronger or weaker depending on the exposure to shocks, access to markets and services,

[61] Also the Ricardian equivalence (Barro 1974) predicts that – following a cash transfer from the government – individuals will reduce their consumption and increase private savings, in anticipation of future increases in taxes.

or, more generally, infrastructure or price dynamics within any given setting (moderators). Other moderators include household or individual-level characteristics such as household size, the age, education or marital status of the recipient.

Finally, as related to several pathways mentioned previously, it is important to consider the time horizon of the programme and evaluation in question, as savings and re-investment decisions may vary over the duration of the programme (King and Behrman, 2009).

3.3 Review of the evidence on the impact of cash transfers on savings

The empirical literature on cash transfers and their impacts on savings is thin, not only in Africa but also in Latin America within the first generation of CCTs (Fiszbein and Schady, 2009). Investments and savings are not always observed separately and are therefore difficult to disentangle empirically from each other within evaluations. In addition, there exists potential to misclassify categories of resources held by poor households. For example, changes in livestock ownership could be considered either as productive assets or as savings, particularly in the absence of formal financial services, depending on how they are viewed and utilized by the household.

In general, most of the existing evidence comes from CCTs and Latin America. This literature points to the fact that households do not spend all the cash transfer but tend to save part of it. Rubalcava et al. (2009) highlight that participant households of the Progresa transfer programme receive a relatively large benefit amounting to almost a third of average monthly per capita consumption (or slightly over 30 pesos per capita per month). The authors show that programme beneficiaries save part of the transfer: income is higher than expenditure for both treatment and control households; however, treated households save almost 13 pesos per capita per month more than controls, considering the post-programme average (of three follow-ups at 6, 18 and 24 months). The difference in these savings between treatment and control is primarily (around 70 per cent) accounted for by the fact that treatment households own significantly more small livestock, a source of savings in the absence of financial institutions. Since small livestock

and poultry are typically women's investments, the authors suggested that the use of the benefit for investment reflected women's control over the transfer.

Gertler et al. (2006) show, using later evaluation rounds, that Oportunidades (formerly Progresa) participants used 88 per cent of their transfer for consumption goods and services and invested (or saved) the remaining 12 per cent. The programme enabled poor rural households to overcome liquidity and credit constraints and as a result led to an increase in productive investments: households invested in agricultural activities and non-farm enterprises with an estimated rate of return of 17 per cent, thus generating higher income. In a follow-up study (Gertler et al., 2012), the authors found that household investments had grown to about 26 per cent of the transfer. Ribas et al. (2010) investigate the effect of Tekoporã, a Paraguayan CCT, on savings (among other things). Programme participants saved 20 per cent more than the control; whereas control households spend on average more than they earned, income for treated households was approximately three per cent higher than their expenditures. Angelucci et al. (2012) study the impacts of the Oportunidades CCT in Mexico on, among others, household savings. The authors find a positive effect on the probability of having savings (between 6 and 13 percentage points), but no impacts on the amount of savings.

More recently, there has been a tendency towards savings-linked CCT interventions that combine the income support with the provision of access to saving services as a way to increase financial inclusion; these programmes have been implemented mainly in Latin America. There are two main types of savings-linked CCTs: on one hand, a traditional CCT programme aimed at fostering human capital accumulation can be adapted to include a formal savings component (hybrid programme); on the other hand, a CCT is designed with the primary goal of mobilizing savings behaviour (Winkler, 2014). An array of savings-linked CCTs exists such as the 'simple saving account', that directly pays the transfer into saving accounts (implemented in Mexico under Oportunidades); the 'personal capitalization accounts' where matching grants are deposited to reward participants who save – this instrument's ultimate goal is to promote investment of accumulated savings (see the Personal Capitalization Account Pilot in Peru); the 'child and youth accounts' that are targeted at children and youth to help finance schooling or other investments

through the accumulation of assets (see Jovened con Oportunidades in Mexico or SCAE – Subsidios Condicionados a la Asistencia Escolar in Colombia) (Zimmerman and Moury, 2009). The global experience so far is fairly limited, and to the best of our knowledge no impact evaluation has been carried out, therefore no conclusive evidence on their performance is available. Mechanics and incentives in CCT programmes, including savings-linked CCTs, are likely to differ significantly from those of UCTs.

Mechanics and incentives in CCT programmes – and in savings-linked CCTs – are, however, likely to differ from those of UCTs.[62] Evidence from social cash grants (or UCTs) is still scant, relatively recent and comes from outside Latin America, but it still points to the same general conclusions: households tend to save part of the transfer. For example, Duflo (2003) assesses the impact of the South African Social Pension programme and find that pensions received by women have a larger impact on savings as compared to those received by men; whereas on average 50 per cent of *non*-pension income is saved overall, women save 82 per cent of the transfer/pension and men save 53 per cent of the transfer/pension (although this difference is not significant).

Also from South Africa, Delany et al. (2008) find that, after almost 10 years, the Government's Child Support Grant recipients are more likely to have some forms of savings compared to eligible non-beneficiaries (21 per cent versus 11 percent); participant households are also more likely to have a bank account (42 per cent versus 24 per cent) – it is however unclear whether this finding is driven by the fact that the grant can be paid into a bank account (this is not a necessary condition) or by the fact that beneficiaries have more money to open a bank account. Ravallion and Chen (2005) analyze temporary poverty-focused cash transfers under China's Southwest Poverty Reduction Project. Findings highlight that households saved rather than consumed the majority of the transfer due to the perception that the programme was transitory.

Stoeffler and Mills (2014) investigate the long(er)-term impacts of a cash transfer project that lasted 18 months in rural Niger by studying the effects one year and a half after its

[62] It is still debated whether cash transfers should include conditions or not. Baird et al. (2014) try to assess the relative effectiveness of CCTs and UCTs; their review however focuses on education outcomes only.

termination; as part of the pilot project households were encouraged to set up local rotating savings groups aimed at savings for productive investments. The authors find that among the beneficiary group the participation in savings groups increased by 8 to 9 percentage points and the amount saved increased by around 65 per cent.[63] Blattman et al. (2016), investigated the impacts of a cash transfer programme called Women's INcome Generating Support (WINGS) implemented in Northern Uganda that, in addition to providing a one-off lump sum grant (USD 150), encouraged ultra-poor women to start non-farm businesses by offering a 5-day business skills training and ongoing supervision. Sixteen months after the first transfer, the programme had tripled the amount saved and had doubled participants' micro-enterprise ownership and earnings.

Most similar to our study, recent research by Haushofer and Shapiro (2016) investigates the experimental impacts of a UCT targeted to poor households in Kenya over a one-year period using data from a randomized controlled trial (RCT). Between 2011 and 2013, programme beneficiaries received transfers of at least USD 400 from the non-governmental organization GiveDirectly, through the mobile money system M-Pesa. The transfers were intentionally large and transitory, either a lump sum or monthly installment over nine months. Authors report impacts on a broad spectrum of outcomes, including cash savings: initial levels of cash savings (fairly low at USD 10) were doubled thanks to the programme and larger impacts were recorded for households receiving larger payments; however, there was no significant difference dependent on the gender of the transfer recipient.

3.4 The Zambian Child Grant Programme and study design

The CGP was originally implemented in three rural districts of Zambia, selected based on their high poverty and child malnutrition rates: Kaputa in Northern Province, and Kalabo and Shangombo in Western Province (see map, Figure B.2 in Appendix B). It was established by the

[63] The authors find that the use, number, and amount from tontines (local rotating savings and credit associations), increased significantly among programme participants. They also find the programme significantly increased livestock assets as well as having productive impacts on micro-enterprises and agriculture.

Zambian Ministry of Community Development and Social Services (MCDSS)[64] in 2010 and reached 20,000 households by 2012. The overarching goal of the programme was to reduce extreme poverty and the intergenerational transfer of poverty, while the primary objectives focused mainly on the health and development of very young children but also included supplementing income, increasing assets and improving food security.

The target group comprised all households with a child under the age of three years at programme initiation.[65] The primary caregiver or mother of the target child received the benefits, thus virtually all recipients were women.[66] Beneficiary households received 55 ZMW (Zambian Kwacha), or approximately USD 12 per month (paid bimonthly), irrespective of household size. The transfer represented a 27 per cent increase to the household's baseline monthly expenditure and was calculated as an amount sufficient to purchase food equivalent to one meal per day on average for all household members for a month. The value of the transfer was adjusted over time to keep up with inflation (reaching ZMW 70 by the end of 2014). The transfer was distributed through a local pay-point manager and results from an operational audit suggested that the programme administration largely functioned as expected (AIR, 2011). Households were expected to graduate from the programme when the target child turned five years old, however operational information indicated that this rule was not uniformly implemented in a timely manner across programme areas.

The impact evaluation of the CGP was commissioned by the Government of Zambia and UNICEF as part of the Transfer Project, a consortium of international research partners, civil society and national governments, to support improving knowledge and practice on cash transfers in Africa. The impact evaluation consisted of a longitudinal cRCT with one baseline (2010) and four subsequent follow-ups (at 24, 30, 36 and 48 months). An experimental design was feasible

[64] The Ministry changed name to Ministry of Community Development, Mother and Child Health (MCDMCH) in 2013 and then most recently to Ministry of Community Development and Social Services (MCDSS).
[65] Although the CGP included eligibility for all households with a child under the age of five, a more stringent cut off was utilized for the evaluation sample, such that each household would have a child who did not age out of the programme within a 24-month period.
[66] The CGP defines recipients as the female head of household, who can be a mother or a grandmother. If no female head of household is present, the father can be named the household recipient (AIR, 2011).

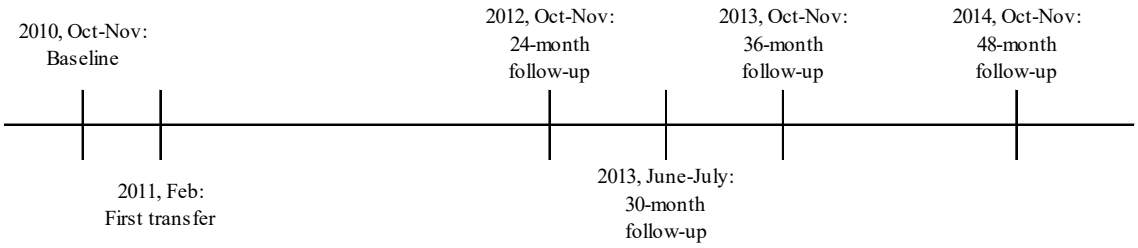
because the government was not able to immediately scale-up the programme in the three initial districts due to financial and human resource constraints. In each of the three (geographically targeted) districts, 30 communities[67] (hereafter referred to also as clusters) were randomly selected (out of a total of approximately 100) by lottery to appear in the study. Within each of these 90 clusters all eligible households were identified[68] (more than 100 eligible households in each cluster) and from this list, 28 households were randomly selected to enter the study sample leading to a representative sample of 2,519 households that met the targeting criteria across 90 clusters in three districts.

The baseline survey was conducted in October-November 2010 prior to the start of the programme; hence, study participants were blinded at baseline. After baseline, a coin flip conducted by the Permanent Secretary of the MCDSS determined which group of randomly selected clusters would be in the early treatment or the delayed entry control (45 in each treatment and control). The treatment group received its first transfer in February 2011 and the delayed entry control group was to receive transfers after the study period.[69] This analysis uses data from the baseline, 24-, 36- and 48-month waves. The 30-month wave was a shorter survey to assess the impact of the programme on consumption smoothing, fielded during the harvest season, and the survey instrument is thus less comparable to those from other survey rounds collected during the lean season.

[67] These administrative and geographic units in Zambia are usually referred to as CWACs. These Community Welfare Assistance Committees represent the authorities at the community level.
[68] The identification process entailed house to house visits coupled with public awareness campaigns.
[69] The delayed-entry or control group was expected to start receiving the grant at the end of the experimental evaluation period. However, over the last decade, the Government of Zambia tried several different models of social cash transfers and decided to go for a 'unique' model – an inclusive labour-constrained one – and expand coverage (reaching 175,000 beneficiary households as of 2015). CGP participants as well as the control group are being retargeted for the new national programme. The control group was given a lump sum 'compensation' payment after the 48-month survey. In 2017, the Government of Zambia has scaled up the programme to national scale; the target caseload for 2017 is 590,000 and 700,000 for 2018.

Figure 3.1 represents the evaluation timeline while Figure B.3 in the Appendix presents the flowchart of the study design.

Figure 3. 1 – Timeline of the events in CGP evaluation



The main survey instrument is extensive, and includes, among others, modules on consumption, health, education, housing, agricultural and other productive activities. During each wave, a module on women's empowerment, including indicators on decision-making, savings and future expectations was administered to one woman in the household, typically the biological mother or primary care-giver of the target child. In order to avoid potential bias, the interviewers were clearly instructed during the survey training to carry out interviews privately (one to one). Our key indicator on cash savings comes from this module. The study sample size was powered to detect significant effects for child anthropometry measures, which is the smallest sub-group of analysis in the sample (i.e. children under the age of five), accounting for non-response and attrition rates. The study underwent ethical review at the American Institutes for Research (AIR) in Washington, D.C. and at the University of Zambia in Lusaka. Questionnaires and reports for the CGP are available on the Transfer Project website (http://www.cpc.unc.edu/projects/transfer).

3.5 Data, attrition and methodology

3.5.1 Data

The full baseline sample contains 2,519 households and 14,565 individuals. Our analysis sample comprises all female respondents to the empowerment module. We exclude the one per cent of male respondents who answered this module in the absence of a qualifying female respondent. In total the pooled cross-sectional sample consists of 9,584 observations, which includes all women

interviewed at least once in any of the four waves (Table 3.1, column A). A slightly smaller number of women, 9,431 (column B) were interviewed at least twice across the four waves while 1,983 women were interviewed in all four rounds representing a total sample of 7,932 (column C). The complete (balanced) panel is the sample we utilize for subsequent analysis;[70] however, sensitivity analysis shows that results are consistent across all three samples.

Table 3.1: Samples of women answering empowerment module across waves in the Child Grant Programme evaluation

	Column A Cross-sectional	Column B Unbalanced panel	Column C Balanced panel
Baseline	2,492	2,442	1,983
24-month follow-up	2,284	2,253	1,983
36-month follow-up	2,421	2,387	1,983
48-month follow-up	2,387	2,349	1,983
Total	9,584	9,431	7,932

Note: One woman per household, targeting the cash transfer beneficiary, answered the empowerment module. I exclude the one per cent of male respondents who answered this module in the absence of a qualifying female respondent.

Table 3.2 reports the background characteristics of the panel of women and the households they live in. The mean age of women respondents is 30, 73 per cent are married, while few are divorced or separated (10 per cent), widowed (6 per cent) or never married (11 per cent). Women have low levels of education, approximately 30 per cent have never attended school and around 71 per cent did not complete the primary level (not shown in Table 3.2). The mean household size is six and the mean number of children aged zero to five years is 1.9 per household. There are very few elderly people in these households and the majority of adults (1.3 on average) are in the age range 19 to 35 years. Finally, the sample is poor with a mean monthly per capita expenditure of ZMW 40, or approximately USD 0.30 per person per day, well below the 2010 national extreme poverty line (ZMW 90.5 per capita); poverty rates at baseline are exceptionally high in our sample with 95 per cent of women/households living in extreme poverty.[71]

Overall, randomization was successful in producing balanced treatment and control groups. We found no significant differences between treatment and control women along a

[70] The balanced panel sample of women are those who were interviewed in each of the four waves and do not have missing values for any of the outcome or control variables used.
[71] According to the Official Evaluation report (AIR, 2011, p. 25) in the CGP sample 95 per cent of individuals were living in extreme poverty compared to 69 per cent in rural Zambia and 85 per cent in the three districts.

number of household and individual characteristics, including key outcomes of interest, the proportion of women savers and amount saved (see Table 3.2, column 4). No indicator is statistically significant at the five per cent level. Thus, we conclude that testing for equivalence at baseline confirms the successful random assignment of the programme.

Table 3.2: Baseline characteristics of women (balanced panel) and test for equivalence at baseline

	All	Control	Treated	P-value of diff. Col(2)-Col(3)
	(1)	(2)	(3)	(4)
Age (years)	29.34	29.21	29.48	0.65
Age squared (years)	938.18	927.92	948.60	0.64
Ever attended school	0.71	0.70	0.73	0.36
Never married	0.11	0.11	0.11	0.85
Divorced or separated	0.10	0.11	0.08	0.07
Widowed	0.06	0.06	0.05	0.50
Household characteristics				
Shangombo district	0.37	0.37	0.37	0.99
Kaputa district	0.28	0.29	0.28	0.90
Consumption expenditure per capita (ZMW)	40.31	39.56	41.08	0.59
Household size	5.66	5.61	5.71	0.58
Number of members aged 0-5	1.90	1.91	1.90	0.76
Number of members aged 6-12	1.26	1.27	1.25	0.83
Number of members aged 13-18	0.55	0.52	0.58	0.29
Number of members aged 19-35	1.34	1.30	1.37	0.22
Number of members aged 36-55	0.53	0.52	0.53	0.81
Number of members aged 56-69	0.06	0.06	0.06	0.93
Number of members aged 70+	0.03	0.03	0.03	0.96
Key outcome measures				
Proportion of women holding any savings	0.17	0.16	0.18	0.44
Amount saved (ZMW)	13.46	14.21	12.70	0.64
Amount saved (logged)	0.62	0.59	0.66	0.54
Additional indicators for sensitivity analysis				
Household owned any milk cows (last 12 months)	0.06	0.07	0.05	0.50
Household owned any cattle (last 12 months)	0.11	0.11	0.10	0.63
Household owned any goats (last 12 months)	0.02	0.01	0.03	0.09
Household owned any chicken (last 12 months)	0.44	0.44	0.44	0.92
Household owned any ducks (last 12 months)	0.03	0.04	0.02	0.26
Household owned any livestock (last 12 months)	0.51	0.51	0.51	0.97
N	1,983	999	984	

Note: T-tests based on standard errors clustered at the community level.

3.5.2 Attrition

Despite successful randomization and equivalence of treatment and control samples with respect to background characteristics, it is also important to examine threats to validity of causal inferences (internal validity) due to overall and differential attrition. Household attrition was nine per cent at 24 months, two per cent at 36 months and 4 per cent at 48 months (see Table B.1 in Appendix B). The higher attrition rate at 24 months is driven by out migration in Kaputa District

as a result of the drought-induced drying of the Cheshi Lake, a source of livelihood for households in the area. The majority of out-migration took place over the initial study years, and many of these households returned by the 36-month follow up. Household-level analysis of overall and differential attrition was conducted as part of the larger evaluation and based on results it was concluded that neither is a threat to internal validity (AIR, 2013; AIR, 2014; AIR, 2016).

For the current analysis, we conduct attrition analysis at the individual (women) level. Overall attrition based on our analysis sample is higher than that at the household level with 20.4 per cent of the baseline sample (2,492 women) lost to follow-up (see Table B.2 in the Appendix); however, we find no evidence of differential attrition between treatment arms. The higher attrition rate as compared to the household-level analysis could be due to several factors, including individual out-migration of women from survey households, or temporary absence during the fieldwork period, which resulted in a different respondent being selected for the empowerment module.

We further investigate differential attrition by background characteristics across treatment and control groups using the same core characteristics reported in Table 3.2. Table B.3 in the Appendix shows differences in baseline characteristics between panel women and those lost to follow-up by treatment arm.[72] Differential attrition is assessed in columns 7 and 8, where we test for significant differences in means across the groups lost to follow-up in the two study arms. The number of members aged 13−18 is significantly higher in the treatment group lost to follow-up, in comparison to the control group lost to follow-up, however the difference is small in magnitude. None of the other differences are statistically significant, suggesting that, consistent with the household-level analysis, individual attrition does not threaten the internal validity of our results.[73]

[72] Table B.3 in the Appendix shows differences in baseline means 1) between those attriting and non-attriting in the control arm; 2) between those attriting and non-attriting in the treatment arm; and finally 3) the difference between those attriting in the treatment and the control arm. Bullet points 1) and 2) help to understand whether attrition is random; bullet point 3) highlights whether attrition is random across the treatment and the control group.

[73] Moreover, individual fixed effects are used in the main analysis. This is another way to counter endogenous selection into remaining in sample.

3.5.3 Key indicators and analysis methodology

Women are asked whether they are currently saving in cash and how much they have saved in the last month. The primary measure of current cash savings was collected using the following question: *"Some people try to save some money for emergencies or to buy something special in the future. Are you currently saving (in cash)?"*, and the secondary savings outcome is value of cash saved in the last month (logged) *("How much have you saved in cash in the last one month?"*). Our measure of savings amount is from the last month so there is likely to be some fluctuation over the course of the year although all four survey rounds were conducted during the same period. In the Section on robustness checks (3.6.3), I also explore other forms of savings – mainly livestock – which are measured at the household level.

In order to estimate impacts on women savings, we estimate the following difference-in-differences (DD) model on women interviewed in all four survey rounds:

$$Y(i,j,t) = \alpha + \beta_T T(j) + \beta_{R2}(R2) + \beta_{R3}(R3) + \beta_{R4}(R4) + \beta_{TR2}(T(j) * R2) + \beta_{TR3}(T(j) * R3) + \beta_{TR4}(T(j) * R4) + \sum_{k=1}^{K} \theta_k X_k(i,j,t) + \varepsilon(i,j,t) \qquad (1)$$

In this framework, $Y(i,j,t)$ is the the outcome indicator for the individual woman *i* in community *j* at time *t,* and is equal to one if the woman is currently saving in cash (0 otherwise) in our first estimation and is equal to the amount saved (logged) in our second estimation. $T(i)$ is a binary indicator of treatment status, equal to one if in the treatment group, *R2*, *R3* and *R4* are indicators for the three time periods where *R2* refers to the 24-month follow-up, *R3* to the 36-month follow-up and *R4* to the 48-month follow-up while β_{TR2} ,β_{TR3} and β_{TR} capture the intent-to-treat (ITT)[74] effect at times two, three and four respectively; *X* is a set of basic control variables

[74] Analysis of administrative data by the study team indicates that not only were payments made on schedule during the study period, but that over 95 per cent of beneficiaries collected their payments on time. An operations module fielded as part of the evaluation did not reveal any indication of leakage due to bribes or requests for payments from village elders or programme officials. The ITT will thus be very close to the average treatment effect on those treated up to 36-months. After 48 months, some households whose target child turned five started graduating; indeed, around 25 per cent of households report not receiving the transfer at 48 months. (In line with graduation criteria, the proportion of households in my treatment sample that report not being a beneficiary of the programme increases over time: at 24 months less than 4 per cent of treatment sample households report not currently being a beneficiary of the programme compared to 10 per cent after 36 months and 28 per cent after 48 months).

that are all measured at baseline and ε is the error term. Robust standard errors are adjusted for clustering at the community level.[75]

Although with successful randomization covariates are not necessary to estimate an unbiased ITT estimate, we also present estimates adjusted with a vector of pre-treatment covariates to increase the precision of the point estimates and to account for any residual imbalances between treatment and control. Unadjusted specifications include district dummies as this was the stratifying indicator for the randomization.[76] In the multivariate model, our set of basic demographic covariates include: 1) women's characteristics (age in years and its square, whether the woman has ever attended school and splines for marital status, where the omitted indicator is currently married or cohabiting); and 2) household characteristics (log of household size, a set of indicators capturing household composition and district of residence indicators).[77] Means for these variables are presented in Table 3.2.

Equation (1) is estimated over the (balanced) panel; results were replicated for the pooled cross-section and the unbalanced panel. For clarity of exposition, and given findings are consistent across the three samples, results are reported for the balanced panel of women only.

We discuss hereafter two sets of threats to identification. The first one is anticipation of treatment. As this is a delayed-entry type randomization, it is possible that individuals in the control group might change their behavior in anticipation of the expected roll-out of the programme to them. We do not expect anticipatory effects to play a big role. If households in the control group wanted to increase consumption in anticipation of future cash transfers, then they

[75] Randomization happened at the community level and households are clustered in communities. Cluster-robust standard errors account for the lack of independence across observations due to the nested nature of households in communities. It is assumed that correlation across communities equals zero, but the within-group correlation is allowed to vary.

[76] Main differences are thought to exist between districts and not between communities within districts. Randomization was at community level within districts, so communities were expected to be balanced. In any case, estimations were also carried out using community fixed-effects that control for time-invariant characteristics of a community that may affect the outcomes of interest.

[77] Control variables include: women's age in years and women's age in years squared, whether the woman ever attended school (binary), her marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', household size (logged), number of members in different age groupings: aged 0-5 years, 6-12 years; 13-18 years; 19-35 years; 36-55 years; 56-69 years; 70+ years; and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo). Household size is logged in order to be able to include household composition controls.

would either need to reduce their savings or borrow. However, data indicates that consumption increased over time in the control group, but so did savings. Moreover, it is unlikely that these extreme poor households might be able to rely on their savings or borrow for such long period (Attanasio and Mesnard, 2005) and in any case, there would have been some degree of uncertainty with respect to the actual roll-out of the programme (Boone et al., 2013). If anticipation effects are at play then, we would understate the impact on savings and our findings could then be interpreted as lower bound estimates of the true impact.[78]

A second threat is related to the control group behaviour. Control households might underreport well-being, either mistakenly thinking that they could gain eligibility into the programme or due to disappointment effects that could derive from the false expectation of the control group of receiving the cash transfer in the future. If the control group under-performs, this could bias the treatment effects upwards. However, the CGP is not explicitly poverty targeted, even though potential beneficiaries might not be completely aware of all the programme details.

Programme field reports indicate that households in the control group had understood that eventually they would enter the programme.[79] Control households would then have been 'disappointed' by the announcement made during endline data collection that the CGP was coming to an end. However, the endline field report indicates, even if anecdotally, that some control households were rather excited at the idea of being compensated.

We believe underreporting is an unlikely threat to our findings. Consumption in the control group mirrors closely GDP per capita growth by increasing over time between 2010 and 2013, then dropping and stagnating thereafter (see Handa et al., 2018). Data was collected by a private firm and during data collection no mention of the programme or the Ministry implementing it was made. Moreover, it would be difficult to consciously underreport consumption given it asks about over 200 expenditure items (Handa et al., 2018); and

[78] It has also been suggested that the expectation of receiving cash transfers could have an insurance effect by increasing investments in the control group (Bianchi and Bobba, 2013).
[79] Their level of understanding however is not clear and would have likely been accompanied by uncertainty.

underreporting would have to be systematic within the other economic variables considered including cash savings, livestock and investments.

3.6 Results

3.6.1 Simple difference-in-difference and descriptive statistics

A simple difference-in-difference for women's cash savings outcomes is shown in Table 3.3. Comparing each outcome in the three panels across columns reveals changes over time for each arm, whereas comparing rows shows the differences in each outcome across arms at a specific point in time. Table 3.3 reports first differences − between control and treatment at a specific time and between baseline and follow-up for each arm − as well as double differences at each follow-up.

At baseline, the proportion of women who save is balanced between the treatment and the control group and is in the 16−18 per cent range. However, after 24 months of receiving transfers, this percentage more than doubles in the treatment group, passing from 18.1 per cent to 47.6 per cent, whereas it only increases slightly in the control group (from 15.6 per cent to 21.8 per cent). After three and four years, the percentage of women holding cash savings in the treatment group has decreased to 35.1 and 36.5 per cent respectively but it is still higher than the corresponding figures for the control group (22.3 and 19.2 per cent at 36 and 48 months).

Additional descriptive information indicates that cash savings are typically kept at home (93 per cent at baseline), confirming that these women most likely do not have access to institutions or other formal saving facilities.[80] No significant differences are found between treatment and control groups regarding the place of saving (not shown).

The middle panel of Table 3.3 shows that at baseline the amount saved in the past month does not differ between the treatment and control group (around ZMW 14, approximately USD

[80] Three per cent of women savers report keeping their savings at a bank or post office, whereas around 1.5 per cent report either an informal saving scheme, a shop or an non-governmental organization / microfinance institution. These statistics do not change significantly over time.

2.8, which represents around a third of monthly baseline consumption). However, at follow-ups, women in the programme save ZMW 48 to 56 more than at baseline – more than a threefold increase with respect to initial levels of savings; women in the control group also save more, but the increase at follow-ups with respect to the baseline ranges between ZMW 7 and 14. These statistics refer to the full sample and not only to the proportion of women savers.

In the treatment group, the proportion of women savers increases in the first follow-up but then decreases in later rounds, while the amount saved rises in first follow-up and then stays relatively stable afterwards. The bottom panel of Table 3.3 reports the amount saved for women savers only and helps to clarify this differential trend. The amount saved by women savers only in the treatment group keeps growing over time. This suggests that women saving small amounts at 24 months might have stopped saving by 36 months; impacts on the amount saved at 36 and 48 months would then be driven by those women who were saving relatively more at 24 months, and that continue to do so at later waves. We discuss further this explanation in the next section.

Differences between baseline and endlines are significant at the 5 per cent level not only in the treatment but also in the control group at least until the 36-month follow-up for the top two panels. The fact that the control group is also gaining could be linked to anticipatory effects which we discussed in the section on empirical strategy but also to Zambia's robust economic growth (World Bank, 2013) and in particular to Zambia's bumper harvests of maize starting from 2010.[81] As maize accounts for 90 per cent of total cereal output in Zambia, it is reasonable that favourable maize harvests might have increased incomes for the control group too, allowing them to save more over time.

In our sample, at baseline, almost 80 percent of households are crop producers. Maize is the most important food crop with roughly 74 percent of agricultural producers growing it,

[81] In May 2010 (before baseline data collection), Zambia recorded an unprecedented bumper harvest of more than 2.7 million tonnes of maize, representing a 48 per cent increase from the previous year. The previous record harvest was 1.9 million tonnes in 1989. In 2011, the maize harvest was even greater (3.0 million); in 2012 (the year of the 24-month follow-up), the maize harvest (2.8 million) was lower than in 2011 but still higher than the bumper harvest of 2010; finally, in 2013 (the year of the 36-month follow-up), there was a reduction in the maize harvest with respect to 2010, but maize production was still higher than the average (2.5 million) (Faostat Database).

followed only by cassava and rice (around 30 and 20 percent respectively). The share of maize producers is balanced between treatment and control arms at baseline (75 versus 72 percent respectively) and on average producers report 1.25 crops on average (crop diversification); whereas only roughly 5 percent of crop producers are involved in other agricultural activities such as livestock rearing (agricultural diversification).

Given that the bumper maize harvests affected equally treatment and control, the validity of the estimates is not undermined (internal validity). The specific circumstances could however affect the external validity of the impact estimates for the future. If, at baseline, everyone had a higher than average amount of maize, consumption and possibly savings, then the true impact of the intervention at follow-ups might be underestimated. Alternatively, if saving is only possible once a minimum income level is reached (and the maize boom helps in reaching this level) the true impact of the programme might be overestimated (upper-bound estimate). Based on these hypotheses, it is therefore difficult to conclude whether ITT estimates provide an upper or a lower bound of the true treatment effect.

In summary, the basic difference-in-differences analysis highlights a positive impact of the programme on the extensive and intensive margins of savings. These findings will be corroborated in the next section using a regression framework that also permits introduction of controls.

Table 3.3: Difference-in-difference: Mean women's cash savings outcomes by treatment status and wave, balanced panel

		Baseline	**24-month follow-up**	*Difference (24 months/baseline)*	**36-month follow-up**	*Difference (36 months/baseline)*	**48-month follow-up**	*Difference (48 months/baseline)*
Proportion of women savers	Control	15.6	21.8	*6.2**	22.3	*6.7***	19.2	*3.6*
	N	*999*	*999*		*999*		*999*	
	Treated	18.1	47.6	*29.5****	35.1	*17.0****	36.5	*18.4****
	N	*984*	*984*		*984*		*984*	
	Difference (T-C)	*2.5*	*25.7****	***DD 24 months 23.3****	*12.7****	***DD 36 months 10.3****	*17.3****	***DD 48 months 14.8****
Amount saved (ZMW)	Control	14.2	23.1	*8.9***	28.7	*14.5****	21.2	*7.0**
	N	*999*	*999*		*999*		*999*	
	Treated	12.7	60.5	*47.8****	55.7	*43.0****	69.2	*56.5****
	N	*984*	*984*		*984*		*984*	
	Difference (T-C)	*-1.5*	*37.4****	***DD 24 months 38.9****	*27.0****	***DD 36 months 28.5****	*48.0****	***DD 48 months 49.5****
Amount saved (ZMW) conditional on saving	Control	91.0	105.8	*14.8*	128.6	*37.6*	110.2	*19.2*
	N	*156*	*218*		*223*		*192*	
	Treated	69.9	126.9	*57****	158.7	*88.8****	189.6	*119.7****
	N	*178*	*468*		*345*		*359*	
	Difference (T-C)	*-21.1*	*21.1*	***DD 24 months 42.2****	*30.1*	***DD 36 months 51.2**	*79.4****	***DD 48 months 100.5****

Note: N for full sample is 7,932 in the top two panels and 2,139 in the bottom panel. *** p<0.01, ** p<0.05, * p<0.1- Robust standard errors clustered at the community level.

We explored motives behind savings using a question collected only in the 36-month and 48-month surveys which asks women who save in cash what are the three most important reasons for saving. Women's reasons are classified among more than ten options, including purchasing bulk or other food items, household consumables, agricultural inputs, assets to start a new small business and so on. Table B.4 in the Appendix shows no difference across arms in reasons for savings at both follow-ups. we further classify these reasons into precautionary versus investment categories and create three mutually exclusive groups defined by whether the woman saves mainly for investment reasons, mainly for precautionary reasons, or for both reasons (see Table 3.4 footnotes for details on the classification).

The main reason for saving among both groups is to smooth income fluctuations (Table 3.4: 74 and 59 per cent at 36 and 48 months respectively) and the three most reported specific reasons are to: 1) purchase bulk or other food items; 2) purchase household consumables; and 3) for medical expenses or health care. At 36 months, there are more women in the treatment arm who save mainly for investment purposes (11.7 per cent in the control group versus 17.4 per cent in the treatment group). However, the 5.7 percentage point difference between arms is only marginally significant at the 10 per cent level, and at 48 months the difference is no longer statistically significant. These statistics should be interpreted carefully; indeed, the treatment and the control groups might not be balanced anymore as there are more women in the treatment group who save; moreover the sample size is relatively small.

Table 3.4: Self-reported reasons for women's cash saving at 36 and 48 months

		Reasons for saving			
		Mainly investment	Mainly precautionary	Investment and precautionary	No reason reported (.)
36 months	Control	11.7	77.6	9.0	1.8
	Treated	17.4	71.9	9.6	1.2
	Difference	5.7*	-5.7	0.6	-0.6
48 months	Control	0.29	0.55	0.15	0.02
	Treated	0.25	0.61	0.11	0.03
	Difference	-0.04	0.06	-0.04	0.01

Note: N is 568 and 551 panel women who report any cash savings at 36 and 48 months respectively. Differences between treatment and control *** p<0.01, ** p<0.05, * p<0.1. T-tests based on standard errors clustered at the community level.
Investment reasons include: school fees/schooling expenses; to purchase household durable assets; to purchase livestock; to purchase agricultural inputs or tools; to purchase assets to start a new small business; to make home improvements; and to purchase new land or house. The following reasons were classified as precautionary: to purchase bulk or other food items; to purchase household consumables; to buy new clothing/shoes; medical expenses/health care; to repay debts; to spend on services.

3.6.2 Child Grant Programme impacts on cash savings

It has already been shown elsewhere (AIR, 2013; AIR, 2014; Seidenfeld, et al., 2014a; Handa et al., 2014) that the CGP has a positive impact on household and food consumption as well as on food security. Findings reported in this Section suggest however that the impact of the unconditional CGP in Zambia goes beyond merely satisfying basic needs; indeed, once poor households meet their basic short-term subsistence requirements, the programme has the potential to impact other areas.

Table 3.5 displays the impact estimates of the CGP on women's cash savings, both in terms of extensive (columns 1 and 2) and intensive margins (columns 3 and 4). Overall, we find positive programme impacts on the probability of holding savings as expected by the simple difference-in-difference reported in the previous Section. After 24 months the programme significantly increased the proportion of women savers in the treatment group by an additional 23 percentage points, by 10 percentage points after 36 months and by 15 percentage points after 48 months of implementation. The average impact across the three follow-up waves is 16 percentage points and estimates are nearly identical between unadjusted and adjusted models.

The 24-month effect is larger than the 36-month one and the difference is statistically significant at the 1 per cent level whereas there is no statistically significant difference between the 36- and 48-month estimate. As highlighted in the previous Section, this could be explained by a compositional shift where women who saved small amounts stopped savings after two years while only women with relatively larger savings continued to save.

More specifically, there might be a threshold level of income above which beneficiaries are able to save; the level of income does depend on the grant received but also on the level of other incomes that vary with the economic situation of the country. In particular, in Zambia – as already highlighted − a series of bumper maize harvests occurred between 2010 and 2012, whereas in 2013 (the year of the 36-months follow-up) for the first time there was a contraction in the maize production as compared to 2010 levels. This contraction probably led to a fall in

household incomes, possibly limiting the ability to save for poorer households and women who are not able to reach this minimum income level anymore.

Alternatively, some women may not be saving, preferring to invest in human capital, economic investments and so on. Descriptive statistics reported in Table 3.4 show that more women in the treatment group saved for investment reasons; still, the difference is only marginally significant and data is not available for the 24-month follow-up. Further analysis on investments in non-farm enterprises in Section 3.7.1 however suggests that this could be part of the story.

The differential impact could also be explained if the effect of the programme dissipates over time (namely, no retention effects). If this was true we would expect a much smaller ITT estimate at endline when almost a third of households are not receiving the grant anymore due to graduation; still, there is no difference between the 36- and 48-month impacts. Considering also that the impacts on other forms of savings, shown below in Section 3.6.3, do not reveal a downward trend over time, there does not seem to be support for the hypothesis that the effects are seeping away in the longer-term.

We also exclude that the differential impact over time might be driven by attrition. Indeed, the same results hold for the unbalanced panel and cross-sectional samples (not shown); moreover, results for the 36-month estimates are consistent when the 24-month sample is excluded (in other words, only the baseline-36-48-month panel is used). We further exclude that recipients at 36 months do not feel the cash transfer as transitory, as they did at 24 months, and so do not save as much (permanent income hypothesis, discussed further in Section 3.6.3 on robustness checks.

We find a similar pattern of impacts on the amount saved in the last month (right panel of Table 3.5). The ITT effect of 1.1 at 24 months is positive and highly significant (Table 3.5, columns 3-4); this estimate corresponds to a 110 per cent increase in cash savings for the treatment group relative to the control group. At 36 months the ITT goes down to 0.53, but it is still positive and highly significant; finally, after 48 months there is a 79 per cent impact on the amount saved last month for the treatment group relative to the control. The average impact across follow-ups

is 82 per cent and it is highly significant. Again, the impacts for unadjusted and adjusted models are nearly identical.

Table 3.5: Impact on women's cash savings: proportion of women holding any cash saving and amount saved (last month, logged)

	Any cash savings		Amount saved (last month, logged)	
	(1) Unadjusted	(2) Adjusted	(3) Unadjusted	(4) Adjusted
24-month follow-up	0.0621*	0.0567*	0.247**	0.226*
	(0.0319)	(0.0321)	(0.118)	(0.118)
36-month follow-up	0.0671**	0.0593*	0.355***	0.326**
	(0.0332)	(0.0337)	(0.127)	(0.128)
48-month follow-up	0.0360	0.0267	0.193*	0.158
	(0.0286)	(0.0293)	(0.104)	(0.105)
Treated	0.0241	0.0178	0.0679	0.0373
	(0.0321)	(0.0317)	(0.112)	(0.110)
DD 24-month	0.233***	0.233***	1.112***	1.112***
	(0.0468)	(0.0468)	(0.180)	(0.180)
DD 36-month	0.103**	0.103**	0.553***	0.554***
	(0.0449)	(0.0450)	(0.180)	(0.180)
DD 48-month	0.148***	0.148***	0.787***	0.787***
	(0.0448)	(0.0449)	(0.184)	(0.185)
N	7,932	7,932	7,932	7,932
R-squared	0.072	0.079	0.087	0.096
Baseline control mean	0.16		0.59	

Notes: Estimations use difference-in-difference modeling (DD indicates treatment effect). Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Unadjusted specifications include district dummies as this was the stratifying indicator for the randomization. Estimations with adjustment include woman's age, education and marital status, household size and household demographic composition, and districts − see text for details.

Overall, these findings suggest that unconditional cash can not only raise consumption, but also women's cash savings and therefore contribute to women's financial standing. We now turn to robustness checks to show that: 1) our results are not sensitive to functional form; 2) our measures are capturing savings (rather than cash on hand); 3) cash savings are not crowding out traditional forms of savings (livestock) and 4) perceptions of transfer duration (expectations) do not seem to act as a mechanism for savings behaviour.

3.6.3 Robustness checks

Robustness checks for functional form and modelling choices

We investigate the robustness of our results to alternative specifications including individual fixed effects, analysis of covariance (ANCOVA) methods and marginal effects from a probit model. Results from individual level fixed-effects are reported in Table B.5 in the Appendix, and are identical to the results reported in Table 3.5 from our main specifications; indeed on a balanced panel, the fixed effect estimator is equivalent to a difference-in-differences model without controls estimated using OLS. We estimate the ITT effect using ANCOVA models which estimates cross-sectional impacts of the outcome indicator of interest as a function of treatment, and the value of the outcome variable at baseline. Given that the autocorrelation over time of our main outcome variables is low,[82] this specification is sometimes preferred to the DD estimates (McKenzie, 2012).

Findings are reported in Table B.6 in the Appendix. Results are comparable to the main estimates reported in Table 3.5, and standard errors are smaller, which is consistent with power gains associated with ANCOVA models. Marginal effects from a probit model are reported in Table B.7 in the Appendix. Impacts on the probability of women holding cash savings are very similar in magnitude to those reported in Table 3.5, however the 36-month impact is statistically significant at the 10 per cent level.[83] As a further check, we therefore also estimated marginal effects from ANCOVA probit (Table B.8 in the Appendix) which are highly statistically significant at all waves. Finally, even though we report results based on the balanced panel of

[82] The autocorrelation between baseline and follow-up of my saving outcomes (binary) is relatively low in the range of 0.05 and 0.07.

[83] Non-linear models – such as probit or logit – could be used rather than the linear probability model (LPM) to estimate the impact of the programme; however, the interpretation of LPM coefficients is more straightforward. The main concern with using the LPM to fit a regression with a discrete dependent variable is that it can predict probabilities outside the 0-1 interval. However, in our case only 0.11 per cent of prediction falls below 0 (and none above 1) suggesting that the scope for bias is limited. Moreover, as the majority of variables on the right-hand side are discrete (especially in the unadjusted model) the case for the LPM is strengthened (Wooldridge, 2002, Chapter 15). Indeed, non-linear models such as probit and logit in the presence of interaction terms between binary variables are more unstable (many extreme values – zeroes or ones – depending on the value of the interaction term). However, we checked for the robustness of our preferred specification by estimating a probit model too and found consistent results. Indeed, LPM and probit provide very similar results when the scope is to study the average effect (Angrist and Pischke, 2009; Hellevik, 2009).

women, sensitivity analysis shows that results are consistent across the unbalanced panel and pooled cross-sectional samples (not shown).

As for the estimation of the intensive margins, in our main estimation the amount saved is equal to zero for women who do not save; in order to accommodate the log transformation, we use log (1+amount saved). This approach is conservative and it should lead to a downward bias in our impact estimates. Following Burbidge et al. (1988), we have also used the inverse hyperbolic sine transformation with no significant change in impact estimates. Tobit modelling/estimations confirm the downward bias in our estimates.[84]

Are we actually capturing savings?

One potential critique of investigating cash savings outcomes in cash transfer evaluations is that regular payments mechanically generate cash in-hand which may be reported as savings, although in practice it may actually be used for short-term consumption purposes. In the case of the CGP, for example, payments are made on a bi-monthly basis, and respondents could have been interviewed during a period where more cash was on hand due to recent paydays.

We believe this concern does not discredit our results for three reasons. First, the survey question we use is explicit about distinguishing money for current, versus future, consumption. Second, to ensure that what is designated as savings is indeed savings, we confirm that in our sample savers have higher welfare levels compared to non-savers. Within both the treatment and control groups, mean consumption of savers is always higher than that of non-savers at each survey round. For example, among the treatment group only and averaging across all four rounds (baseline, 24-, 36- and 48-months) mean consumption among savers is significantly higher at ZMW 70 compared to ZMW 49 for non-savers.

Further, we exploit the variation in time since last payment to show that we are capturing more than cash on hand. Across the first two follow-ups, the vast majority of households in the

[84] Unfortunately, no information is available on household income, otherwise we could also compute the household savings rate, following Deaton (1997), as the difference between the logarithms of monthly income and expenditures on consumption.

treatment sample (68 per cent) reported having received the CGP payment within a month from the date of the interview and the remaining 32 per cent two or more months from the date of the interview. After 48 months, 25 per cent of households report having being paid within a month. Reported mean savings rates are no different between these treatment households which report receiving a transfer more or less recently.

In addition, the DD impact estimate on savings at 24 months is 24.8 percentage points for households that received the payment within the last month and 20 percentage points for those who received the payment two or more months ago. At 36 months the point estimates for these two sub-samples are 10.8 and 12.6 percentage points respectively. The DD impact estimates on savings at 48 months for these two sub-samples are 28 and 13.7 percentage points respectively. However, when testing the equity of coefficients between households that received the transfer more or less recently, the difference is not statistically significant at 24 and 36 months, and only marginally significant (at the 10 per cent level) at 48 months. Overall, these statistics further support the notion that impacts on cash savings in this paper are not being driven mechanically by cash on hand due to recent cash transfer payments.

Do increased cash savings crowd out other forms of traditional saving?

Poor households in rural areas without access to formal savings mechanisms typically consider livestock as a primary form of savings (Hulme and Arun, 2009). In fact, two studies that investigate the impact of Mexico's CCT on savings use indicators of livestock assets rather than actual cash as their measure of savings (Gertler et al., 2012; Rubalcava et al., 2009). One critique of my findings could be that women are substituting cash savings for more traditional forms of savings such as livestock, thus leading to no net increase in broader wealth stores. We assess this hypothesis by estimating the impact of the programme on overall household livestock ownership. Although we cannot distinguish which individual in the household is the owner, there is evidence suggesting that in many rural African societies rearing of small-scale subsistence animals such as goats, sheep and poultry falls under women's domains (Njuki and Sanginga, 2013; SOFA Team

and Doss, 2011). This same argument regarding gendered ownership of livestock is made by Rubalcava et al. (2009) in rural Mexico.

Results in Table 3.6 show that the programme had an overall positive impact on the probability of owning any livestock, whether small or large. We are particularly interested in the impact on small-scale animals: the CGP had a positive impact on the likelihood of owning chickens (between 12 and 16 percentage points up to the 36-month wave), ducks (around 3 percentage points up to the 36-month wave), and goats (3-4 percentage points, excluding at 24 months). The transfer also had an effect on the intensive margin of the number of each type of livestock (not shown). We also find an impact on owning larger, cash-generating livestock such as cattle (9-10 percentage points) in which men tend to be more involved.

Hence the increase in cash savings by women is occurring even as programme households also increase their ownership of both small and large livestock. These results suggest that the overall financial standing of women and their households has improved as a result of the programme while there is no evidence of crowding-out.

Table 3.6: Impact of the CGP on household livestock ownership (last 12 months)

Dependent variable	DD 24 months	DD 36 months	DD 48 months	Baseline, control mean	N	R-squared
Any chickens	0.127**	0.159***	0.0294	0.44	7,931	0.160
	(0.0492)	(0.0424)	(0.0395)			
Any ducks	0.0344***	0.0333***	0.0230*	0.035	7,929	0.026
	(0.0120)	(0.00926)	(0.0137)			
Any goats	0.0417***	0.0193	0.0349**	0.011	7,931	0.061
	(0.0138)	(0.0158)	(0.0152)			
Any milk cows	0.0327	0.0163	0.0656***	0.066	7,931	0.040
	(0.0242)	(0.0226)	(0.0211)			
Any cattle	0.0909***	0.101***	0.105***	0.112	7,931	0.094
	(0.0242)	(0.0299)	(0.0277)			
Any livestock	0.174***	0.183***	0.169***	0.509	7,929	0.097
	(0.0498)	(0.0421)	(0.0385)			

Notes: Estimations use difference-in-difference modeling (DD indicates treatment effect). Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Impact estimates reported are from the unadjusted model. Unadjusted specifications include district dummies as this was the stratifying indicator for the randomization; impact estimates from the adjusted model (controlling for woman's age, education and marital status, household size and household demographic composition, and districts) are consistent.

Expectations of transitory versus permanent income – are impacts fading away over time?

According to the Permanent Income Hypothesis (Friedman, 1957), if households perceive the cash transfer as temporary (rather than permanent), they may be more likely to save funds for precautionary motives, rather than use them for immediate consumption. As the magnitude of my impacts varies over time, one hypothesis could be that initially beneficiaries viewed the cash transfer as transitory and boosted savings as a precautionary measure, but over time considered it permanent and thus reduced savings.

To investigate how perceptions of transfer duration may have acted as a mechanism for savings behaviour, we utilize a question asked at follow-up waves: *"For how long in the future do you expect to continue receiving the money?"* At 24 months, 15 per cent of respondents believed they would receive the money for a relatively short time span (up to two years), compared to 27 per cent at 36 months and 37 per cent at 48 months. This is consistent with the eligibility requirement of having a child under the age of five as, over time, households would be more likely to have children 'age out' of the programme and the cash would increasingly appear to be transitory.

At 24 months, the majority of beneficiaries believed they would receive the transfer over the long term: 80 per cent of panel households in the treatment group expected to receive the transfer for five years or longer; compared to 61.7 and 32.3 per cent at 36 and 48 months respectively. The effect sizes we report here are thus the reverse of expectations if cash savings were being driven purely by the permanent income hypothesis: at 24 months we record the largest positive impact in terms of savings and this is also the year when the vast majority of treated households report expecting to receive the transfer over the long-term.[85]

Further, after 48 months of programme implementation, a rather substantial proportion of households did graduate out of the programme as the index child turned five. Twenty-five per cent of households in the treatment group report they are no longer receiving the transfer at 48

[85] To further test this hypothesis, we check whether households who had an older target child at baseline (24 months or older as opposed to 0 to 23 months), and therefore would expect to stop receiving the transfer shortly, are reporting higher savings rates; heterogeneous analysis, consistent with our hypothesis, indicates no differential impact by the age of the target child at baseline.

months. Therefore, whereas up to the 36-month wave, the ITT estimated should be relatively close to the average treatment effect (ATE), at the 48-month wave this is no longer the case. The positive and significant impacts of the programme after 48 months strengthen the hypothesis that the results for women's cash savings are not fading out and provide preliminary indications of the sustainability of the programme.

3.7 Women's cash savings, empowerment and productive investments

We have found large positive impacts both on the proportion of women holding cash savings and the amount saved in the last month. Questions remain as to the potential pathways through which the CGP may have led to such a large increase, particularly because UCTs typically do not have savings as the primary objective, but also as to some of the potential consequences of cash savings on these poor women's lives. We discuss two potential feedback effects.

First, we discuss potential feedback effects between savings and participation in non-farm enterprises. If the programme impacts on women's productive activities, then these investments could represent a potential mechanism to increase income generation, and therefore savings, but could also be a consequence of women's savings.

Second, we discuss whether the programme led to an increase in women's decision-making power. The impact on women's cash savings represents evidence of an increase of women's financial standing in the household; at the same time, a shift in intra-household preferences could have facilitated women's savings. These impacts could be reinforcing each other.

3.7.1 The virtuous cycle between women's productive investments and savings

Descriptive statistics on savings motivation provide some indication that women in the programme tend to save more for investment purposes at 36 months. In this Section, we focus on productive investments which have, in theory, the potential to improve the well-being of

programme households in the long term and thus result in a sustainable pathway out of poverty. In particular, in the 24-, 36- and 48-month surveys a NFE module was implemented which asked the main respondent whether the household had operated any NFEs or provided any small business services (e.g. retail or buy-trade store, transport, home brewing, trade, and so forth) over the last 12 months. Note that as baseline information on NFE was not collected, impacts on these indicators are cross-sectional (unadjusted and adjusted) and rely on the randomized design of the evaluation to estimate causal impacts.[86]

Table 3.7: Impact of the Child Grant Program on household Non-Farm Enterprise Operations at 24, 36 and 48 months

	24 months		36 months		48 months	
	(1) Unadjusted	(2) Adjusted	(3) Unadjusted	(4) Adjusted	(5) Unadjusted	(6) Adjusted
Treated	0.165***	0.162***	0.140***	0.137***	0.178***	0.173***
	(0.0408)	(0.0405)	(0.0325)	(0.0315)	(0.0352)	(0.0331)
N	1,983	1,983	1,983	1,983	1,983	1,983
R-squared	0.080	0.095	0.173	0.192	0.101	0.129
Control	0.293		0.309		0.22	

Notes: Estimations use single difference modelling. Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Unadjusted specifications include district dummies as this was the stratifying indicator for the randomization. Adjusted estimations include: woman's age, education and marital status, household size and household demographic composition, and districts.

As shown in Table 3.7, the programme has led to an increase in the proportion of households operating NFEs. Programme households are 16 percentage points more likely to operate a small business after 24 months in comparison to control households (Table 3.7, columns 1 and 2), 14 percentage points more likely after 36 months (Table 3.7, columns 3 and 4), and 17 percentage points more likely after 48 months (Table 3.7, columns 5 and 6); at 48 months 40 per cent of households operate an NFE compared to roughly half in the control group (22 per cent). As with savings impacts, the point estimates are very similar between unadjusted and adjusted models. The vast majority of households in my sample are rural and engaged in agriculture,

[86] To provide some confidence in cross-sectional estimates, we replicate estimates in Table 3.5 for any cash savings using single-difference models. We find results are very similar to DD impacts. For example, at 24 months the DD estimate is 23.3 percentage points versus 25.3 percentage points from the cross-sectional estimator; at 36 months the point estimates are 10.3 percentage points versus 11.9 respectively; finally, at 48 months the DD estimate is 14.8 percentage points versus 16.7 percentage points.

however the CGP appears to enable households to expand their income-generating activities. In line with some existing evidence on the impact of cash transfers on entrepreneurship, my findings suggest that liquidity constraints, and therefore the lack of start-up capital limits the operation of NFEs by poor households.[87] Additionally, further analysis shows the CGP had a positive impact on total revenue and profits from NFEs (see Table B.9 in the Appendix).[88]

This impact on non-farm enterprises is particularly interesting given the three main types of businesses reported in my sample, representing about 70 per cent of all reported ones, are petty trade, fish-selling and home brewery, activities which are typically operated by women in rural Africa (Nagler and Naude, 2014; Ackah, 2013; Canagarajah et al., 2001; Rijkers and Costa, 2012).

Although we have no specific information in the data on whether these non-farm enterprises are owned or run by a woman or man,[89] we can check household members' engagement in these activities. We define involvement as spending at least one day per week working in the business in an average month of operation. Table B.10 in the Appendix shows that households are far more likely to have women engaged in NFEs than men – 83 per cent of households report a woman engaged in an NFE compared to 57 per cent who report a man across the three survey waves. Across the three most common types of activity, the largest gender differences are in petty trade and home brewery.[90]

Time-use patterns among men and women in programme households collected as part of the same module therefore indicate that NFEs are primarily operated by women.[91] Moreover, my

[87] Some evidence refers to cash targeted directly to small business owners: De Mel et al. (2008; 2009); Fafchamps et al. (2014); McKenzie and Woodruff (2008); other evidence to high potential entrepreneurs: Fafchamps and Quinn (2015); Blattman et al. (2016) report the impact of a cash transfer provided together with a business skills training element and targeted to ultra-poor women. Haushofer and Shapiro (2016) find no significant impact.

[88] Descriptively, there is no significant difference in terms of number of months the main NFE was in operation in the last 12 months (around 6-7 months). However, the total revenue in an average month from businesses of households in the programme are significantly higher than those of control households (more than triple after 24 months and almost double after 36 months), as are profits (around a third higher after three years). These statistics should however be interpreted carefully, as more households in the treatment group operate NFEs and therefore the sample may no longer be balanced.

[89] Or when exactly the NFE was set up.

[90] In home breweries, for example, 92 per cent of households report a woman engaged in this activity and only 38 per cent report a man engaged in this activity. The last two columns of Table B10 report average days in the reference week and tell the same story – overall, women spend more days at an NFE than men, particularly in petty trade and home brewery.

[91] We also find that more women are now participating in business activities in the treatment group relative to the control group.

findings from Table 3.7 hold even when we restrict the focus to petty trade and home brewery businesses only; the impacts of the programme on these non-farm enterprises primarily operated by women, are still significant (not shown). The evidence from both overall household impacts, and additional time use patterns point to strong impacts of the programme on female-operated NFEs.

Taken together, these results suggest that the CGP facilitated women's involvement in NFEs through provision of start-up capital. Because the transfer size is relatively low on a bi-monthly basis, one hypothesis is that women were able to save a small amount of funds each month until they could start an NFE. Rather than enabling the household more generally to invest in enterprises by contributing to the household pool of resources, the CGP enabled women to save in cash and to start their own businesses. These findings overall suggest a positive impact of the programme on women's financial and economic empowerment. This also resonates with the fact that in almost 90 per cent of beneficiary households it is primarily the woman who picks up the grant that decides how the CGP is used.

Although we cannot fully disentangle whether or not savings contribute to the start-up costs of NFEs, or if profits from NFEs then contribute to savings, these dyanmics could contribute to a virtuous cycle for women and their households.

3.7.2 Shifts in intra-household preferences

Impacts on women's savings could be driven in part by a change in intra-household dynamics, specifically through an increase in women's control over resources within the household and thus ability to exert her savings versus consumption preferences. Evaluations, particularly from CCTs in Latin America, have suggested that it is possible for transfers to increase women's bargaining power or raise proxy measures of women's status within the household, however a recent review indicates mixed findings (van den Bold et al., 2013).

In a paper by Bonilla et al. (2017), the impact of the CGP on intra-household decision-making is investigated using a mixed method approach. Quantitative findings indicate that women in treatment households made more sole and joint decisions in five out of the nine domains analyzed, as well as a composite score of decision-making.[92] For this analysis, we are particularly interested in the decision-making indicator that captures the control over income; indeed, we expect that an increase in women's control over resources could potentially influence their cash savings. Results from Bonilla et al. (2017) indicate the CGP had an impact on the probability of women making sole or joint decisions related to their own income by almost 4 percentage points (a 6 per cent increase over baseline control means) and by 6 percentage points as regards her partner's income.

The qualitative research presented in Bonilla et al. (2017) suggests that when women in these households are asked directly about how they themselves would define or describe empowerment within their own communities, financial resources to spend how they wish are the consistent focus of conceptualizations. In the eyes of the beneficiaries themselves, the cash transfer allows choices, in terms of both consumption and investment, which makes them feel empowered. The impact on women's savings then represents, in itself, evidence of an increase of women's standing in the household. With the caveat that women's empowerment has multiple definitions and interpretations, it seems plausible that the CGP facilitated impacts on savings, in part due to women's increased joint decision-making control over income in the household; at the same time, the shift in intra-household dynamics could be a result of the increase in women's cash savings.

[92] Impacts were found for decisions related to: (1) children's schooling, (2) own income, (3) partner's income, (4) children's clothes or shoes, and (5) family visits.

3.8. Non-financial savings constraints

Literature on savings in low-income settings has suggested that apart from income poverty, a range of other social, psychological and organizational obstacles constrain people from saving as much as they would like. Social network pressures are one such barrier, where individuals and households find it difficult to say no to requests and claims on resources from family members or neighbours (Baland et al., 2011; Karlan et al., 2014; Hulme and Arun, 2009; Platteau, 2000; Ashraf, 2009). Further, the lack of safe and secure places to store savings, a consequence of thin financial markets, make it harder to fend off these claims. Dupas and Robinson (2013) show that even something as simple as a lock box with a key can significantly increase savings rates among self-employed individuals in rural Kenya.

Present bias and time inconsistency can also impede efforts to save, particularly for resource constrained individuals who tend to be myopic and prefer present rather than future payoffs (Hulme and Arun, 2009; Duflo et al., 2006; Laibson, 1997; Gul and Pesendorfer, 2001; Gul and Pesendorfer, 2004). Thaler and Benartzi (2004) in the United States and Ashraf et al. (2006) in the Philippines show that commitment devices − such as formal account agreements restricting access to savings or commitment to allocate part of future salaries — can promote higher savings.

In this Section, we exploit additional information available in the survey instrument to understand whether these non-financial constraints are operating in my population and how the CGP might have been able to further encourage savings. We look at the possibility of social network pressures descriptively through a set of questions in the operations module of the survey which ask beneficiaries if they had ever been approached for money by members of the community, or asked to take care of any relatives or friends as a result of receiving the cash transfer. Across the first two follow-up survey rounds (24 and 36 months), less than five per cent of recipients reported ever being asked for money, and around one per cent reported taking on care for new household members (question asked at 36 and 48 months only). Thus, we can

conclude that it is unlikely that social network pressures posed a large barrier to saving in my sample.

To measure present bias, in each wave a set of six questions involving an inter-temporal choice task (without incentives) was administered as follows: *"Suppose you suddenly win the Lotto. If you could choose between these two options which would you choose?"* Thereafter a series of choices was offered from *"ZMW 200 today"* to "*ZMW X in one month?"* where X varied, increasing from 200 up to a maximum of 800. This inter-temporal choice task captures all factors that would lead one to prefer money today over money tomorrow − what Frederick et al. (2002) refer to as time-discounting − rather than just an innate time preference trait. Using the baseline data, we code women as 'impatient' if they would never wait one month for any value of future money and use this as a measure of present bias. We estimate triple difference models to see if the CGP had differential effects on savings depending on whether or not the woman was impatient at baseline (see Table B.11 in the Appendix).

Results indicate that the average differential effect of being impatient at baseline on savings at 36 months is higher by 10 percentage points, an effect that is statistically significant at the ten per cent level, whereas no statistically significant heterogeneous impacts are present either at 24 months or at 48 months. In effect, impatient women at baseline start at a low rate of cash savings and 'catch up' with other (non-impatient) women. These results at 24 months suggest that the CGP might have shifted time preferences, loosening the present bias pressure comparatively more for those needing money immediately and allowed them to increase their capacity to save. Here the likely mechanism is the alleviation of short-term liquidity constraints which allows this group to think more about the future, as has been found in other settings including Kenya (Handa et al., 2016e).

However, results are only marginally significant and only at 36 months and it is difficult to draw firm conclusions from these results. If need differs across women in our sample, results may not capture time inconsistency but rather reflect women's poverty. Women with higher economic scarcity would therefore experience a stronger present bias: these women may choose

to receive a smaller amount of cash now rather than waiting to receive a larger sum later in the future. The response of women in these circumstances may therefore be the result of tighter liquidity constraints rather than time-inconsistent preferences.

Finally, in each survey round we asked those who reported positive savings where they kept their money. Less than ten per cent of savers kept their money in a place other than their house, the most common places being an informal savings scheme (such as a rotating savings and credit association-ROSCA) or a bank or post office. Given that the sample size for women savers who kept their money in a safe place is extremely small, we cannot compare the treatment effect on savings among women who at baseline had a safe place to keep their money versus those who kept their money at home, as we have low power to detect statistical significance.

3.9. Discussion and conclusion

This Chapter provides evidence on the impact of the Government of Zambia's CGP, a UCT programme targeted to households with children under the age of five, on women's saving outcomes. Primary results show that the CGP has enabled poor women to save in cash even in the absence of inclusive financial systems. The absence of safe savings mechanisms is acute in the remote districts of rural Zambia, and 93 per cent of women who save within the sample keep their money at home. Moreover, we find that the impact of cash savings has not come at the cost of other traditional forms of savings as measured by livestock holdings. In addition, we show that results are robust across different specifications; they are not simply a result of having more cash on hand due to recent payments. Finally, impacts do not seem to be fading away over time.

The CGP had no explicit gender objective and was not designed to specifically impact savings, nor to encourage entreprenuership or 'empower' women. Still, by providing money to poor women with little access to cash to start with, the programme enabled them to save in cash, to invest in productive activities and improve their standing in the household. These impacts can make a difference in beneficiaries' lives. The evidence suggesting that unconditonal cash given directly to women is utilized beyond expenditure on short-term needs, and that it can potentially

affect all these outcomes, is novel.

In a recent paper, Dupas et al. (2018) investigate the impact of offering a free bank account and assistance on savings in the poorest areas of three countries (Malawi, Uganda and Chile). They find that even among those poor individuals who open a free account, the use is very low. The main reason seems to be that people are simply too poor; namely, once these poor individuals cover subsistence expenditures, they do not have enough money left to save.

This finding resonates well with mine, which highlights that the main barrier to women's cash saving is actually a financial one. Women are extremely poor, have little access to cash and several constraints including food and/or child expenditures at baseline. The cash enables them to overcome their liquidity constraints and to start saving. Results further indicate that women value liquidity consistently with some of the advantages of cash savings; for example, in the face of a negative shock, it is easier to spend cash than to liquidate bulky assets.[93]

The large impact of the programme on women's cash savings also has a broader interpretation, as cash availability could proxy for women's financial empowerment. Qualitative research from the same programme (Bonilla et al. 2017) also suggests that women relate empowerment within their own communities to the availability of financial resources to spend how they wish. This 'empowerment' finding is particularly relevant among women who seem to have little control over resources at baseline.

We discuss two potential feedback effects; in particular, the impact of the programme on women's intra-household decision-making (Bonilla et al., 2017) related to control over resources (income) and the strong effects of the CGP on women's management, revenue and profits related to non-farm enterprises could facilitate women's cash savings while at the same time being further reinforced by savings. The link between increases in cash savings and the increase in non-farm

[93] Quotes from transcripts of in-depth interviews conducted among women in treatment and control communities as part of the impact evaluation of the CGP provide evidence of this: "*It is me that saves money and he doesn't even know about it. If it is my money I save myself and use it without telling him. When it is his he keeps and I won't see it. When I force him through his relatives he gives me, even ZMW 12 if it is school fees. If I want money for food he doesn't give me cash . . .. My savings are very important because it helps solve problems like sickness. Hospitals don't have medicine here, mostly we are told to buy. So I can use that money to buy medicine. I can also use the same money to buy food*." Quote from married female beneficiary recorded in the qualitative interviews. For further details see Bonilla et al. (2017).

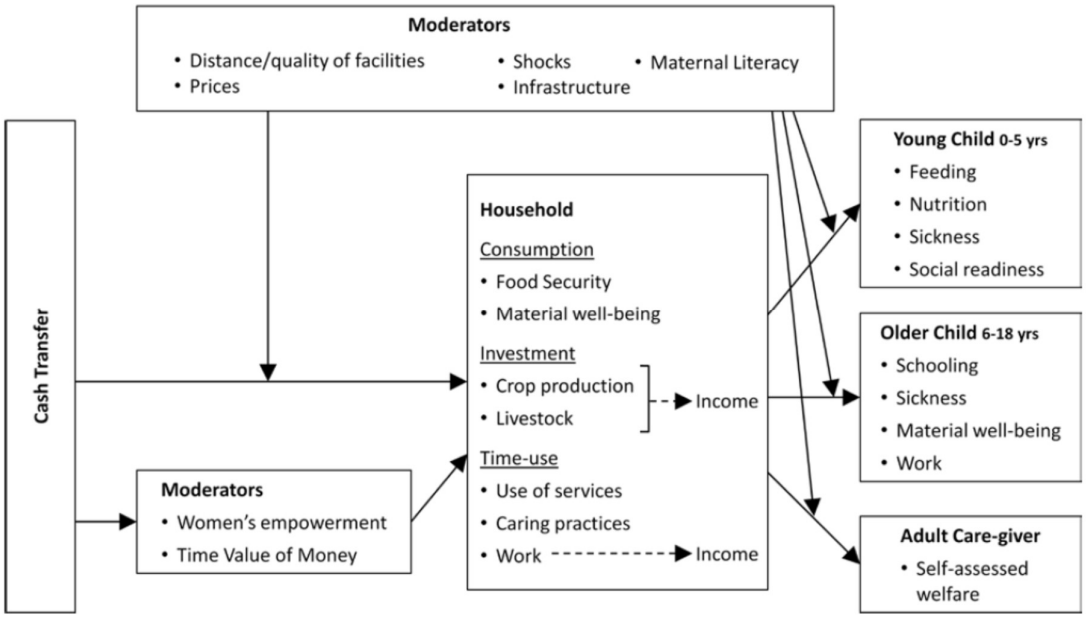
enterprises, which are primarily operated by women, offers a compelling story of the potential for UCTs directed to women to foster self-employment and their economic empowerment. Although we cannot disentangle whether or not savings contribute to the start-up costs of non-farm enterprises, or profits from non-farm enterprises then contribute to savings, these dyanmics could potentially contribute to a virtuous cycle for women and their households. We also provide some evidence dismissing some non-financial constraints to savings within the sample, including social network pressures to share resources.

There are a number of limitations to this analysis. First, we acknowledge the fact that since savings and saving amounts are self-reported, there may be an incentive for respondents to under-report their savings, particularly if they are kept at home. However, this does not threaten the internal validity of the study, since we have no reason to suspect that control and treatment households would under-report savings at different rates. Also, the study sample is made up primarily of households with young children, and women who are targeted may not be comparable to the average poor population of women. Finally, we cannot provide a direct comparison between cash savings under the control of men and women, or compare overall cash savings at the household level with those reported at the individual level, as the data for these measures was not collected.

Overall, the findings suggest that UCTs can increase savings and investment and contribute to women's financial and economic empowerment. Thus, UCTs can be considered along with other financial instruments, as a way to promote savings, without an explicit savings objective, and notably, to do so in a manner which, when targeted to women, specifically raise women's financial and economic empowerment. These findings are particularly notable, because they occur in a government-run large-scale programme, which was already reaching 20,000 beneficiary households in 2010. UCTs could result in long-term sustainable improvements in well-being and provide a pathway out of poverty for low-income rural households.

Appendix B

Figure B.1: Conceptual framework for impact evaluation of Child Grant Programme



Source: AIR (2011).

Figure B.2: Map of Zambia highlighting three intervention districts and Lusaka

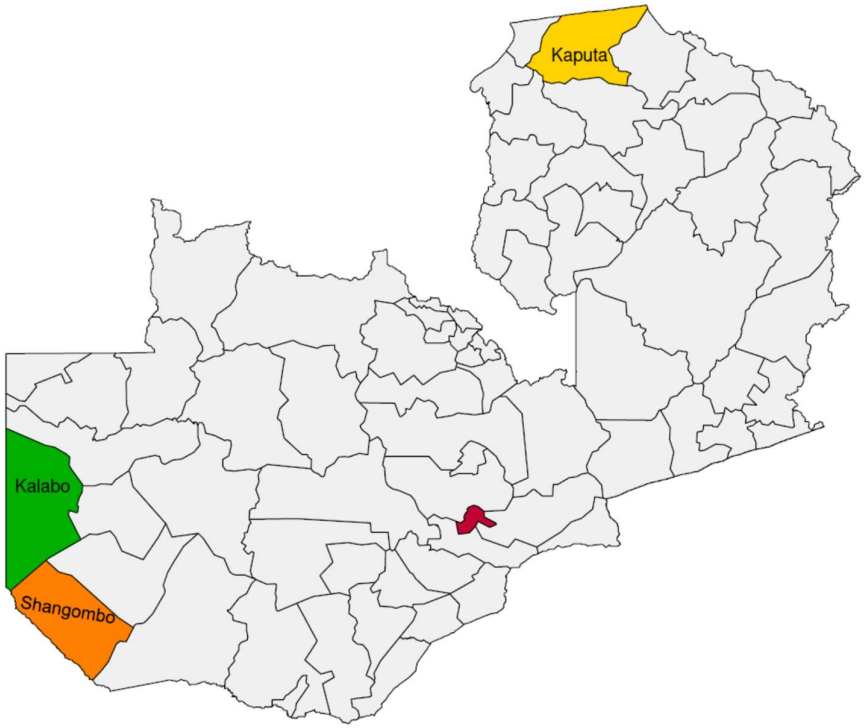


Figure B.3: Study flow chart for impact evaluation of Child Grant Programme

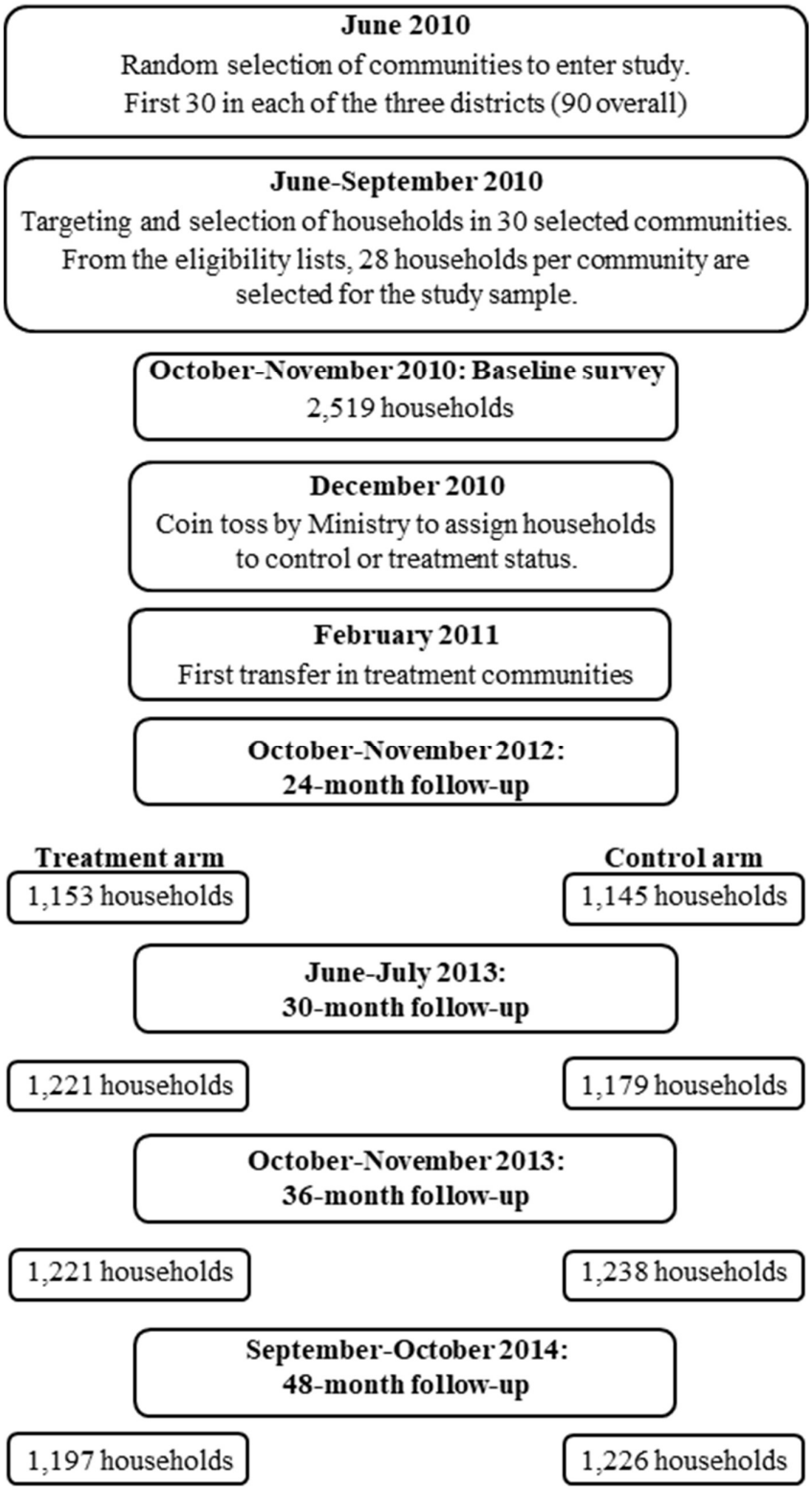


Table B.1: Number of households interviewed by wave and household overall attrition

	Households	Household overall attrition
Baseline	2,519	
24-month follow-up	2,298	9%
36-month follow-up	2,459	2%
48-month follow-up	2,423	4%
Total	9,699	

Table B.2: Individual level attrition rate by treatment arm, based on the balanced panel of women

	Overall	Control	Treatment	P-value of diff.
Attrition rate	20.4	19.8	21.0	0.66

Notes: Sample of women at baseline: 2,492. T-tests based on standard errors clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1

Table B.3: Testing individual differential attrition (from baseline to endline) by baseline characteristics

	Control			*Treatment*			*Difference*	
	Attritors	Non-attritors	P-value	Attritors	Non-attritors	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age (years)	31.39	29.21	0.01	31.39	29.48	0.01	-0.00	0.99
Ever attended school	0.74	0.70	0.24	0.75	0.73	0.68	-0.01	0.84
Never married	0.09	0.11	0.47	0.10	0.11	0.70	-0.01	0.69
Divorced or separated	0.13	0.11	0.36	0.10	0.08	0.42	0.03	0.25
Widowed	0.08	0.06	0.41	0.13	0.05	0.00	-0.05	0.11
Shangombo district	0.19	0.37	0.00	0.21	0.37	0.00	-0.02	0.81
Kaputa district	0.51	0.29	0.00	0.53	0.28	0.00	-0.02	0.87
Consumption expenditure per capita (ZMW)	39.65	39.56	0.97	42.80	41.08	0.42	-3.16	0.37
Household size	5.71	5.61	0.58	5.90	5.71	0.26	-0.19	0.48
Number of members aged 0-5	1.96	1.91	0.38	1.84	1.90	0.37	0.12	0.14
Number of members aged 6-12	1.23	1.27	0.74	1.31	1.25	0.46	-0.08	0.56
Number of members aged 13-18	0.51	0.52	0.81	0.69	0.58	0.08	-0.18	0.03
Number of members aged 19-35	1.27	1.30	0.59	1.29	1.37	0.22	-0.03	0.75
Number of members aged 36-55	0.59	0.52	0.23	0.59	0.53	0.31	-0.00	0.99
Number of members aged 56-69	0.12	0.06	0.02	0.12	0.06	0.02	-0.00	0.98
Number of members aged 70+	0.03	0.03	0.74	0.05	0.03	0.12	-0.02	0.24
Proportion of women holding any savings	0.18	0.16	0.54	0.20	0.18	0.45	-0.03	0.55
Amount saved (ZMW)	12.91	14.21	0.77	19.77	12.70	0.13	-6.86	0.24
Amount saved (logged)	0.66	0.59	0.55	0.79	0.66	0.24	-0.13	0.46
Household owned any milk cows (last 12 months)	0.03	0.07	0.04	0.03	0.05	0.29	-0.00	0.92
Household owned any cattle (last 12 months)	0.05	0.11	0.02	0.07	0.10	0.17	-0.02	0.44
Household owned any goats (last 12 months)	0.02	0.01	0.36	0.05	0.03	0.13	-0.03	0.14
Household owned any chicken (last 12 months)	0.36	0.44	0.01	0.40	0.44	0.15	-0.04	0.38
Household owned any ducks (last 12 months)	0.04	0.04	0.66	0.03	0.02	0.35	0.01	0.73
Household owned any livestock (last 12 months)	0.41	0.51	0.00	0.45	0.51	0.05	-0.04	0.38
Impatient	0.26	0.22	0.22	0.18	0.19	0.81	0.08	0.09
N	247	999		262	984			

Notes: Overall N for control is 1,246. Overall N for treated is 1,246. *** p<0.01, ** p<0.05, *** p<0.1. T-tests based on standard errors clustered at the community level.

Table B.4: Self-reported reasons for women's cash saving by treatment status at 36 and 48 months

	36-month follow-up				48-month follow-up			
	All	Control	Treatment	P-value of diff. (C-T)	All	Control	Treatment	P-value of diff. (C-T)
(1) To purchase bulk or other food	0.58	0.60	0.57	0.62	0.47	0.51	0.45	0.28
(2) To purchase household consumables	0.43	0.47	0.41	0.45	0.35	0.33	0.37	0.50
(3) School fees/expenses	0.30	0.27	0.32	0.39	0.34	0.35	0.34	0.91
(4) To buy new clothing	0.22	0.23	0.21	0.64	0.20	0.15	0.22	0.07
(5) Medical expenses/Health care	0.41	0.41	0.41	0.96	0.34	0.32	0.35	0.70
(6) To repay debts	0.01	0.00	0.02	0.22	0.02	0.01	0.03	0.24
(7) To purchase household durable	0.07	0.07	0.08	0.89	0.07	0.06	0.08	0.29
(8) To purchase livestock	0.04	0.02	0.05	0.18	0.07	0.06	0.07	0.71
(9) To purchase agricultural inputs or	0.07	0.09	0.06	0.32	0.12	0.17	0.10	0.11
(10) To purchase assets to start a new	0.10	0.10	0.10	0.90	0.15	0.15	0.16	0.76
(11) To make home improvements	0.04	0.03	0.06	0.22	0.07	0.08	0.07	0.76
(12) To purchase new land or house	0.01	0.01	0.01	0.53	0.01	0.02	0.01	0.48
(13) To spend on services	0.00	0.00	0.00	0.31	0.00	0.00	0.00	
(14) Other	0.01	0.00	0.01	0.59	0.02	0.01	0.03	0.31

Notes: N is 568 and 551 panel women who report any cash savings at 36 and 48 months respectively. Differences between treatment and control *** p<0.01, ** p<0.05, * p<0.1. P-values are reported from Wald tests on the equality of means of Treatment and Control for each variable. Standard errors are clustered at the community level.

Table B.5: Impact of the Child Grant Programme on women's cash savings at 24, 36 and 48 months, including individual-level fixed-effects

	(1) Any cash savings	(2) Amount saved (last month, logged)
24-month follow-up	0.0621*	0.247**
	(0.0319)	(0.118)
36-month follow-up	0.0671**	0.355***
	(0.0332)	(0.127)
48-month follow-up	0.0360	0.193*
	(0.0286)	(0.104)
DD 24 months	0.233***	1.112***
	(0.0468)	(0.180)
DD 36 months	0.103**	0.553***
	(0.0449)	(0.180)
DD 48 months	0.148***	0.787***
	(0.0448)	(0.184)
N	7,932	7,932
R-squared	0.043	0.052
Baseline control mean	0.16	0.59

Notes: Estimations use individual level fixed-effects. Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.6: Impact of the Child Grant Programme on women's cash savings at 24, 36, and 48 months, ANCOVA estimates

	Any cash savings		Amount saved (last month, logged)	
	(1) Unadjusted	(2) Adjusted	(3) Unadjusted	(4) Adjusted
Impact at 24 months	0.255***	0.252***	1.174***	1.156***
	(0.0328)	(0.0330)	(0.136)	(0.138)
R-squared	0.089	0.097	0.102	0.111
N	1,983	1,983	1,983	1,983
Baseline control mean	0.16	0.16	0.59	0.59
Impact at 36 months	0.125***	0.118***	0.616***	0.583***
	(0.0348)	(0.0346)	(0.151)	(0.150)
R-squared	0.043	0.059	0.051	0.068
N	1,983	1,983	1,983	1,983
Baseline control mean	0.16	0.16	0.59	0.59
Impact at 48 months	0.171***	0.167***	0.851***	0.825***
	(0.0292)	(0.0291)	(0.148)	(0.147)
R-squared	0.076	0.083	0.094	0.102
N	1,983	1,983	1,983	1,983
Baseline control mean	0.16	0.16	0.59	0.59

Notes: Estimations use ANCOVA models. Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Estimations with adjustment include woman's age, education and marital status, household size and household demographic composition, and districts − see text for details. Both adjusted and unadjusted models include the baseline value of the outcome variable and district indicators.

Table B.7: Impact of the Child Grant Programme on women's cash savings at 24, 36 and 48 months, marginal effects from probit estimates

	Any cash savings	
	(1) Unadjusted	(2) Adjusted
24-month follow-up	0.0758*	0.0695*
	(0.0413)	(0.0415)
36-month follow-up	0.0844*	0.0764*
	(0.0438)	(0.0443)
48-month follow-up	0.0438	0.0342
	(0.0385)	(0.0390)
Treated	0.0297	0.0226
	(0.0416)	(0.0412)
DD 24 months	0.230***	0.233***
	(0.0627)	(0.0633)
DD 36 months	0.0959*	0.0967*
	(0.0578)	(0.0581)
DD 48 months	0.155**	0.156**
	(0.0604)	(0.0608)
N	7,932	7,932
Pseudo R-squared	0.0608	0.068
Baseline control mean	0.16	0.16

Notes: Marginal effects from probit model. Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Unadjusted specifications include district dummies as this was the stratifying indicator for the randomization. Estimations with adjustment include woman's age, education and marital status, household size and household demographic composition, and districts − see text for details.

Table B.8: Impact of the Child Grant Programme on women's cash savings at 24, 36 and 48 months, probit ANCOVA marginal effects

	Any cash savings	
	(1)	(2)
	Unadjusted	Adjusted
Impact at 24 months	0.259***	0.258***
	(0.0332)	(0.0333)
N	1,983	1,983
Pseudo R-squared	0.0713	0.079
Baseline control mean	0.16	0.16
Impact at 36 months	0.123***	0.117***
	(0.0350)	(0.0351)
N	1,983	1,983
Pseudo R-squared	0.0355	0.05
Baseline control mean	0.16	0.16
Impact at 48 months	0.176***	0.171***
	(0.0295)	(0.0295)
N	1,983	1,983
Pseudo R-squared	0.0683	0.0747
Baseline control mean	0.16	0.16

Notes: Estimations use ANCOVA models. Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Estimations with adjustment include woman's age, education and marital status, household size and household demographic composition, and districts − see text for details. Both adjusted and unadjusted models include the baseline value of the outcome variable and district indicators.

Table B.9: Impact of the Child Grant Programme on household non-farm enterprise revenues and profits at 24, 36 and 48 months

	24 months		36 months		48 months	
	(1) Revenues (logged)	(2) Profits (logged)	(3) Revenues (logged)	(4) Profits (logged)	(5) Revenues (logged)	(6) Profits (logged)
Treated	1.096***	0.901***	0.775***	0.654***	1.024***	0.885***
	(0.238)	(0.208)	(0.173)	(0.151)	(0.216)	(0.193)
N	1,983	1,983	1,983	1,983	1,980	1,980
R-squared	0.119	0.119	0.181	0.176	0.145	0.146
Control mean	1.43	1.25	1.57	1.39	1.18	1.04

Notes: Estimations use single difference modelling. Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. These are adjusted estimations including: woman's age, education and marital status, household size and household demographic composition, and districts. Unadjusted specifications are consistent.

Table B.10: Proportion of female respondents and 'comparable' males engaged in non-farm enterprises at 24, 36 and 48 months (%)

	Female respondents	Comparable males	Female respondents	Comparable males
	Proportion engaged in NFE		*Mean days worked*	
		Full Sample		
Any NFE	82.7	57.0	3.4	3.3
Fish selling	53.1	64.6	3.0	4.3
Petty trader	85.6	38.2	5.3	3.1
Home brewery	91.6	38.2	4.9	2.6
		Treatment		
Any NFE	83.7	58.2	3.6	3.1
Fish selling	57.2	64.9	3.3	3.7
Petty trader	86.4	41.3	5.3	2.6
Home brewery	92.5	40.9	5.1	2.5
		Control		
Any NFE	81.1	54.9	3.1	3.7
Fish selling	47.2	64.2	2.6	5.6
Petty trader	84.1	32.0	5.2	3.9
Home brewery	90.4	33.9	4.6	2.7

Notes: The table compares the participation in NFEs (intensive and extensive margins) for female respondents and a 'comparable' adult male, defined as the spouse or partner of the woman, and if there is no spouse/partner, her brother or next closest adult male relative. Approximately 78 per cent of households have a 'comparable' male so we restrict our comparisons to this smaller sample of households in order to offer a clean comparison. Means are averaged across the 24-, 36- and 48-month survey rounds.

Table B.11: Impact of the Child Grant Programme on women's cash savings and heterogeneous impacts by women's impatience at baseline

	Any cash savings		Any cash savings by women's impatience at	
	(1) Unadjusted	(2) Adjusted	(3) Unadjusted	(4) Adjusted
24-month follow-up	0.0621*	0.0569*	0.0295	0.0243
	(0.0319)	(0.0321)	(0.0354)	(0.0354)
36-month follow-up	0.0671**	0.0595*	0.0551	0.0476
	(0.0332)	(0.0337)	(0.0380)	(0.0384)
48-month follow-up	0.0360	0.0269	0.0179	0.00874
	(0.0286)	(0.0293)	(0.0313)	(0.0319)
Treated	0.0241	0.0178	0.0232	0.0185
	(0.0321)	(0.0317)	(0.0363)	(0.0359)
DD 24 months	0.233***	0.233***	0.242***	0.242***
	(0.0468)	(0.0468)	(0.0520)	(0.0520)
DD 36 months	0.103**	0.103**	0.0843*	0.0845*
	(0.0449)	(0.0450)	(0.0485)	(0.0486)
DD 48 months	0.148***	0.148***	0.142***	0.142***
	(0.0448)	(0.0449)	(0.0471)	(0.0471)
Impatient at baseline			-0.105***	-[illegible]
			(0.0261)	(0.0263)
Impatient at baseline * DD 24 months			-0.0263	-0.0266
			(0.0587)	(0.0587)
Impatient at baseline * DD 36 months			0.104*	0.104*
			(0.0539)	(0.0539)
Impatient at baseline * DD 48 months			0.0452	0.0447
			(0.0667)	(0.0668)
Impatient at baseline * 24-month follow-up			0.149***	0.149***
			(0.0363)	(0.0363)
Impatient at baseline * 36-month follow-up			0.0545	0.0545
			(0.0422)	(0.0422)
Impatient at baseline * 48-month follow-up			0.0825*	0.0830*
			(0.0436)	(0.0439)
Impatient at baseline * Treated			-0.0103	-0.0176
			(0.0392)	(0.0387)
N	7,932	7,932	7,932	7,932
R-squared	0.072	0.079	0.075	0.082

Notes: Estimations use difference-in-difference modelling (DD indicates treatment effect). Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Unadjusted specifications include district dummies as this was the stratifying indicator for the randomization. These are adjusted estimations including: woman's age, education and marital status, household size and household demographic composition, and districts.

4 The Impact of Zambia's Unconditional Child Grant on Schooling and Work: Results from a Large-Scale Social Experiment[94]

4.1 Introduction

Cash-based assistance has the potential to influence schooling outcomes. Unconditional cash transfers – that provide cash with no strings attached − can have an 'income effect' on the demand for schooling: by raising incomes they help poor households to overcome liquidity constraints and send their children to school. Cash transfers conditional on schooling can also have a 'substitution effect' as they lower the opportunity cost (or price) of schooling (Baird et al., 2014).

There is a substantial evidence base on the impacts on schooling of conditional cash transfers (CCTs), mainly in Latin America. There have been several published studies on the impact of CCTs on schooling (Schultz, 2004; Attanasio et al., 2010; Sadoulet et al., 2004; Dammert, 2009; Paxson and Schady, 2007), all showing different magnitudes of positive impacts. A review by Fizbein and Schady (2009) suggests that CCTs for schooling are effective in raising school enrolment and attendance, and in middle income countries where primary school enrolment rates are already high CCT impacts have been more significant at the secondary school level.

[94] This chapter is an adapted and developed version of the following joint work: Handa, S., Natali, L., Seidenfeld, D., Tembo, G. and the Zambia Cash Transfer Evaluation Team (2016). The Impact of Zambia's Unconditional Child Grant on Schooling and Work: Results from a large-scale social experiment, *J. Dev. Effectiveness* 8(3): 346-367.

Schooling impacts from conditional cash transfers – where programme participation is conditional on education-related behaviour – are almost expected by design, provided the programmes are implemented well and benefit levels are set high enough. Also, although beneficiary households can decide how to spend money, the conditionality related to education behaviour provides a push for households to spend on schooling (Davis et al., 2016).

Evidence on the impacts on education outcomes of unconditional cash transfers which are not directly linked to school attendance are less straightforward as conditions to facilitate human capital acquisition are not built into these programmes. Yet, a systematic review of the impacts of cash transfers on schooling outcomes (Baird et al. 2014) found that both conditional and unconditional cash transfers are effective in increasing school enrolment and attendance. Published evidence from unconditional cash transfers (UCTs) is however far less abundant although growing rapidly.

The paper published by the Kenya Cash Transfer for Orphans and Vulnerable Children Study Team (2012) was the first to use experimental data on the impact of an unconditional cash transfer run by the government − though a substantial grey literature does exist.[95] Handa et al. (2016)[96] and, more recently, Kilburn et al. (2017) examine the impacts of two large scale government-run programmes respectively in Zambia and Malawi and find positive impacts on schooling outcomes.

Cash transfers have also been found to influence child work. Indeed, a major barrier to school attendance is represented by the need for children to work and earn an income; cash transfers might be reducing child work by providing an alternative source of income. Indeed, a review of the impacts of cash transfers – including both conditional and unconditional – on child work (De Hoop and Rosati, 2014) report no evidence of detrimental impacts. Overall, instead, there is some indication of a reduction in participation and time spent working, which tends to be stronger for economic activities

[95] See for instance evaluation reports from Ghana, Zambia and Lesotho at www.cpc.unc\projects\transfer.
[96] The article appeared in the *Journal of Development Effectiveness* and is the published version of this chapter.

for boys and for chores for girls.

Overall, given the prevalence of studies on the impacts of CCTs on schooling and work, existing studies mainly focus on programmes that explicitly target households with children in school-age range and where the primary objectives of the programme are directly related to these children. Studies investigating how the cash transfer affects other children in these households, those not explicitly targeted and therefore not meant to be the primary beneficiaries of the programme, are rare.

This paper contributes to the limited but growing evidence on whether unconditional cash transfers shape schooling and child work in sub-Saharan Africa. We report on the impact on child school attendance and work engagement of the Government of Zambia's Child Grant Program (CGP), an unconditional cash transfer programme in three of the poorest and most remote districts of the country. The intriguing and even exciting feature of unconditional cash transfers is that, since money is not tied to behaviour, impacts may be found in any sphere, depending on what the major constraints are facing households, and how the household itself believes the cash can best address its needs. In this article we take advantage of the unconditional nature of the Zambian CGP, which targets families with very young children and whose objectives are focused on their health and development, to see if the programme has an impact on the schooling and work of school-age children who in principle are not the main target population.

We use data from a large-scale social experiment involving 2,519 households, half of whom were randomized out to a delayed-entry control group, that was implemented to assess the impact of the programme. Our findings indicate that the CGP had a significant impact on attendance (binary) among children aged 11-14 with point estimates in the range of 7 to 8 percentage points; this result is supported by a half day significant impact on regular attendance as measured by the number of days attended in the last week. This is the age range when sharp dropout begins to occur in Zambia. We provide evidence on the potential pathways through which the unconditional cash transfer impacts school attendance. Households in the CGP spend more on education, and in particular on uniforms,

key barriers to school enrolment and attendance in the study areas. For older children, a key barrier across the developing world is the time-cost of school attendance in terms of income foregone. The CGP has reduced work-for-pay among children aged 11−14 suggesting that alleviating the opportunity cost of schooling is another pathway through which the programme facilitates school attendance.

We characterize our contribution to the literature therefore as being one of the few articles that study the impact of a government-run unconditional programme on schooling and child work using experimental data in sub-Saharan Africa. However, a key difference of this programme is that the CGP's objectives are focused on very young children, and so schooling effects, particularly among older children, would be considered somewhat secondary level or 'spillover' effects facilitated in part by the unconditional nature of the programme.

The remainder of this paper is structured as follows. Section 4.2 provides a summary of the existing literature on the impacts of cash transfers on child schooling and work outcomes. Section 4.3 presents the main features of the Child Grant Programme in Zambia and reviews the evaluation design while Section 4.4 provides information on the education system in Zambia. Section 4.5 describes the survey data and discusses attrition and baseline balance. Section 4.6 reports the empirical strategy and Section 4.7 the main results. Section 4.8 analyzes the impact on schooling inputs, Section 4.9 reports robustness checks and finally Section 4.10 concludes.

4.2 Literature review

Conditional cash transfers have typically been found to increase child schooling (Attanasio et al., 2010; Behrman et al., 2005; de Janvry et al., 2006; Edmonds, 2006; Edmonds and Schady, 2012; Glewwe and Kassouf, 2012; Filmer and Schady, 2011; Fiszbein and Schady, 2009; Paxson and Schady, 2010; Sadoulet et al. 2004; Schultz, 2004; Skoufias and Parker, 2001; see García and Saavedra, 2017 for a meta-analysis of CCT studies on schooling outcomes). Schooling impacts from

conditional cash transfers – where programme participation is typically conditional on children attending school[97] – are somewhat straightforward provided the programmes are implemented well and benefit levels are set high enough.

However, published evidence from unconditional cash transfers is less abundant although growing in recent years. According to a 2014 systematic review of the impacts of cash transfers on schooling outcomes, there were 50 journal articles/working papers on CCTs overall compared to only five on UCTs in sub-Saharan Africa and three comparing CCTs versus UCTs (Baird et al., 2014).

Samson et al. (2010) investigate the impact of South Africa's Child Support Grant on primary school enrolment using a difference-in-differences (DD) propensity score matching and find a statistically significant impact of 7 percentage points. In an experimental study, Miller et al. (2010) report a weakly significant 4 percentage point increase in school enrolment based on the Mchinji Social Cash Transfer Scheme in Malawi.

The Kenya CT-OVC Study Team (2012) study is however the first published paper on the impact of a large unconditional cash transfer run by a government in sub-Saharan Africa using experimental data. After two years of implementation, the authors find no impact on enrolment among children aged 6 to 17 but a significant 7.8 percentage point impact for secondary school-age children (ages 13−17).

Hausofer and Shapiro (2016) use a randomized controlled trial (RCT) to investigate the impact of large unconditional transfers of money provided by the NGO GiveDirectly to poor households in rural Kenya. Restricting their analysis to households with school-age children, they find no impact on an educational index which combines enrolment and education expenditures. Whereas there is no discerned effect of the programme on school enrolment, there is however a significant impact on education expenditures.

[97] CCTs have also been conditioned on health related behaviour, usually visiting health care centres.

More recently, Kilburn et al. (2017) investigate the impact of Malawi's Social Cash Transfer Programme (SCTP) using data from a cluster-randomized control trial. Their findings indicate that children aged 6 to 17 years in beneficiary households – ultra-poor labour-constrained –are 12 percentage points more likely to be enrolled in school. Using causal mediation analysis, they show that the key mechanism for this impact is through an increase in education expenditures − and of notebooks and uniforms in particular.

Both conditional and unconditional cash transfers are effective at increasing school enrolment and attendance (Baird et al., 2014). A couple of studies have investigated the impact of cash transfers on schooling in Africa while at the same time comparing conditional versus unconditional cash transfers. The finding that CCTs are effective in improving children's outcomes is neither novel nor surprising, however these papers (Baird et al., 2011; Benhassine et al., 2015; Akresh et al., 2013) also highlight a significant impact of unconditional cash transfers.

Baird et al. (2011) discuss the results of cash transfers implemented by an NGO and targeted to households with girls in school in the Zomba district of Malawi.[98] They found that dropout rates declined in both the conditional and unconditional treatment arms, though more so in the former group.

Benhassine et al. (2015) investigate the effects of 'labeled' unconditional versus a conditional cash transfer in Morocco. The programme targets poor communities and provides small cash transfers to all households with primary school aged children (6−15 years). The experiment is set as a multiple treatment arm RCT to test conditional versus unconditional transfers.[99] The unconditional cash transfer was 'labeled' in the sense that it had an implicit education messaging in its school-based enrolment procedure. Over two years, the authors find a 7.4 percentage points impact on enrolment; the results for the UCT are even larger than those found in the CCT arm.

[98] The experiment also included girls out of school, however the paper discussed here focuses on enrolled girls only.
[99] The experimental setting also allows to compare the effect of transfers paid directly to fathers versus mothers.

Akresh et al. (2013) evaluate the relative effectiveness of conditional versus unconditional cash transfers over two years in rural Burkina Faso with a similar design to the one used by Benhassine and co-authors, with the sole difference that the unconditional cash transfer was not 'labeled'. The Nahouri Cash Transfers Pilot Project was targeted at households with children aged 7 to 15. Transfers – whether conditional or unconditional – were equally effective on children's educational outcomes.

The studies reviewed mainly focus on educational rather than work outcomes. However, a review of the impacts of cash transfers – including both conditional and unconditional – on child work (De Hoop and Rosati, 2014) reports no evidence of a detrimental impact. Overall, there is instead some indication of a reduction in participation and time spent working, which tends to be stronger for economic activities for boys and for chores for girls.

Although we have mainly focused on the impacts of unconditional cash transfers in this Section, the existing literature is mainly based on studies from Latin America, and therefore on conditional cash transfers. When cash is provided conditional on the children of beneficiary households attending school, results are somewhat less surprising. When programmes are conditioned on specific children's outcomes and households do not comply with the requirements set for receiving the cash grant, their access to benefits might be withdrawn. Also, although beneficiary households can decide how to spend money, the conditionality related to education behaviour provides an incentive to spend on schooling (Davis et al., 2016).

Finally, the studies reviewed mainly focus on programmes that explicitly target households with children in the school-age range[100] and the primary objectives of the programme are directly

[100] There are few exceptions; for instance, Malawi's Social Cash Transfer Programme (SCTP) focuses on ultra-poor labour-constrained households and GiveDirectly targets poor households (with a thatched roof).

related to these children.[101] Studies investigating how the cash transfer affects other children in these households, not explicitly targeted and therefore not meant to be the primary beneficiaries, are rare.

4.3 Intervention and the evaluation design

4.3.1 The Zambian Child Grant Programme

The Child Grant Programme (CGP) is nationally owned and implemented. The Zambian Government's Ministry of Community Development and Social Services (MCDSS)[102] initiated the CGP in 2010 as part of their piloting a series of alternative cash transfer designs in the country.[103] This categorical model aims at alleviating poverty and preventing its intergenerational transmission. The CGP provides unconditional cash transfers to any household with a child under 5 years old;[104] however, to ensure every beneficiary household receives the transfers for a minimum of two years, only households with children 36 months or below were enrolled in the scheme.

There are no explicit welfare criteria, apart from geographical targeting. The scheme is implemented in three districts selected based on their high poverty, infant mortality and child malnutrition rates: Kalabo and Shangombo in the Western Province and Kaputa in Northern Province, which had never received cash transfers before (see map, Figure C.1 in the Appendix). All three districts are near the Zambian border with either the *Democratic Republic of Congo* (Kaputa) or Angola (Shangombo and Kalabo) and require a minimum of two days of travel by car to reach from Lusaka. In addition, Shangombo and Kalabo are cut off from Lusaka by a flood plain that turns into a river in the rainy season. Hence, these three districts represent some of the most remote locations in

[101] For instance, Progresa provides transfers for children aged 8−17 conditional on being enrolled in school and with at least 85 per cent attendance; similarly, Bolsa Familia is targeted to children aged 6 to 15.
[102] The Ministry changed name to Ministry of Community Development, Mother and Child Health (MCDMCH) in 2013 and then most recently to the Ministry of Community Development and Social Services (MCDSS).
[103] The four different targeting models included the child grant, labour-constrained, multiple categorical and universal old-age pension models.
[104] The Child Grant Programme has a continuous enrolment system so that households are enrolled into the programme as soon as they have a newborn baby.

Zambia, and indeed part of the learning around the CGP is to test delivery and monitoring mechanisms for remote communities in anticipation of an eventual scaled-up programme.

The primary objectives of the programme are to address young child health and nutrition and household food security; secondary outcomes relate to asset building and economic strengthening. Recipient households receive a flat transfer – irrespective of household size – equivalent to 60 kwacha (ZMW) a month (equivalent to USD 12 or USD 24 purchasing power parity - PPP), an amount deemed sufficient to purchase one meal a day for everyone in the household for one month. The transfer is equivalent to 27 per cent of pre-programme household monthly consumption. There are no conditions to receive the money and payments are made every other month (bi-monthly) through a local pay-point manager. The designated recipient of the cash was the mother or primary caregiver of the target child. The programme was implemented successfully: all households typically receive the cash transfer on time and do not report unjust solicitations although there is some indication of misperceptions of conditions by the recipients many of whom believed that feeding and children's cleanliness were prerequisites to receive the grant (AIR 2013; 2014).

Initially, the scheme reached all eligible households in around 90 communities – half of which were also included in the evaluation – and covered 10,000 beneficiary households; over time, the Child Grant Programme continued to gradually expand coverage.

4.3.2 Evaluation design

The CGP impact evaluation is a longitudinal multisite cluster-randomized controlled trial (cRCT) with random assignment at the community level. The study is designed with random selection of

communities[105] (hereafter referred to also as clusters) to enter the study, random sampling of (eligible) households within communities and random assignment within districts at the community level.

In June 2010, a public lottery was held at the Ministry headquarters to randomly select (and order) 30 communities – out of roughly 100 within each district – to enter the study[106] (*random selection of communities*). Ministry staff from the three districts participated in the process, and this helped to create understanding and transparency in the selection process for those implementing the programme.[107]

The targeting was then conducted and, in the 90 communities identified in the random selection process – 30 in each district – all eligible households with at least one child under three years of age were identified. Among all eligible households in each community, 28 households (out of more than 100) were randomly sampled and included in the study (*random selection of households*).

To avoid anticipation effects, random assignment occurred only after the baseline data collection was complete. The unit of randomization is the community, so as to avoid contamination.[108] As the Ministry did not have sufficient resources or capacity to deliver the programme to all eligible households immediately, it instituted a policy of randomly assigning communities to current or delayed treatment, deeming it to be the most ethical and fair way to select the order in which communities receive the resources as they became available.[109]

---

[105] These administrative and geographic units in Zambia are usually referred to as CWACs. These Community Welfare Assistance Committees represent the authorities at the community level.

[106] The Ministry provided a list of all communities that could be feasibly be reached in the first year along with the potential number of household beneficiaries in each community. The identification number of each community was then written on separate slips of paper which were then placed altogether in a glass bowl (to make sure the slips were randomly ordered and situated in the glass bowl, each community was assigned a 5-digit number using a random generator to determine the order of placement).

[107] Communities were drawn from the bowl by Ministry staff from the districts and provinces where the programme would be implemented and ordered by how they were drawn from the glass bowl.

[108] The stable unit treatment value assumption (SUTVA), namely the assumption of no interference between households, would be violated if control households were identified within clusters.

[109] In practice, households in communities assigned to the delayed treatment condition were expected to start receiving the Child Grant after the evaluation had terminated. However, after piloting a series of alternative cash transfer designs, the Government of Zambia decided to implement a unified model nationally − the 'inclusive' or 'labour-constrained' model –

Within each of the three geographically targeted districts, communities were randomly assigned to either the treatment condition to start the programme early in 2011 or to the control (or delayed treatment) condition initially planned to start the programme at the end of 2013. In December 2010, the Ministry's Permanent Secretary flipped a coin to determine which half of the list of randomly selected (and ordered) communities would be in the treatment or the control condition (*random assignment of communities to treatment*). The randomization process was conducted in public with local officials, Ministry staff, and community members present as witnesses. Given the public nature of the randomization and the fact that this was a government programme, study participants could not be blinded at follow-up. The flowchart of the study design is reported in the Appendix[110] (see Figure C.2).

The impact evaluation was designed as longitudinal and included baseline and several follow-ups; further details are provided in Section 4.5. The final study sample size was just over 2,500 households. The sample size was determined through a power analysis to ensure that the study was able to detect meaningful effects on child anthropometry measures, which is the smallest subgroup of analysis in the sample (i.e. children under the age of five), accounting for non-response and attrition rates.

MCDSS and UNICEF Zambia commissioned the evaluation to the American Institutes for Research (AIR) and the University of North Carolina at Chapel Hill, while data collection was conducted by Palm Associates, a local independent private firm unaffiliated with the Ministry. The study underwent ethical review at the American Institutes for Research in Washington, DC and the University of Zambia's Research Ethics Committee, and informed consent was obtained from all

---

while expanding coverage (reaching 175,000 beneficiary households as of 2015, 590,000 in 2017 and aiming to scale-up to 700,000 households in 2018). CGP participants as well as the control group households were retargeted for the new national programme (the Harmonized Social Cash Transfer) starting in 2016, and those in the control group who did not qualify were given a lump sum 'compensation' payment of Zambian kwacha (ZMW) 500.

[110] Note that this is a multisite social experiment because random assignment of communities occurs within each of the three districts

study participants. Questionnaires and reports for the CGP are available on the Transfer Project website (http://www.cpc.unc.edu/projects/transfer).

Finally, the baseline report of the Zambian Child Grant Programme Impact Evaluation (AIR, 2011) provides a conceptual framework – developed by the implementing Ministry and development partners in Zambia – exploring how the CGP could impact household and child level outcomes. In the short term, the expectation is that targeted households, living in extremely poor conditions, will spend almost all of the grant on basic necessities; once they have met these demands however, households might decide to save or invest part of the cash transfer in other areas such as human capital or productive activities. Any impact of the programme on children will work through the household via spending or time allocation decisions (including use of services).[111] The unconditional nature of the programme allows households the freedom to spend the transfer as they wish, and expands the potential range of outcomes the programme may affect, including schooling of 'non-target' children.

4.4 Context: the Zambian educational system

Zambia's school system consists of a primary (7 years) and a secondary (5 years) level. Children are expected to enter primary school at age 7 and complete lower basic (grades 1-4) at age 11 and middle basic (grades 5-7) education by age 13. Students take common examinations at the end of the primary cycle and successful pupils are awarded a Certificate of Primary Education and allowed to continue to secondary education. Secondary education is divided into junior secondary (or upper basic) – which comprises grades 8 and 9 – and senior secondary (or high school) – which corresponds to grades 10 to 12. Again, there are common examinations at the end of grade 9 (Junior Secondary School Certificate) and successful students are allowed to continue to senior secondary. Successful

[111] As highlighted in Baird et al. (2014), "The ultimate impact of a cash transfer programme on schooling outcomes will depend on a number of moderating factors such as […] transfer size, recipient of the transfer, baseline enrolment rate and so on".

completion of high school requires passing the School Certificate Examination by the end of grade 12. This examination is also used for selection into tertiary education programmes at universities, colleges and technical institutes. Due to the common examinations at grades 7 and 9, these are threshold levels at which significant dropout occurs.

Net enrolment in primary education[112] has been steadily increasing over the last years: it rose from 70.5 per cent in 2001 to 85.9 per cent in 2004 and 93.7 per cent in 2012. Primary completion rates have increased from 63.3 per cent in 2000 to 74.1 per cent in 2004 and 91.3 per cent in 2012 (World Development Indicators, World Bank). However, children are entering school at later ages. During 2004 to 2012, less than 47 per cent of children entered school at age 7[113] while to the remainder enrolled later.[114] The reasons for late enrolment range from inadequate classroom space, long distances to schools, socio-economic and cultural beliefs (Musonda and Kaba 2011). Although primary completion rates are above 90 per cent, transition to lower secondary education has not improved much. According to the latest data, of those students who were enrolled in primary education in 2010, only 56 per cent continued to secondary education – a percentage similar to that recorded in 2001 (World Development Indicators, World Bank).

There are a number of barriers that prevent child schooling,[115] in particular economic constraints. School fees represent a major barrier to transition to secondary schooling; even though no formal fee is charged at primary level, there are still some informal fees that households often incur such as Parent Teacher Association or other types of contributions. Other indirect costs encompass, among others, uniforms and shoes; these are often compulsory at secondary level but

[112] Net School Enrolment measured as the total is the ratio of children of the official primary school age who are enrolled in primary school to the total population of the official primary school age.
[113] The average net intake ratio in grade 1 for the period 2004−2012 was 47.6 per cent. The average net intake ratio corresponds to the number of new entrants in the first grade of primary education who are of the official primary school entrance age, expressed as a percentage of the population of the same age.
[114] As suggested by the average gross intake ratio in grade 1 for the period 2004−2012, 118 per cent. The average gross intake ratio is the number of new entrants in the first grade of primary education regardless of age, expressed as a percentage of the population of the official primary entrance age.
[115] I am thankful to Mumbi Christopher − Headmaster of Serenje Boma Secondary School, and Mulenga Astone − Head Teacher of Chimfunde Primary School (Serenje), who provided valuable insights into constraints to schooling in rural areas in Zambia.

there is social stigma attached to not owning these items even at the primary level; moreover, there are stationary and equipment costs. Another important physical constraint is distance; indeed, for many rural children it takes a long time to travel to school, even primary school. For this reason, and especially at the lower secondary level, children often have to move away from home to continue their schooling; not only does this represent an extra cost for households, but it also represents a risk for children who may then have to live on their own. Barriers to female access are particularly stringent in this context; safety risks are higher for girls walking long distances to school or living on their own; lack of sanitation can also prevent girls from continuing their schooling after attaining menarche. School teachers we spoke to during field work also suggested that early marriage was an important cause of girls' school dropout.

4.5 Data and sample

4.5.1 Data, instruments and outcome indicators

In October-November 2010, a baseline survey was conducted in study areas prior to the random assignment to treatment status; therefore, neither the households nor the enumerators knew who would receive the programme. The first payment of the cash transfer was made in February 2011. Follow-up data was collected 24, 30 and 36 months after baseline. Table 4.1 shows the timing of data collection (see also Figure C.2). All waves of data collection occurred during the lean season with the exception of wave 3, which was administered during the harvest season and entailed a shorter survey focusing on consumption in order to assess the impact of the CGP on consumption smoothing by comparing impacts across the agricultural cycle. Since the 30- and the 36-month observations are gathered during the same (academic) year we do not use the 30-month survey in this paper.

Table 4.1: Timing of data collection in CGP evaluation

Baseline	Follow-ups		
Wave 1	**Wave 2**	**Wave 3**	**Wave 4**
Oct-Nov 2010	Oct-Nov 2012	June-July 2013	Oct-Nov 2013

The core survey instrument used in each round is a household questionnaire, though a community questionnaire was also administered in each wave to gather information on local prices, shocks, and other contextual characteristics; a health facility questionnaire was also administered at baseline.

The main respondent to the household questionnaire is typically the intended CGP recipient, the primary caregiver of young children, virtually all of whom are women. The household survey is multi-topic and very similar – both in layout and coverage – to the national Living Conditions and Monitoring Survey (LCMS); the instrument also takes elements from the Zambia Demographic and Health Survey (ZDHS). The questionnaire contained detailed modules on consumption expenditures, household assets and housing conditions, subjective poverty and food security, but also health, early childhood development, anthropometric measurements and, of main interest for this paper, an education module that collects information for all household members (above age 3) and one on economic activities specific for children 5 to 18 years old.

Our main schooling variable captures whether the child is currently attending school ('Is … currently attending school?'); it is a dummy equal to 1 if the child is currently attending school, 0 otherwise. Given the age range of the children we focus on, virtually all are still in primary school. We further look at regular attendance, measured as the number of days a child attended school in the last week; it is a continuous variable that ranges between 0 and 5.

As for child work, a child is defined as working if s/he normally does any work, either paid or unpaid, including unpaid domestic work/chores (binary variable).[116] We explore both extensive and intensive margins by analyzing not only whether the child is involved in any work but also how

[116] The question asks: 'Does…… normally do any work, either paid or unpaid, including unpaid domestic work/chores?'

many hours s/he spend on these activities. A child is then considered to be engaged in un/paid work if s/he spent at least an hour in un/paid work in the past two weeks (binary variable).[117] Unfortunately, it is only possibly to distinguish paid and unpaid activities but not to sharply discern between economic activities and domestic chores. Whereas for paid work additional information was collected on the most common types of work performed, for unpaid work this information is not available at baseline but was only added from the 24-month follow-up onwards.

4.5.2 Sample, integrity of experimental design and descriptive statistics

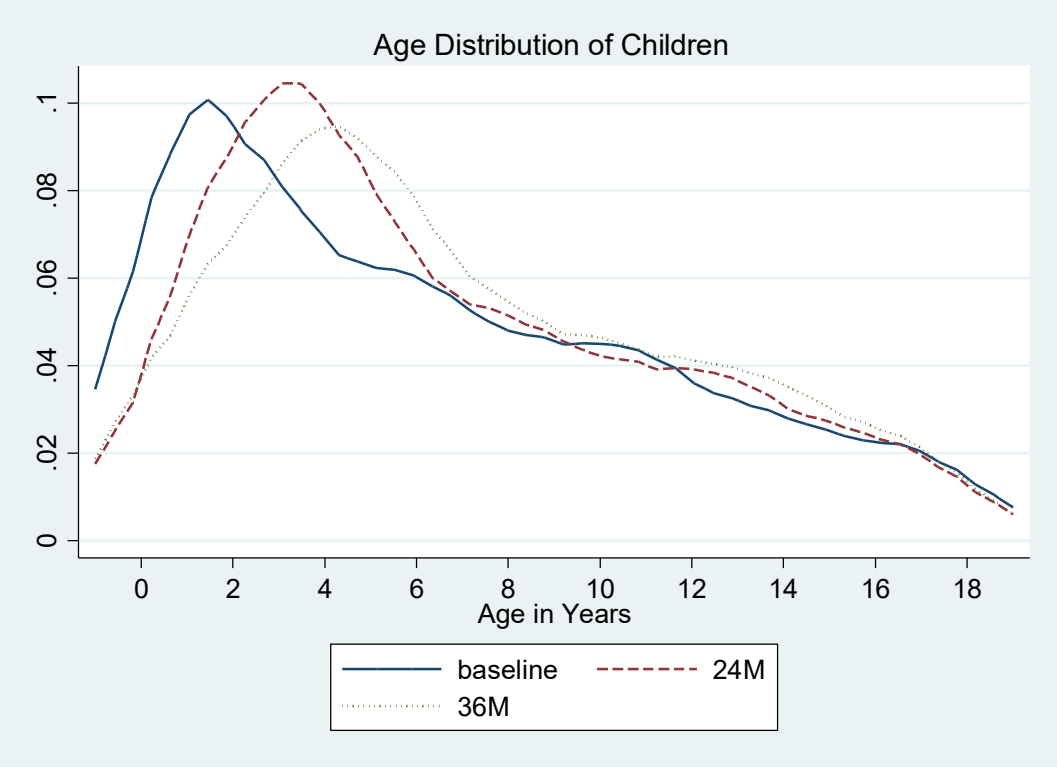
There are 2,519 households and 14,565 people in the evaluation study, including 4,793 children ages 5 and under, with the largest number under 1 year old (1,427). Figure 4.1 below captures the age distribution of children aged 0-17 at baseline and then after two and three years. These density graphs show that the majority of children in these beneficiary households are very young at baseline and even by wave 4 – three years after programme initiation – the modal age is less than 5 years old. In contrast, there are very few children over age 13 and two-thirds of households actually have no members age 14-17 in the household.

Given that the compulsory school age is 7 and households targeted for the programme do not have many children over age 14, we focus our analysis on children age 7−14 in this analysis; this also reflects the structure of the Zambian education system – as discussed in Section 4.4 – as primary school spans from 7 to 13 years old.

We focus on two different samples, the unbalanced and the balanced panel. The unbalanced panel consists of children aged 7−14 at baseline who appear in the survey at least twice (at baseline and at least one follow-up). The balanced panel consists of children who appear in all three waves of the study.

[117] The question asks: 'In the past two weeks, how many hours did…… spend in [paid/unpaid] work?'

Figure 4.1: Age distribution of children in the Child Grant Programme



Baseline balance

Table 4.2 reports demographic and household characteristics of this group at baseline (cross-section). The sample is predominantly rural and mainly engaged in agriculture. The mean household size is 7 and the sample as a whole is very young which is in line with the eligibility requirement of the programme. On average two household members are children under 5 and two are between 6 and 12. There are very few elderly members of the household and the majority of adults (1.2 on average) is in the age range 19−35. Children are on average 10 years old and half of them are girls and half are boys. Virtually all of the transfer recipients are female with a mean age of 35; 80 per cent are married and a significant proportion (30 per cent) never attended school. Finally, the sample is poor – with 95 per cent of the sample living in extreme poverty (not shown)[118] – and mean monthly per capita

[118] In 2010 units, the extreme poverty line is roughly ZMW 90.5 and the moderate poverty line is roughly ZMW 155.

expenditure is around ZMW 34, or less than 30 US cents per person per day (equivalent to USD 0.60 PPP).

Around 74 per cent of children aged 7−14 were attending at baseline and attendance is around three days per week.[119] Around half of the communities (46, according to the community questionnaire) have a primary school within the CWAC; sixty-eight per cent of households at baseline do not know where the nearest lower basic school is located and half of them (53 per cent) report that the school is more than half an hour away (not shown). In our sample, children take on average thirty minutes to reach primary school.

Six out of ten children are involved in some form of work, either paid or unpaid, with the vast majority involved in unpaid work; note that paid work is quite low among this age group − only 2 per cent in this sample. On average, a child spent around 12 hours in the last two weeks in paid and unpaid work, or less than an hour per day.

Assignment into the programme is random for households with children under age 5 at baseline while our analysis sample are children aged 7−14, not explicitly targeted by the programme.[120] The last column in Table 4.2 provides the test for equality of means at baseline to assess balance among this sample of children. There is no statistically significant difference between treatment and control children in that age range in our outcome variables (schooling and child work).

All household level characteristics are also balanced with one exception: recipients in control communities are around 4 percentage points more likely to be divorced in the treatment arm. We control for this variable in our econometric specifications (see below). The baseline official evaluation report tested a much longer list of indicators (over 60) for differences between the treatment and control groups and concluded that the randomization was successful (AIR 2011).

[119] This refers to attendance among all children whereas attendance among attending children only is around 4.5 days.
[120] Even though it is the household – rather than the child *per se* − which is the beneficiary of the programme.

Table 4.2: Baseline characteristics of children aged 7−14 and test for baseline equivalence, cross-sectional sample

	All	Control	Treatment	P-value of diff.
Child's age in years	10.05	10.00	10.11	0.15
Child is female	0.50	0.51	0.50	0.67
Recipient's age	35.24	35.20	35.27	0.88
Recipient never married	0.06	0.07	0.06	0.54
Recipient divorced or separated	0.10	0.13	0.08	0.02
Recipient widowed	0.08	0.08	0.08	0.83
Recipient attended school	0.71	0.69	0.72	0.50
Household size	7.26	7.15	7.37	0.25
Number of members aged 0-5 years	1.97	1.96	1.97	0.85
Number of members aged 6-12 years	2.22	2.21	2.24	0.72
Number of members aged 13-18 years	0.97	0.92	1.02	0.11
Number of members aged 19-35 years	1.18	1.14	1.21	0.22
Number of members aged 36-55 years	0.82	0.81	0.83	0.79
Number of members aged 56-69 years	0.08	0.08	0.08	0.90
Number of members aged 70+ years	0.03	0.03	0.02	0.66
Shangombo district	0.30	0.31	0.29	0.85
Kaputa district	0.30	0.30	0.29	0.89
Outcome indicators				
Child is currently attending school	0.74	0.74	0.74	0.97
Days in attendance in prior week	3.31	3.32	3.29	0.84
In past two weeks…				
Child is involved in paid or unpaid work	0.58	0.60	0.56	0.39
Child is involved in paid work	0.02	0.02	0.03	0.45
Child is involved in unpaid work	0.55	0.57	0.53	0.34
Hours in paid or unpaid work	12.64	12.58	12.70	0.95
Hours in paid work	0.24	0.15	0.33	0.35
Hours in unpaid work	12.40	12.43	12.36	0.98
Indicators used for heterogeneous analysis				
Per capita consumption expenditure (ZMW)	33.53	32.46	34.60	0.37
Education expenditures per child (ZMW)	13.68	14.88	12.47	0.26
Education expenditures per child (logged)	1.51	1.58	1.46	0.39
Uniform expenditures per child (ZMW)	5.46	6.00	4.92	0.25
Uniform expenditures per child (logged)	0.66	0.71	0.61	0.38
Distance to primary school in minutes*	31.91	32.76	31.05	0.48
N children	3,080	1,542	1,538	
N households	1,590	795	795	

Notes: Standard errors are clustered at the community level. 'Recipient' indicates official programme beneficiary. Education and uniform expenditures are computed among all children, not only those currently attending.
*Distance to primary school in minutes is computed as average minutes to primary school per household (average across waves per child and then per household).

Attrition analysis

Loss to follow-up (attrition) at the household level in the study is quite low, with the highest rate (9 per cent) reported at 24 months and driven by the drying of the Cheshi Lake in Kaputa District in the

[121] All values are expressed in ZMW, and deflated to 2010/baseline. Baseline values were rebased.

Northern Province. However, many of these households were recovered in subsequent rounds so that overall attrition by wave 4 (36-month follow-up) was only 2 per cent (see Table C.1 in the Appendix). As shown in the official Evaluation Report, there is no significant difference in attrition by treatment status and selective attrition, namely the characteristics of households more likely to be re-surveyed do not differ across arms (AIR, 2011).

We also check for overall and selective attrition at the individual level. Child attrition rates do not significantly differ across treatment arms (see Table C.1 in the Appendix). However, as a further check we also verify whether characteristics differ between children lost to follow up in treatment and in the control arm at baseline. We focus on the unbalanced panel which is where attrition is lowest and on which we will focus on when interpreting results (columns 5 and 6 in Table C.2 in the Appendix). Children lost to follow-up in the treatment group are more likely to be living in households with a smaller number of elderly ($p<0.5$). Among attritors, there are no other statistically significantly differences at the five per cent level between arms.

Overall, we conclude that attrition does not threaten the internal validity of the experiment. Nevertheless, specially constructed household weights (inverse probability weights) were used for a robustness check to make sure that the impact estimates are robust and representative of all eligible households. Other sensitivity analyses are discussed in Section 4.9.

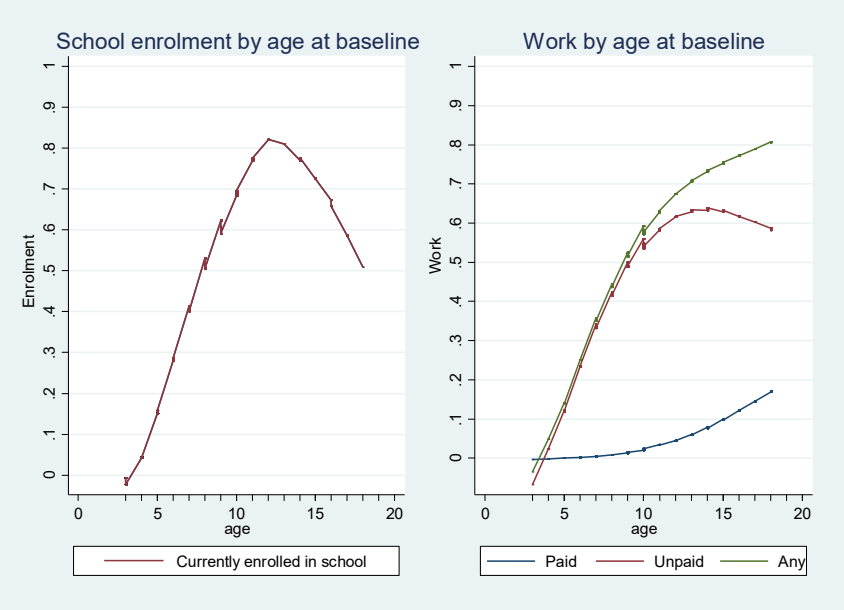
Schooling and child labour

Three-quarters of children in our sample attend school. Virtually all children (99.4 per cent) attending school are still in primary, and 84 per cent in lower basic school (grades 1−4). Moreover, only 44 per cent of children aged seven – the school entry age – are attending school. Overall, these statistics indicate children start school later than expected.

Figure 4.2 presents density graphs for school attendance and work by age to understand better the pattern of schooling in Zambia. School attendance rates increase steeply starting from early ages up to age 12−13 at which point dropout begins. This is also the age that coincides with the rather

sharp increase in paid work (as diplayed in the right hand panel of Figure 4.2). While overall rates of paid work are low, the age at which school dropout begins also coincides with the age when unpaid work is displaced by paid work. Based on these trends we hypothesise that the CGP might delay dropout around ages 12-13. On the other hand, the space where an impact might be observed on work is above age 13−14 and there are very few children in that age group in the CGP − thus we do not expect to see much impact, if any, on work given the nature of the target population.

Figure 4.2: School and work by age at baseline



At baseline, very few children are involved in paid work in our sample; around half of them (49 per cent) are involved in casual labour, almost a third (29 per cent) in farming crops and 10 per cent in domestic work. We do not have data on the type of activity performed for children involved in unpaid work at baseline. In theory, it is possible that children performing unpaid work are actually engaged in agricultural work. However, based on a question from the 24-month follow-up not asked at baseline, 98 per cent of children engaged in unpaid work actually carry out domestic activities two years after the onset of the programme; this suggests unpaid work might be proxying for domestic chores.

Finally, Table 4.3 below provides some indication of the complementarity and/or substitution between child work and schooling. Whereas almost half of the children aged 7−14 combine schooling with some form of work (paid or unpaid) almost a third only go to school; as children grow, it is more likely that they will be involved in work. If we consider complementarity/substitution of schooling with paid work then almost three-quarters of children only go to school whereas the rest are mainly idle. This seems to support some complementarity between schooling and unpaid work and some subtitution between schooling and paid work.

Table 4.3: Schooling and child work among children aged 7−14

	Any work (either paid or unpaid)		Paid work	
	Not working	Working	Not working	Working
	Children aged 7−14 (N=3,025)			
Not enrolled	13.7	11.9	25.1	0.5
Enrolled	28.0	46.4	72.6	1.8
	Children aged 7−10 (N=1,817)			
Not enrolled	20.0	14.4	34.1	0.3
Enrolled	29.0	36.6	65.2	0.4
	Children aged 11−14 (N=1,208)			
Not enrolled	4.4	8.0	11.6	0.8
Enrolled	26.3	61.3	83.8	3.8

Notes: This table uses the cross-sectional sample of children with non-missing data for schooling and work outcomes.

4.6 Empirical strategy

The core model we estimate is a multivariate difference-in-differences (DD) controlling for a select number of covariates (described below) that are all measured at baseline.

$$Y(i,j,t) = \alpha + \beta_T T(j) + \beta_{R2}(R2) + \beta_{R4}(R3) + \beta_{TR2}(T(j) * R2) + \beta_{TR4}(T(j) * R4) + \sum_{k=1}^{K} \theta_k X_k(i,t) + \varepsilon(i,t) \quad (1)$$

In this framework, $Y(i,j,t)$ is the the outcome indicator (either schooling or child labour) for the individual child *i* in community *j* at time *t*. $T(j)$ is a dummy variable indicating treatment status; R2 and R4 are dummy variables for the two time periods considered where R2 refers to the 24-month follow up and R4 to the 36-month follow-up; β_{TR2} and β_{TR4} capture the intent-to-treat effect at times

2 and 4; X is a set of control variables measured at baseline; ε is the error term. Robust standard errors are adjusted for clustering at the community level.[122] All models are estimated using linear probability models for ease of interpretation.

The key assumption behind the DD is that of parallel trends. Unfortunately, we cannot run a parallel trend test as we do not have multiple pre-treatment data points nor an alternative control group to explicitly test for differences in trends between treatment arms; it is also difficult, given the unconditional nature of the grant, to identify another outcome supposedly not affected by the programme to see if it displays the same trend across treatment and control clusters. However, the randomness of the treatment assignment to start with, as well as the fact that control communities are drawn from the same districts as treatment ones (geographical proximity), and the relatively short period between baseline and follow ups should lend some confidence to the parallel paths assumption. In addition, we have analyzed trends in village-level prices (captured through the community questionnaire) during the study period and found no statistically significant difference between treatment and control arms over time. Finally, we also analyzed impacts on both beneficial and detrimental external shocks at the community level by treatment status and found no significant difference across arms (AIR, 2013; AIR, 2014).

Similar to Skoufias and Parker (2001), we estimate equation (1) separately for schooling and child work. We report adjusted estimates. In the multivariate model, our set of basic demographic covariates include: 1) child characteristics (age, age squared and gender); and 2) household characteristics (age of recipient, marital status of recipient, whether the recipient has ever attended school, log of household size, a set of dummies capturing household composition, district dummies).[123] All variables are measured at baseline, means of which are presented in Table 4.2.

---

[122] Randomization happened at the community level and households are clustered in communities. Therefore, it cannot be assumed that each household is independent from the other as households in the same community share community-level characteristics that differ from those of households in other communities. Clustered robust standard errors help in taking into account this lack of independence and the clustering of households in communities.

[123] Control variables include: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy

We estimate equation (1) for children aged 7−14 and then separately for younger and older children given the sharp drop-out rate that begins in the older age group. We report estimates for both the unbalanced and the balanced panel. The unbalanced panel tracks outcomes among the same age cohort of children as they move through the study. For those aged 7−14 years at baseline, the unbalanced panel analysis compares them to children aged 9−16 and 10−17 at 24 and 36 months respectively. The balanced panel is the strongest in terms of internal validity, however it may not be representative of the typical child in beneficiary households due to the higher level of attrition in this sample. We report results for both panels but focus the discussion on the unbalanced panel in case of discrepancies. The benefit of the unbalanced panel is that it includes the households living in Kaputa who were briefly lost due to the drying of the lake at 24 months, but were then interviewed at 36 months.[124]

We also carry out an analysis of heterogeneity of treatment to investigate whether the effects of the intervention on our main outcomes of interest vary depending on observed characteristics of subgroups of the population at baseline. To investigate the heterogeneous impacts of the programme, we use a difference-in-difference-in-difference estimation (DDD). The following equation is estimated:

$$\begin{aligned} Y(i,j,t) = \alpha &+ \gamma_1 C(i) + \gamma_2 T(j) + \gamma_3 (C(i) * T(j)) + \gamma_4 (R2) + \gamma_5 (T(j) * R2) + \gamma_6 (C(i) * R2) \\ &+ \gamma_7 (C(i) * T(j) * R2) + \gamma_8 (R3) + \gamma_9 (T(j) * R3) + \gamma_{10} (C(i) * R3) \\ &+ \gamma_{11} (C(i) * T(j) * R3) + \sum_{j=1}^{J} \vartheta_j X_j(i,t) + \varepsilon(i,t) \end{aligned} \quad (2)$$

where − apart from the previously mentioned set of variables − C is the characteristic of

is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo). Household size is logged in order to be able to include household composition controls.

[124] The main evaluation reports for this study use pooled cross-sectional DD impact estimates, which compare children aged 7−14 with children of the same age at follow-up. This approach does not take advantage of the longitudinal nature of the study. The evaluation reports containing these estimates are publicly available at http://www.cpc.unc.edu/projects/transfer/countries/zambia.

interest, either individual or household characteristic, measured at baseline. On the right-hand side, a triple interaction term for each follow-up is included involving three variables: treatment, time, characteristic of interest measured at baseline; the equation also includes all the individual variables as well as their pairwise interactions. The main parameters of interest are the coefficients on the triple interaction terms at 24 and 36 months, γ_7 and γ_{11} respectively; these coefficients indicate how the impact of the CGP programme on our primary outcomes varies with the initial degree of the characteristic of interest.

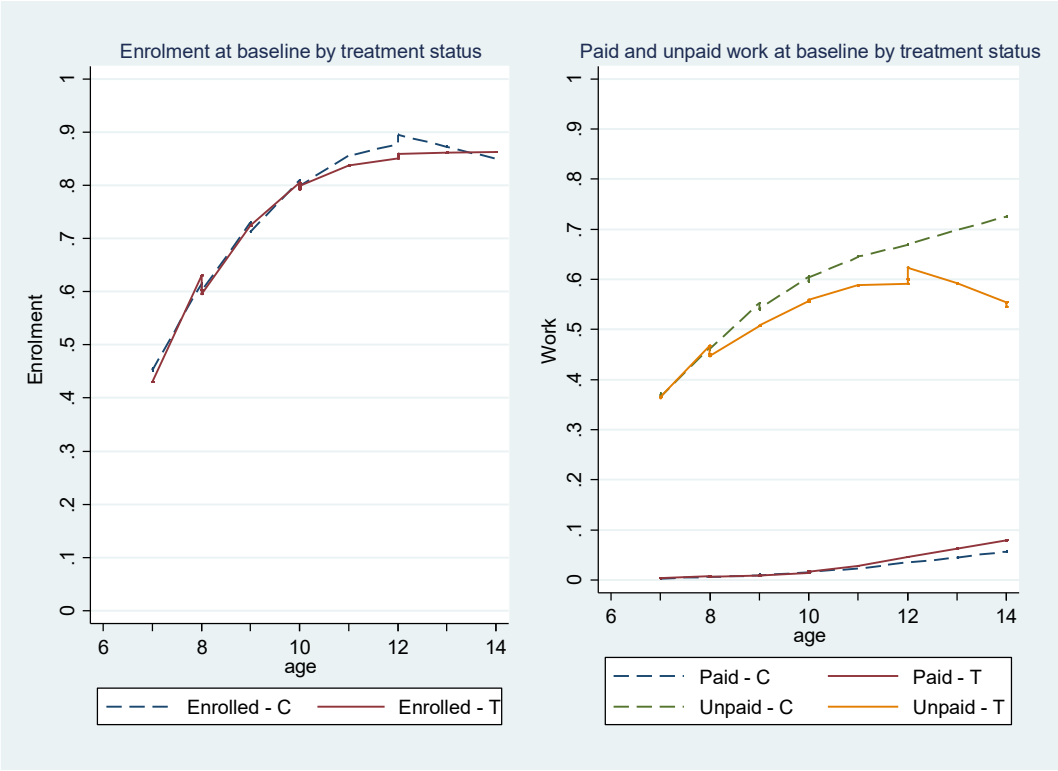
Apart from gender, for both schooling and child work outcomes, we also test whether there are heterogeneous impacts depending on 1) the level of consumption (and in particular whether the household falls in the bottom/poorest 50 per cent); 2) the household size; indeed the cash transfer is flat and we hypothesise it could have larger impacts in smaller households that have at their disposal a larger amount to spend; 3) the education of the recipient (or intended beneficiary), in order to see whether the level, and possibly value, of education could further stimulate programme impacts; and finally 4) the average distance to primary school (in minutes) as the availability and distance from school are main barriers in many developing countries (see Handa, 2002; Filmer, 2004 among others) and could therefore influence the impacts of the programme.

4.7 Results

We begin by showing graphically the pattern in school enrolment and work by study arm at baseline and at 36 months to get a sense of the underlying patterns in the data and the likely impact of the programme. Figures 4.3 and 4.4 show baseline and 36-month follow-up outcomes respectively for the unbalanced panel of children aged 7−14 at baseline. For schooling, at 36 months (left panel, Figure 4.4), we see a clear separation in the two lines by around age 11 which is exactly the age when the curve begins to flatten, suggesting that the CGP might be delaying dropout at this critical threshold. On the other hand, as far as work is concerned, there appears to be a programme effect for paid work,

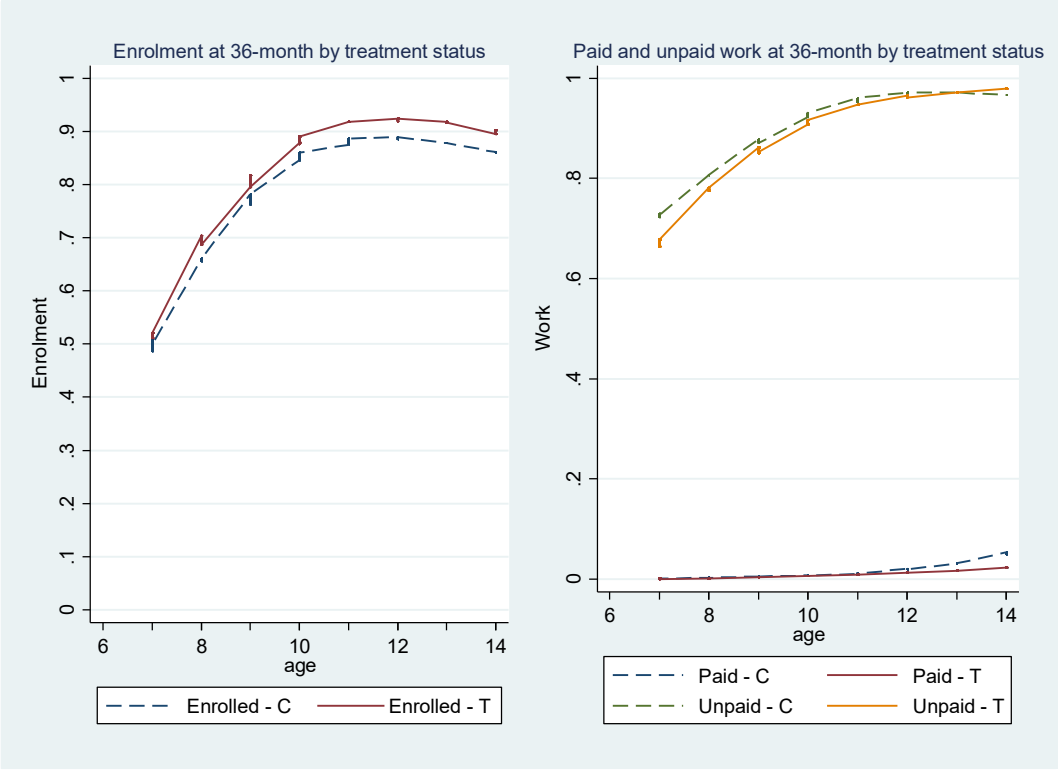
where it seems the CGP might be reducing the increase in paid work among older children (right panel, Figure 4.4).

Figure 4.3: School and work by age and study arm at baseline



Notes: The curves are obtained through a smoothing procedure called lowess (robust locally weighted regression). These figures refer to the unbalanced panel.

Figure 4.4: School and work by age and study arm at 36 months



Notes: The curves are obtained through a smoothing procedure called lowess (robust locally weighted regression). These figures refer to the unbalanced panel.

4.7.1 Impact estimates

Table 4.4 (columns 1 and 2) presents impact estimates on attendance for children aged 7−14 for both the balanced and unbalanced panels, adjusted with the set of covariates as described above in Section 4.6 on the empirical strategy. There are no statistically significant impacts of the CGP on attendance for this age group in any wave for either sample with the exception of a marginally significant impact of 4.4 percentage points at 36 months.

Table 4.4: Impact of CGP on school attendance and child work among children aged 7−14 at baseline

	SCHOOL		ANY WORK		PAID WORK		UNPAID WORK	
	(1) Unbalanced panel	(2) Balanced panel	(3) Unbalanced panel	(4) Balanced panel	(5) Unbalanced panel	(6) Balanced panel	(7) Unbalanced panel	(8) Balanced panel
24-month follow-up	0.0592***	0.0509***	0.235***	0.239***	0.00760	0.00406	0.258***	0.264***
	(0.0172)	(0.0172)	(0.0346)	(0.0368)	(0.0102)	(0.0104)	(0.0349)	(0.0372)
36-month follow-up	0.0389**	0.0331*	0.282***	0.287***	-0.00887	-0.00840	0.322***	0.328***
	(0.0188)	(0.0197)	(0.0344)	(0.0363)	(0.00869)	(0.00979)	(0.0339)	(0.0352)
Treated	-0.00690	0.00118	-0.0477	-0.0207	0.00583	0.000495	-0.0510	-0.0199
	(0.0243)	(0.0263)	(0.0438)	(0.0450)	(0.00787)	(0.00961)	(0.0442)	(0.0457)
Impact at 24 months	0.0179	0.0181	0.0636	0.0427	-0.0281**	-0.0193	0.0622	0.0347
	(0.0247)	(0.0262)	(0.0479)	(0.0497)	(0.0117)	(0.0119)	(0.0480)	(0.0502)
Impact at 36 months	0.0440*	0.0440	0.0464	0.0195	-0.0221*	-0.0197	0.0441	0.0117
	(0.0249)	(0.0273)	(0.0489)	(0.0502)	(0.0129)	(0.0140)	(0.0484)	(0.0498)
N	8,208	7,014	8,116	6,943	8,116	6,943	8,137	6,961
R-squared	0.123	0.133	0.251	0.244	0.022	0.023	0.247	0.236
Baseline control	0.74	0.74	0.61	0.60	0.02	0.02	0.57	0.57

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Specifications are adjusted and include as controls: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings, (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).

Dependent variables: School attendance is a binary variable equal to 1 if the child is currently attending school, 0 otherwise. Any work is a binary variable equal to 1 if the child is normally involved in any work, either paid or unpaid, including unpaid domestic work/chores. Paid work is a binary variable equal to 1 if the child spent at least an hour in paid work in the past two weeks. Unpaid work is a binary variable equal to 1 if the child spent at least an hour in unpaid work in the past two weeks.

Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

The results on child work for children aged 7−14 are reported in Table 4.4 (specifications 3 to 8) and these also show no programme impacts with the exception of a significant negative impact on paid work in the unbalanced panel at 24 months of around three percentage points, which however fades away at 36 months and is not found in the balanced panel (see columns 5 and 6). Impacts on the intensive margins of schooling and work are reported in Table C.3 in the Appendix and are in line with impacts reported in Table 4.4.

We further report heterogenous impacts of the programme by age, younger children aged 7 to 10 and older children aged 11 to 14. Table 4.5 reports impact estimates for schooling and work for children aged 7−10 and here too, for neither specification nor sample is there a program effect on the outcomes. For this age group we do not disaggregate work between paid and unpaid as participation in paid work is almost null. Findings related to the intensive margins of attendance and work are consistent and are reported in Table C.5 in the Appendix.

Table 4.5: Impact of CGP on school attendance and child work among children aged 7−10 at baseline

	SCHOOL ATTENDANCE (1/0)		ANY WORK (1/0)	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Balanced panel	Unbalanced panel	Balanced panel
24-month follow-up	0.0857***	0.0782***	0.256***	0.237***
	(0.0243)	(0.0274)	(0.0378)	(0.0385)
36-month follow-up	0.0844***	0.0820***	0.313***	0.292***
	(0.0269)	(0.0291)	(0.0388)	(0.0380)
Treated	0.00857	0.00639	-0.0371	-0.0169
	(0.0345)	(0.0360)	(0.0460)	(0.0467)
Impact at 24 months	-0.00213	0.00943	0.0643	0.0505
	(0.0346)	(0.0365)	(0.0493)	(0.0506)
Impact at 36 months	0.0205	0.0289	0.0350	0.0119
	(0.0351)	(0.0366)	(0.0513)	(0.0516)
N	4,964	4,290	4,919	4,253
R-squared	0.168	0.185	0.280	0.271
Baseline control mean	0.65	0.64	0.53	0.53

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Estimations are adjusted and include: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).
Dependent variables: School attendance is a binary variable equal to 1 if the child is currently attending school, 0 otherwise. Any work is a binary variable equal to 1 if the child is normally involved in any work, either paid or unpaid, including unpaid domestic work/chores.
Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

For older children (aged 11−14) on the other hand we now see some programme effects at 36 months, the point estimates in Table 4.6 (columns 1 and 2) indicating an increase in school attendance of between 7 and 8 percentage points depending on the sample (unbalanced or balanced panel). These are large impacts, especially considering that at baseline the attendance rate was high (89 per cent). Impact estimates indicate that the Child Grant Programme enabled beneficiary households to enrol children aged 11 to 14 in school and improve their attendance. Indeed, investigating further, we find that the cash transfer also improved the regularity of attendance – as measured by days attended in the prior week – for this age group. Impact estimates at 36 months indicate a significant half a day attendance impact (columns 3 and 4); these specifications are estimated using the full sample of children, both attending school and not.

Table 4.6: Impact of CGP on school attendance and regular attendance among children aged 11−14 year at baseline

	ATTENDANCE (1/0)		REGULAR ATTENDANCE (days in prior week)	
	(1) Unbalanced panel	(2) Balanced panel	(3) Unbalanced panel	(4) Balanced panel
24-month follow-up	0.0209	0.00512	0.295**	0.167
	(0.0239)	(0.0244)	(0.142)	(0.163)
36-month follow-up	-0.0279	-0.0442	0.00795	-0.145
	(0.0286)	(0.0303)	(0.174)	(0.195)
Treated	-0.0308	-0.00800	-0.0999	-0.0396
	(0.0238)	(0.0224)	(0.150)	(0.149)
Impact at 24 months	0.0508*	0.0341	0.292	0.250
	(0.0286)	(0.0264)	(0.187)	(0.197)
Impact at 36 months	0.0805**	0.0685**	0.433**	0.439**
	(0.0322)	(0.0329)	(0.201)	(0.212)
N	3,244	2,724	3,159	2,653
R-squared	0.072	0.074	0.073	0.076
Baseline control mean	0.89	0.89	3.9	4.0

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Estimations are adjusted and include: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omotted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).
Dependent variables: School attendance is a binary variable equal to 1 if the child is currently attending school, 0 otherwise. Regular attendance is measured as the number of days a child attended school in the prior week.
Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

Table 4.7 (columns 1 and 2) indicates that there is no programme effect on work – which combines participation in unpaid and paid work – among this age group neither after two nor after three years of implementation.[125] There is also no impact on participation in unpaid work, which proxies for domestic work (colums 7 and 8) and which is the main type of work for virtually all children. Results on hours spent working on unpaid work and on any type of work is reported in Table C.6 in the Appendix and is consistent (i.e. no significant impacts).

As this is exactly the age where paid work begins to increase noticeably, we provide separate estimates for the likelihood of engaging in any paid work (columns 3 and 4). These estimates show a significant impact of the CGP in reducing the probability of paid work at 36 months by 4 to 5 percentage points with strongest impacts in the unbalanced panel; impacts at 24 months are not significant or only weakly significant.[126] We find impacts on paid work not only on the extensive margins (paid work halves in the treatment group whereas it increases by 50 per cent in the control group) but also on the intensive margins, as shown in columns 5 and 6; indeed, the programme leads to a significant negative impact of around an hour in the time older children (aged 11−14) spend on paid activities.

[125] Table C.6 in the Appendix also indicates no impact on the intensive margins, namely hours spent working in paid or unpaid activities.
[126] The 'treated' variable in each of these tables is not statistically significant indicating no difference at baseline between the treatment and control group.

Table 4.7: Impact of CGP on child's work among children age 11-14 years at baseline

	ANY WORK		PAID WORK				UNPAID WORK	
	Participation (1/0)		*Participation* (1/0)		*Hours*		*Participation* (1/0)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unbalanced panel	Balanced panel	Unbalanced panel	Balanced panel	Unbalanced panel	Balanced panel	Unbalanced panel	Balanced panel
24-month follow-up	0.196***	0.219***	0.00500	-0.000554	0.225	0.212	0.238***	0.266***
	(0.0389)	(0.0439)	(0.0186)	(0.0188)	(0.378)	(0.410)	(0.0390)	(0.0431)
36-month follow-up	0.235***	0.257***	-0.00193	-0.00151	0.220	0.236	0.290***	0.319***
	(0.0388)	(0.0437)	(0.0154)	(0.0170)	(0.225)	(0.247)	(0.0385)	(0.0421)
Treated	-0.0621	-0.0240	0.0143	0.00252	0.309	0.263	-0.0760	-0.0281
	(0.0485)	(0.0519)	(0.0155)	(0.0191)	(0.347)	(0.466)	(0.0505)	(0.0549)
Impact at 24 months	0.0648	0.0310	-0.0397*	-0.0234	-0.676	-0.619	0.0649	0.0202
	(0.0547)	(0.0591)	(0.0212)	(0.0221)	(0.442)	(0.532)	(0.0559)	(0.0604)
Impact at 36 months	0.0629	0.0292	-0.0508**	-0.0438*	-1.023**	-1.040**	0.0709	0.0242
	(0.0527)	(0.0568)	(0.0240)	(0.0256)	(0.430)	(0.516)	(0.0529)	(0.0576)
N	3,197	2,690	3,197	2,690	3,180	2,675	3,204	2,695
R-squared	0.178	0.176	0.020	0.022	0.018	0.019	0.188	0.185
Baseline control mean	0.72	0.71	0.04	0.05	0.33	0.41	0.68	0.65

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Specifications are adjusted and include as controls: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).
Dependent variables: Any work is a binary variable equal to 1 if the child is normally involved in any work, either paid or unpaid, including unpaid domestic work/chores. Paid work is either a binary variable equal to 1 if the child spent at least an hour in paid work in the past two weeks or the total number of hours worked in the last 2 weeks. Unpaid work is a binary variable equal to 1 if the child spent at least an hour in unpaid work in the past two weeks.
Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

We also tested for differential impacts on schooling (attendance and regular attendance) and child work (extensive and intensive margins) for children (age groups 7−14, 7−10 and 11−14) by gender. Triple interaction terms – γ_7 and γ_{11} in equation 2 – are never statistically significant, indicating the effect on the schooling and work outcomes explored is not significantly different between girls and boys (not shown). We find no robust heterogenous impact for any of the other interactions considered – as listed in Section 4.6 on empirical strategy – neither on schooling nor on child work. The triple interaction terms are never statistically significant with few exceptions (only weakly significant).[127]

4.8 The impact of CGP on school inputs

As discussed in Section 4.4, financial barriers remain important in Zambia even at primary levels despite the elimination of formal school fees. The provision of an unconditional cash transfer has the potential to alleviate some of these barriers that inhibit educational access; in this Section we investigate some of the potential pathways through which the CGP generates the impacts on school attendance reported above.

For each child currently enrolled in school, the evaluation team collected detailed data on expenditure made by the household on each individual child on education in the current school year. Education expenditures include fees, uniform, transport, stationary and books, PTA levy and any other items as reported by respondent. Of particular interest is spending on uniforms as according to our informal discussions with teachers and school administrators during field work children who cannot afford uniforms often suffer from peer pressure and stigma which can increase both absenteeism and eventual dropout.

[127] Although not reported, differential impacts have been estimated also by age group and results are available upon request. No significant impact is found; however it should be noted that samples become smaller.

Using these data we estimate DD models using the unbalanced panel sample[128] to see whether education and uniform expenditures are higher for children in the treatment group with respect to those in control areas. Results for expenditure on education and uniforms are reported in Table 4.8. In each table we report results for all children in our analysis sample (ages 7−14) and also for the older age group (11−14) since impacts on schooling appear only among the older group and out-of-pocket expenditures are also higher among this age group. Our estimations are run over the full sample of children, whether attending or not.

Results indicate that school expenditures – which on average represent about a fourth of the monthly transfer – are 45 per cent higher for treated children than for their control peers after two years among children enrolled aged 7 to 14; though lower, the difference is still large and significant at 36 months (36 per cent). Among older children aged 11 to 14 the differences in overall spending are also quite large at 51 and 47 per cent after 2 and 3 years respectively. The last two columns of Table 4.8, which focus only on the uniform component of overall education spending, are revealing, and show that the CGP has enabled households to increase their spending on school uniforms for children of all ages (primary and secondary levels). Again, the point estimates are quite impressive − an increase of between 34 and 67 per cent in spending on uniforms depending on the wave and age group.

[128] We also estimated the DD using the balanced panel sample and results are consistent − these are available upon request.

Table 4.8: Impact of CGP on education and uniform expenditures by age group

	Total education expenditures, logged		Uniform expenditures, logged	
	(1)	(2)	(3)	(4)
	Age group 7−14	Age group 11−14	Age group 7−14	Age group 11−14
24-month follow-up	0.370***	0.510***	0.0303	0.165
	(0.107)	(0.178)	(0.0781)	(0.140)
36-month follow-up	0.596***	0.638***	-0.119	0.0686
	(0.117)	(0.219)	(0.115)	(0.190)
Treated	-0.151	-0.223	-0.112	-0.141
	(0.127)	(0.146)	(0.0935)	(0.121)
Impact at 24 months	0.446***	0.512***	0.656***	0.673***
	(0.130)	(0.182)	(0.121)	(0.172)
Impact at 36 months	0.355**	0.471**	0.344**	0.364*
	(0.145)	(0.222)	(0.149)	(0.196)
N	8,230	2,395	8,212	2,393
R-squared	0.153	0.107	0.081	0.077
Baseline control	1.6	1.9	0.70	0.83
Baseline control	14.8	20.0	6.0	7.0

Notes: The dependent variables are schooling and uniform expenditures made by the household on each child. Schooling and uniform expenditures are in log form. We report results for all children, whether attending or not. Estimations use difference-in-difference modelling. Robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Estimations are adjusted and include: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo). Estimations without controls are consistent with these results.
Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

4.9 Robustness checks

Several sensitivity analyses were carried out. As attrition was highest at 24 months due to migration out of the Cheshi Lake in Kaputa District, we re-estimated all our main regressions 1) using only the baseline to 36-month panel; 2) dropping observations from the Kaputa district; and 3) using inverse probability weights (IPW). Results are robust in each of these approaches: impacts on school attendance for older children (11−14 years old) remain statistically significant at 36 months.

All regressions of binary variables, such as whether the child attends or not, or whether the child works or not, were estimated using ordinary least squares (OLS) – namely linear probability models; indeed, probit and logit models are more unstable in the presence of interaction terms between binary variables (Angrist and Pischke 2009). Probit results from our main specifications

confirm the robustness of our findings. Similarly, results for time spent on different activities and for expenditures are also consistent with Tobit estimations (not shown).

All main impacts were also re-estimated using the same specifications but without the set of baseline covariates. Results using these unadjusted specifications are consistent and once more confirm successful randomization. All variables measured in hours were also estimated in the logged form.

As a robustness check, we also estimated the child work and schooling models using seemingly unrelated equations estimation. These results are reported in Table C.4 in the Appendix and confirm the lack of significant impacts of the programme on either outcome for the age group 7−14.

Finally, we also tried clustering standard errors at the household rather than at the community level, taking into account that intra-class correlation might be driven by siblings or multiple children within the same households rather than households in the same cluster.

4.10 Discussion and conclusions

This paper reports the impact on child schooling and work of the Government of Zambia's Child Grant Programme, an unconditional cash transfer programme targeted to households with children under 5 years of age in three of the most remote districts of the country. Although the CGP focus is on very young children, we look to see if nevertheless the programme has impacts on older children who are not the explicit target group. Indeed, since money is not tied to behaviour, impacts may be found in any sphere, depending on what are the major constraints facing households, and how the household itself believes the cash can best address its needs.

We use data from a large-scale social experiment involving 2,500 households, half of which were randomized out to a delayed-entry control group, that was implemented to assess the impact of

the programme in three districts in Zambia. We find that the CGP has no discernable impact on school attendance of children aged 7 to 14 after three years of implementation. However, when we break down the sample by older and younger children – based on the grade structure of the Zambian schooling system – we find a significant impact among older children aged 11 to 14 which coincides with the exact age range where sharp dropout begins to occur in Zambia with point estimates in the range of 7 to 8 percentage points, and half a day impact on regular attendance as measured by the number of days attended in the last week.

We further provide evidence on the potential pathways through which the unconditional cash transfer facilitates school attendance. Results show that households in the CGP spend more on education, and in particular on uniforms, items cited as key barriers to school enrolment and attendance in study areas. For older children another key barrier across the developing world is the time-cost of school attendance in terms of income foregone. The CGP has reduced work-for-pay among children aged 11 to 14 suggesting that alleviating the opportunity cost of schooling is another pathway through which the programme might facilitate school attendance.

Analyzing schooling impacts of the Child Grant Program with those on women's savings and non-farm enterprises, discussed in the previous chapter, is interesting. Reflecting on the timing (and magnitude) of these effects suggest that low-income households in the sample may need to first set aside money, little by little, and only when they have saved enough money, they might be able to invest in productive activities and in human capital. Households also seem to first invest in income-generating activities (i.e. non-farm enterprises) and then, as their economic situation stabilizes and improves, they invest in their children's schooling.

The fact that schooling impacts are only significant for older children may suggest that parents may be favouring and prioritizing investments in older children at the expense of younger ones. However, in line with the existing literature, the most plausible explanation would seem to be

that older children face a higher opportunity cost of schooling and there is therefore more margin to impact the schooling of these children.

Although there have been several published studies on the impact of CCTs on schooling, the literature on the schooling impacts of unconditional cash transfers is less abundant, though it is increasing, and evidence from government-run programmes in Africa is quite limited. How do the findings from this paper compare to existing evidence?

In terms of unconditional cash transfers, the Kenya CT-OVC Study Team (2012) report an impact of 8 percentage points on older children (12−17 years old), similar to the 7−8 points impact we report here among older children (11−14 years old). Hausofer and Shapiro (2016) do not find any impact on their educational index which combines enrolment and education expenditures, however they find a significant impact on education expenditures. Benhassine et al. (2015) find that in Morocco the labeled cash transfer programme had a 7.4 percentage impact on enrolment; Kilburn et al. (2017) find that children aged 6 to 17 years in beneficiary households are 12 percentage points more likely to be enrolled in school and that it is likely the mechanism works through an increase in education expenditures, and on notebooks and uniforms in particular.

Schooling impacts seem comparable to those found for CCTs in Latin America, in line with the systematic review of the education effects of cash transfers by Baird et al. (2014) which suggests CCTs and UCTs are equally effective.[129] However, comparing conditional and unconditional cash transfers is beyond the objectives of this paper and is not a strict apples-to-apples comparison for several reasons; conditionalities aside, cross-country comparisons of UCTs versus CCTs impacts are likely to confound overall levels of development, differences in demand[130] and supply-side

[129] Brauw and Hoddinott (2008) find a 7-point impact of Progresa on enrolment; Schultz (2004) reports an 8-point impact for the secondary school transition in Progresa; while Attanasio et al. (2010) find a 5−7 percentage points impact from the Colombian CCT Familias en Accion.

[130] Unlike the African context, in Latin America most children tend to finish primary school, whereas the main constraint is transition to and completion of secondary schooling.

constraints, differences in out-of-pocket costs associated with schooling, and values placed on education.

In this paper, we provide evidence that in Zambia cash-based assistance without any behavioural requirements related to education had a positive impact on schooling for older children not explicitly targeted by the programme.

Appendix C

Figure C.1: Map of Zambia

Figure C.2: Flowchart of study design

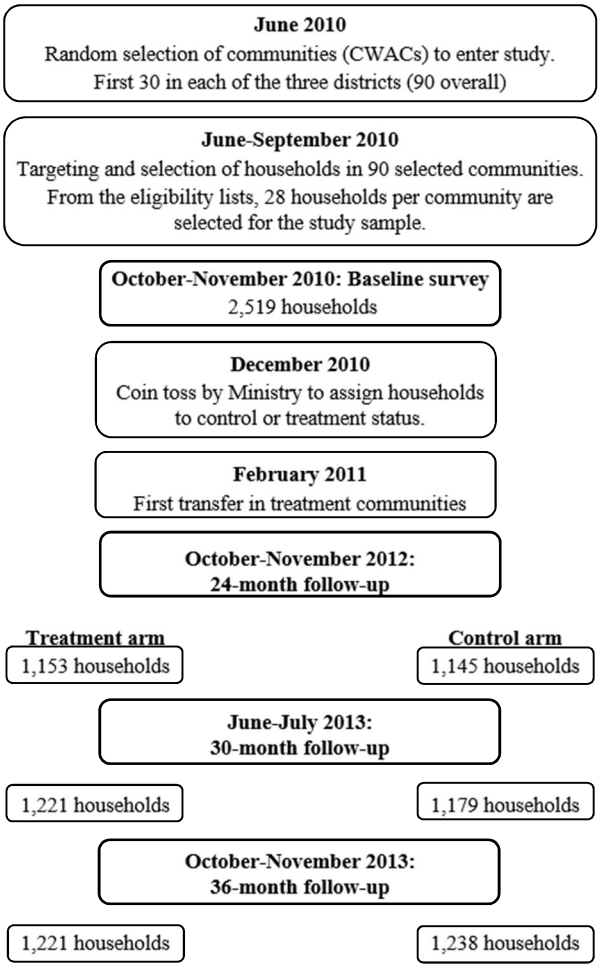


Table C.1: Household and individual level attrition

	Households		*Children 7−14 years*	
	N	Overall attrition (%)	N	Overall attrition (%)
Baseline	2,519		3,080	
2-year follow-up	2,298	9	2,532	18
3-year follow-up	2,459	2	2,689	13
Total	7,276		8,301	

Notes: Overall attrition is calculated as the difference between the number of children covered at baseline (3,080) and the number of children covered in each of the follow-ups (2,532 and 2,689 respectively) as a percentage of the number of children covered at baseline. Overall individual level attrition in the balanced panel would be around 23 per cent and down to 7 per cent in the unbalanced panel; individual level attrition does not significantly differ between control and treatment neither in the balanced nor in the unbalanced panel.

Table C.2: Testing individual differential attrition by baseline characteristics and key outcomes (using unbalanced panel)

	Control		Treatment		Difference Attritors	
	Attritors	Non-attritors	Attritors	Non-attritors	Col(1)-Col(3)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Child's age in years	10.31	9.97	10.67	10.06	-0.37	0.25
Child is female	0.51	0.50	0.52	0.49	-0.01	0.90
Recipient's age	36.32	35.12	35.16	35.28	1.16	0.50
Recipient never married	0.13	0.07	0.06	0.06	0.07	0.31
Recipient divorced or separated	0.06	0.13	0.09	0.08	-0.02	0.62
Recipient widowed	0.10	0.07	0.07	0.08	0.03	0.53
Recipient has ever attended school?	0.76	0.69	0.81	0.72	-0.05	0.49
Household size	8.06	7.08	7.33	7.38	0.74*	0.09
Number of members aged 0-5 years	2.06	1.96	1.87	1.98	0.19	0.25
Number of members aged 6-12 years	2.60	2.18	2.18	2.24	0.42*	0.08
Number of members aged 13-18 years	1.09	0.90	1.12	1.01	-0.02	0.92
Number of members aged 19-35 years	1.41	1.12	1.41	1.20	0.00	1.00
Number of members aged 36-55 years	0.62	0.83	0.64	0.84	-0.02	0.90
Number of members aged 56-69 years	0.20	0.07	0.12	0.08	0.08	0.42
Number of members aged 70+ years	0.08	0.02	0.00	0.03	0.08**	0.02
Shangombo district	0.21	0.32	0.15	0.30	0.06	0.56
Kaputa district	0.52	0.29	0.45	0.28	0.07	0.59
Outcome indicators						
Currently enrolled in school	0.78	0.74	0.71	0.74	0.06	0.28
Days in attendance in prior week	3.39	3.31	3.10	3.31	0.29	0.41
In past two weeks…						
Child is involved in paid or unpaid work	0.58	0.60	0.49	0.57	0.09	0.29
Child is involved in paid work	0.02	0.02	0.02	0.03	0.00	0.97
Child is involved in unpaid work	0.54	0.57	0.46	0.53	0.07	0.39
Hours in paid or unpaid work	10.65	12.72	8.92	13.00	1.73	0.50
Hours in paid work	0.08	0.16	0.16	0.35	-0.09	0.51
Hours in unpaid work	10.58	12.57	8.76	12.65	1.82	0.48
Indicators used for heterogeneous analysis						
Per capita consumption expenditure (ZMW)	32.55	32.45	35.53	34.53	-2.99	0.61
Education expenditures per child (ZMW)	19.38	14.46	16.42	12.09	2.96	0.68
Uniform expenditures per child (ZMW)	6.81	5.94	4.99	4.92	1.82	0.38
Distance to primary school in minutes*	27.66	33.15	28.67	31.24	-1.01	0.79
N children	108	1,434	113	1,425		
N households	71	763	79	756		

Notes: Overall N for control is 1,542 children (or 795 households). Overall N for treated is 1,538 children (or 795 households). ***

Table C.3: Impact of CGP on regular school attendance and child work among children aged 7−14 at baseline

	REGULAR ATTENDANCE (days attended in last week)		ANY WORK (hours in last 2 weeks)		PAID WORK (hours in last 2 weeks)		UNPAID WORK (hours in last 2 weeks)	
	(1) Unbalanced panel	(2) Balanced panel	(3) Unbalanced panel	(4) Balanced panel	(5) Unbalanced panel	(6) Balanced panel	(7) Unbalanced panel	(8) Balanced panel
24-month follow-up	0.365***	0.319***	-0.555	-0.588	0.155	0.141	-0.682	-0.706
	(0.101)	(0.111)	(2.057)	(2.106)	(0.172)	(0.185)	(2.048)	(2.095)
36-month follow-up	0.211	0.162	-0.489	-0.488	0.0702	0.0731	-0.557	-0.561
	(0.134)	(0.145)	(1.669)	(1.720)	(0.122)	(0.132)	(1.654)	(1.705)
Treated	-0.0570	-0.0167	0.0813	1.252	0.180	0.169	-0.0961	1.085
	(0.132)	(0.145)	(1.980)	(2.127)	(0.202)	(0.266)	(2.000)	(2.156)
Impact at 24 months	0.216	0.215	-0.864	-2.033	-0.437*	-0.421	-0.510	-1.686
	(0.150)	(0.164)	(2.719)	(2.841)	(0.243)	(0.294)	(2.723)	(2.848)
Impact at 36 months	0.317*	0.324*	-0.385	-1.548	-0.507**	-0.525*	0.123	-1.021
	(0.164)	(0.180)	(2.511)	(2.640)	(0.248)	(0.302)	(2.513)	(2.657)
N	8,084	6,908	8,061	6,896	8,061	6,896	8,082	6,914
R-squared	0.120	0.130	0.063	0.060	0.016	0.017	0.059	0.057
Baseline control mean	3.3	3.3	12.6	12.5	0.15	0.18	12.5	12.3

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Specifications are adjusted and include as controls: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).
Dependent variables: Regular attendance is measured as the number of days a child attended school in the prior week. Paid/unpaid work captures the number of hours worked in paid/unpaid work in the last two weeks. Any work captures the total number of hours worked by the children in the last two weeks (either paid or unpaid).
Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

Table C.4: Impact of CGP on school attendance and child work for children aged 7−14, jointly modelled

	SCHOOL ATTENDANCE (1/0)		ANY WORK (1/0)	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Balanced panel	Unbalanced panel	Balanced panel
24-month follow-up	0.0585***	0.235***	0.235***	0.239***
	(0.0173)	(0.0346)	(0.0346)	(0.0368)
36-month follow-up	0.0372**	0.282***	0.282***	0.287***
	(0.0184)	(0.0345)	(0.0345)	(0.0363)
Treated	-0.00862	-0.0478	-0.0478	-0.0209
	(0.0243)	(0.0438)	(0.0438)	(0.0449)
Impact at 24 months	0.0197	0.0646	0.0646	0.0437
	(0.0247)	(0.0479)	(0.0479)	(0.0496)
Impact at 36 months	0.0457*	0.0465	0.0465	0.0197
	(0.0246)	(0.0489)	(0.0489)	(0.0501)
N	8,105	8,105	8,105	6,934
Baseline control mean	0.74	0.60	0.60	0.60

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 The 'attendance' and 'any work' equations are estimated jointly using seemingly unrelated regression (SUR). Estimations are adjusted and include: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).

Dependent variables: School attendance is a binary variable equal to 1 if the child is currently attending school, 0 otherwise. Any work is a binary variable equal to 1 if the child is normally involved in any work, either paid or unpaid, including unpaid domestic work/chores.

Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

Table C.5: Impact of CGP on school regular attendance and time spent in child work among children age 7-10 year at baseline

	REGULAR ATTENDANCE (days in last week)		ANY WORK (hours in last 2 weeks)	
	(1) Unbalanced panel	(2) Balanced panel	(3) Unbalanced panel	(4) Balanced panel
24-month follow-up	0.414***	0.385**	-0.658	-1.570
	(0.126)	(0.148)	(1.775)	(1.878)
36-month follow-up	0.365**	0.352*	-0.571	-1.449
	(0.168)	(0.186)	(1.559)	(1.636)
Treated	-0.0315	-0.00840	0.302	1.013
	(0.174)	(0.182)	(1.734)	(1.911)
Impact at 24 months	0.169	0.198	-1.079	-1.643
	(0.186)	(0.197)	(2.330)	(2.495)
Impact at 36 months	0.246	0.259	-0.613	-1.354
	(0.205)	(0.216)	(2.139)	(2.313)
N	4,925	4,255	4,881	4,221
R-squared	0.161	0.176	0.056	0.055
Baseline control mean	2.9	2.9	10.0	10.4

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Estimations are adjusted and include: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).

Dependent variables: Regular attendance is measured as the number of days a child attended school in the prior week. Any work captures the total number of hours worked by the child in the last two weeks (either paid or unpaid).

Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

Table C.6: Impact of CGP on time spent on child work (total hours and hours on unpaid activities only) among children aged 11−14 at baseline

	ANY WORK (hours in last 2 weeks)		UNPAID WORK (hours in last 2 weeks)	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Balanced panel	Unbalanced panel	Balanced panel
24-month follow-up	-1.222	-0.157	-1.413	-0.340
	(2.863)	(2.959)	(2.775)	(2.865)
36-month follow-up	-1.185	-0.0371	-1.399	-0.255
	(2.180)	(2.258)	(2.150)	(2.224)
Treated	-0.0812	1.877	-0.389	1.616
	(2.633)	(2.756)	(2.679)	(2.823)
Impact at 24 months	-0.482	-2.691	0.0378	-2.204
	(3.617)	(3.727)	(3.589)	(3.711)
Impact at 36 months	-0.0968	-1.973	0.926	-0.932
	(3.348)	(3.392)	(3.351)	(3.442)
N	3,180	2,675	3,187	2,680
R-squared	0.029	0.026	0.026	0.022
Baseline control mean	16.6	16.0	16.3	15.6

Notes: Difference-in-differences linear probability estimates with robust standard errors clustered at the community level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Estimations are adjusted and include: child's age in years and child's age in years squared, whether the child is female (binary), recipient's age, recipient's marital status splines (never married, divorced or separated, widowed) where the omitted dummy is 'ever married or cohabitated', whether the recipient ever attended school (binary), household size (logged), number of members in different age groupings (aged 0-5 years, 6-12 years, 13-18 years, 19-35 years, 36-55 years, 56-69 years, 70+ years) and finally, district dummies (Shangombo and Kaputa, with the omitted district being Kalabo).
Dependent variables: Any work captures the total number of hours worked by the children in the last two weeks (either paid or unpaid). Unpaid work captures the number of hours worked in unpaid work in the last two weeks.
Impacts at 24 months and at 36 months correspond respectively β_{TR2} and β_{TR3}.

5 Conclusions

A growing number of studies examine the impacts of cash transfers in sub-Saharan Africa (SSA). In this thesis, I investigate the impacts of transfers in Karamoja, Uganda, on the intra-household allocation of time, the impacts of the Government of Zambia's Child Grant Programme (CGP) on women's cash savings as well as on child labour and schooling.

I explore the impacts of cash transfers in different contexts in SSA: in three of the poorest and most remote rural districts in Zambia and in post-conflict Karamoja, one of the poorest sub-regions of Uganda. Both programmes are unconditional cash transfers; the grants linked to Early Childhood Development (ECD), in Karamoja were implemented by UNICEF and the World Food Programme whereas the CGP in Zambia is run by the government. All three data analyses use experimental designs, which are often considered the strongest in terms of internal validity or the 'gold standard' for testing causal hypotheses. Longitudinal data from the CGP impact evaluation includes follow-ups across three and four years of implementation and therefore sheds light on medium-term impacts, whereas the Karamoja evaluation describes short-term impacts (18 months after baseline).

Exploiting the particular feature of unconditional cash transfers in SSA, the focus of each of the three chapters is on outcomes beyond the narrow primary objectives of the programmes and, partly as a consequence, on household members who are not the explicit target. Here I will present

key findings and contributions from the three cases, covering some issues that have received limited attention in the literature.

In Chapter 2, I investigate the impacts of transfers linked to ECD attendance on the intra-household allocation of time in Karamoja, Uganda. Approximately a year after the programme began, I find a positive impact on adults' participation in agriculture and on time spent on income-earning activities at a time – the lean season – in which they would have normally worked less; specifically, the cash transfer has enabled households to invest more in productive activities, and in agriculture. My findings also indicate a positive impact of the cash transfer on primary school-aged children's involvement in productive work – and agricultural activities in particular. These increases do not appear to crowd out investments in schooling, rather suggesting a decrease in leisure time. Overall, there was no impact on intra-household allocation when the transfer was paid as food. I cannot draw firm conclusions on the relative impacts of different modalities as it cannot be completely excluded that results for food transfers are driven by a lack of enforcement, rather than by their ineffectiveness.

The limitations include the lack of leisure data for children, the underreporting in time use data, and finally the fact that the survey is not an agricultural one and therefore lacks some information such as detailed agricultural production, sales, and productive investments. I am also unable to distinguish the type of activities that children undertake in agriculture which would shed light on whether they are involved in some type of hazardous work.

Chapter 3 investigates the impact of a cash transfer intervention on women's cash savings behaviour. We find the CGP has enabled poor women to save in cash even in the absence of inclusive financial systems; the programme increased not only the proportion of poor women who saved, but also the amount of their savings, up to four years after implementation of the programme. In addition, the increase in women's cash savings does not crowd out other traditional forms of household savings such as livestock.

We find the programme also led to changes in women's intra-household decision-making and increased household investment in non-farm enterprises, potentially creating a virtuous cycle of savings, investment activity and women's standing in the household. The findings suggest that unconditional cash transfers − without an explicit savings objective − can be considered, along with other financial instruments, as a way to promote savings and, importantly, to do so in a manner which, by targeting women, specifically raises their financial empowerment.

There are a number of limitations to this analysis. Since savings and the amounts saved are self-reported, there may be an incentive for respondents to underreport the sum involved, particularly if they keep the cash at home. However, this does not threaten the internal validity of the study, since we have no reason to suspect that control and treatment households would underreport savings at different rates. Also, the study sample is made up primarily of households with young children, and women who are targeted may not be comparable to the average poor population of women. We cannot provide a direct comparison between cash savings under the control of men or women, nor compare overall reported cash savings at household-level or individual-level, as the data for these instances was not collected.

Finally, in Chapter 4, although the CGP focus is on very young children, we examine whether the programme has impacts on the schooling and work of older children who are not the explicit target group. We find that the CGP had a significant impact on attendance (binary) among children aged 11−14 with point estimates in the range of 7−8 percentage points; this result is supported by a half day significant impact on regular attendance as measured by the number of days attended in the last week. This is the age range when sharp dropout begins to occur in Zambia. The CGP has also reduced work-for-pay among children aged 11−14. Finally, we provide evidence on the potential pathways through which the unconditional cash transfer could impact school attendance. Households in the CGP spend more on education, and in particular on uniforms, key barriers to school enrolment and attendance in the study areas.

Unfortunately, it is not possible to look at impacts for older children due to the demographic composition of the relatively young households targeted by the programme. Moreover, we were unable to distinguish between productive and reproductive work but only between paid and unpaid work.

Altogether, these three essays point to the importance of exploring the presence of unintended effects to provide a more complete picture of the consequences of CTs. Large-scale, broad-based programmes are capable of affecting multiple aspects of a beneficiary's life. The study of these second-order effects is particularly interesting in the context of unconditional cash transfers – where beneficiaries have the freedom to spend money as they wish according only to their preferences and the constraints they face. Each of the chapters shows that cash transfers might have large and positive unanticipated impacts: on the schooling and work of older children in the household who are not the explicit target of the intervention (Chapter 4); on women's financial empowerment (Chapter 3) and also more broadly on household productive activities (Chapter 2). A small but regular amount of cash can go a long way in improving a household's well-being. Also, among the potentially negative unintended impacts is an increase in the work load of older children described in Chapter 2, which however does not come at the expense of schooling. Such findings highlight the importance of continuing to assess the impacts of cash transfers including on the well-being of children not explicitly targeted, also to confirm that they suffer no detrimental impact.

Among potentially negative impacts of cash transfers, beyond an increase in child labour, there could be also a negative impact on intimate partner violence (IPV) linked to the fact that the women are the primary recipients of the cash transfer in both programs analyzed. Peterman et al. (2017) show that no significant effect on partner violence could be captured after four years of implementation of the CGP in Zambia, whereas in the Karamoja impact evaluation we cannot exclude this hypothesis as no partner violence measure was collected. However, a recent mixed-method review of cash transfers and intimate partner violence in low- and middle-income countries (Buller

et al. 2018) find little support for increases in IPV with only two studies out of twenty-two indicating mixed or negative impacts; on the other hand, sixteen of these twenty-two studies provide evidence that cash transfers reduce IPV. However, in order to conclude any particular cash transfer policy is effective, a full general equilibrium analysis encompassing all these potential effects would be required.

The cash transfer debate is currently focused on their long-term impacts. On the one hand, it is interesting to understand whether impacts accumulate over time or whether they remain stable or even decrease as households become accustomed to receiving the grant. On the other hand, we know very little about the durability of impacts after the payments end. A seven-year follow-up of the Zambian Child Grant Programme was collected in November−December 2017 once beneficiary households had stopped receiving the grant. It would be interesting for future research to understand whether the positive impacts highlighted in Chapters 3 and 4 persist once cash transfers end, or whether they dissipate over time. Do cash transfers permanently improve the financial and economic position of women? Do households continue to invest in children's education? More broadly, such research would help to understand whether cash transfers have the potential to strengthen households' well-being in a sustainable way and help them to "graduate" out of poverty.

In terms of policy implications, this thesis highlights the overall importance of exploring both intended and unintended impacts of interventions. Unanticipated impacts – whether desirable or not – are neither obvious nor straightforward. It is therefore paramount to explore these unexpected impacts – by going beyond the primary objectives and explicit target of the intervention – as these are likely to influence the long-term impacts and success of the programme as well as the design of future policies and data collection for future impact evaluations.

6 References

Abdulai, A., C. Barrett, and J. Hoddinott (2005). Does Food Aid Have Disincentive Effects? New Evidence from sub-Saharan Africa. *World Development,* 33(10): 1689–1704.

Ackah, C. (2013). Non-Farm Employment and Income in Rural Ghana. *Journal of International Development*, 25:325–339.

Adato, M., B. de la Brière, D. Mindek, and A. Quisumbing (2000). *The Impact of PROGRESA on Women's Status and Intrahousehold Relations*. Washington DC: International Food Policy Research Institute.

AIR American Institutes for Research (2011). Zambia's Child Grant Program: Baseline Report. Washington DC.

AIR American Institutes for Research (2013). Zambia's Child Grant Program: 24-month Impact Report. Washington DC.

AIR American Institutes for Research (2014). Zambia's Child Grant Program: 36-month Impact Report. Washington DC.

AIR American Institutes for Research (2016). Zambia's Child Grant Program: 48-month Impact Report. Washington DC.

Akresh, R., D. de Walque, and H. Kazianga, (2013). Cash Transfers and Child Schooling: Evidence from a Randomized Evaluation of the Role of Conditionality. *World Bank Policy Research Working Paper* 6340 Washington DC, World Bank.

Alderman, H., G.O. Gilligan, and K. Lehrer (2012). The Impact of Food for Education Programs on School Participation in Northern Uganda. *Economic Development and Cultural Change,* 61(1): 1870−218.

Alzúa, M.L., G. Cruces, and L. Ripani (2013). Welfare Programs and Labor Supply in Developing Countries: Experimental Evidence from Latin America. *Journal of Population Economics,* 264: 1255–1284.

Angelucci, M., O. Attanasio, and V. Di Maro (2012). The Impact of Oportunidades on Consumption, Savings and Transfers, *Fiscal Studies* 33(3): 305–334.

Angrist, J.D., and J.S. Pischke (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*, Chapter 3; Making Regression Make Sense. Princeton, NJ, Princeton University Press.

Asfaw, S., A. Carraro, B. Davis, S. Handa, and D. Seidenfeld (2017). Cash Transfer Programmes, Weather Shocks and Household Welfare: Evidence from a Randomised Experiment in Zambia. *Journal of Development Effectiveness*, 9(4), 419−442.

Ashraf, N. (2009). Spousal Control and Intra-Household Decision Making: An Experimental Study in the Philippines. *American Economic Review* 99(4): 1245−1277.

Ashraf, N., D. Karlan, and W. Yin (2006). Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines. *Quarterly Journal of Economics*, 121(2): 635−672.

Attanasio, O., E. Fitzsimons, A. Gomez, M.I. Gutiérrez, C. Meghir, and A. Mesnard (2010). Children's Schooling and Work in the Presence of a Conditional Cash Transfer Program in Rural Colombia. *Economic Development and Cultural Change,* 58 (2): 181–210.

Attanasio, O., and Mesnard, A. (2005). The Impact of a Conditional Cash Transfer Programme on Consumption in Colombia. *IFS Report Summary Familias* 02. London, Institute for Fiscal Studies.

Baird, S., F. Ferreira, B. Ozler, and M. Woolcock (2014). Conditional, Unconditional and Everything in between: A Systematic Review of the Effects of Cash Transfer Programmes on Schooling Outcomes. *Journal of Development Effectiveness*, 6(1): 1-43.

Baird, S., McIntosh, C., and B. Özler (2011). Cash or Condition? Evidence from a Randomized Cash Transfer Program. *Quarterly Journal of Economics*, 126(4): 1709-1753.

Baird, S.J., D.J. McKenzie, and B. Ozler (2018). The Effects of Cash Transfers on Adult Labor Market Outcomes. *Policy Research Working Paper,* 8404. Washington DC, World Bank.

Baland, J., C. Guirkinger, and C. Mali (2011). Pretending to Be Poor: Borrowing to Escape Forced Solidarity in Credit Cooperatives in Cameroon. *Economic Development and Cultural Change*, 60: 1–16.

Banerjee, A., R. Hanna, and G.E. Kreindler (2017). Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs. *World Bank Research Observer*, 32(2), 155-184.

Banerjee, A., D. Karlan and J. Zinman (2015). Six Randomized Evaluations of Microcredit: Introduction and Further Steps. *American Economic Journal: Applied Economics*, 7(1): 1-21.

Barrett, C. (2006). Food Aid's Intended and Unintended Consequences. *FAO Working Paper*, 06-05. Rome, Food and Agriculture Organization of the United Nations.

Barham, T. (2011). A Healthier Start: The Effect of Conditional Cash Transfers on Neonatal and Infant Mortality in Rural Mexico. *Journal of Development Economics*, 94(1): 74–85.

Barham, T., K. Macours, and J. Maluccio (2017). Are Conditional Cash Transfers Fulfilling Their Promise? Schooling, Learning, and Earnings after 10 Years. *CEPR Discussion Papers* 11937, Centre for Economic Policy Research.

Barrientos, A., and J.M. Villa (2015). Evaluating Antipoverty Programs in Latin America and sub-Saharan Africa: Better Policies? Better Politics? *Journal of Globalization and Development*, 6(1): 147–179.

Bastagli, F., J. Hagen-Zanker, L. Harman, V. Barca, G. Sturge, and T. Schmidt (2016). *Cash Transfers: What Does the Evidence Say? A Rigorous Review of Programme Impact and The Role of Design and Implementation Features*. London, Overseas Development Institute.

Basu, K., and P. Van (1998). The Economics of Child Labor. *The American Economic Review*, 88 (3): 412-427.

Benhassine, N., F. Devoto, E. Duflo, P. Dupas, and V. Pouliquen (2015). Turning a Shove into a Nudge? A "Labeled Cash Transfer" for Education. *American Economic Journal: Economic Policy,* 7(3): 86-125.

Bianchi, M., and M. Bobba (2013). Liquidity, Risk, and Occupational Choices. *Review of Economic Studies*, 80 (2): 491–511.

Blattman, C., E.P. Green, J. Jamison, M.C. Lehmann, and J. Annan (2016). The Returns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda. *American Economic Journal: Applied Economics*, 8(2): 35-64.

Bonilla, J., R.C. Zarzur, S. Handa, C. Nowlin, A. Peterman, H. Ring, and D. Seidenfeld (2017). Cash for Women's Empowerment? A Mixed-Methods Evaluation of the Government of Zambia's Child Grant Program. *World Development*, 95: 55-72.

Boone, R., K. Covarrubias, B. Davis, and P. Winters (2013). Cash Transfer Programs and Agricultural Production: The Case of Malawi. *Agricultural Economics*, 44(3): 365–378.

de Brauw, A., D.O. Gilligan, J. Hoddinott, and S. Roy (2014). The Impact of Bolsa Família on Women's Decision-Making Power. *World Development,* 59: 487-504.

de Brauw, A., and J. Hoddinott (2008). Must Conditional Cash Transfer Programs Be Conditioned to Be Effective? The Impact of Conditioning Transfers on School Enrollment in Mexico. *IFPRI Discussion Paper* 00757. Washington DC, International Food Policy Research Institute.

Buller A.M., A. Peterman, M. Ranganathan, A. Bleile, M. Hidrobo and L. Heise (2018). A mixed method review of cash transfers and intimate partner violence in low and middle income countries. *World Bank Research Observer*, Volume 33, Issue 2, 1 August 2018, Pages 218–258.

Burbidge, J.B, L. Magee, and A.L. Robb (1988). Alternative Transformations to Handle Extreme Values of the Dependent Variable. *Journal of the American Statistical Association*, 83(401): 123-127.

Canagarajah, S., C. Newman, and R. Bhattamishra (2001). Non-Farm Income, Gender, and Inequality: Evidence from Rural Ghana and Uganda. *Food Policy*, 26(4): 405–420.

Canavire-Bacarreza, G., and M., Ospina (2015). Intrahousehold Time Allocation: An Impact Evaluation of Conditional Cash Transfer Programs. *Documentos de Trabajo CIEF.* 013481, Universidad EAFIT.

Czuba, K. (2012). Income Generating Activities and Savings Behaviour of Adolescent Girls and Young Women in Karamoja. BRAC Research and Evaluation Unit Report.

Daidone, S., B. Davis, J. Dewbre, and K. Covarrubias (2014a). Lesotho's Child Grant Programme: 24-month Impact Report on Productive Activities and Labour Allocation. Lesotho Country Case Study Report. Rome, Food and Agriculture Organization of the United Nations.

Daidone, S., B. Davis, J. Dewbre, M. González-Flores, S. Handa, D. Seidenfeld, and G. Tembo (2014b) Zambia's Child Grant Programme: 24-month Impact Report on Productive Activities and Labour Allocation. Rome, Food and Agriculture Organization of the United Nations.

Dammert, A. (2009). Heterogeneous Impacts of Conditional Cash Transfers: Evidence from Nicaragua. *Economic Development and Cultural Change*, 58(1): 53–83.

Dammert, A.C., 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: 104-123.

Davis, B., S. Handa, N. Hypher, N. Winder-Rossi, P. Winters, and J. Yablonski (2016). From Evidence to Action: The Story of Cash Transfers and Impact Evaluation in Sub-Saharan Africa. Oxford University Press.

Davis, B., and S. Handa (2015). How Much do Programmes Pay? Transfer Size in Selected National Cash Transfer Programmes in Africa. *Innocenti Research Brief*, 2015-09. UNICEF Innocenti Research Centre.

Dayıoğlu, M. (2013). Impact of Unpaid Household Services on the Measurement of Child Labour. *MICS Methodological Papers*, No. 2. Statistics and Monitoring Section, Division of Policy and Strategy, New York, UNICEF.

Duflo, E., M. Kremer, and J. Robinson (2006). Why Don't Farmers use Fertilizer: Evidence from Field Experiments. Unpublished paper, MIT.

Dupas, P., and J. Robinson (2013). Why Don't the Poor Save More? Evidence from Health Savings Experiments. *American Economic Review*, 103(4): 1138-1171.

Duvendack, M., R. Palmer-Jones, J.G. Copestake, L. Hooper, Y. Loke, and N. Rao (2011). What Is the Evidence of the Impact of Microfinance on the Well-being of Poor People? London, EPPI Centre, Social Science Research Unit, Institute of Education, University of London.

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

Edmonds. E.V., and N. Schady (2012). Poverty Alleviation and Child Labor. *American Economic Journal: Economic Policy*, vol. 4(4): 100-124, American Economic Association.

Fafchamps, M., and S. Quinn (2015) Aspire. NBER Working Paper 21084, National Bureau of Economic Research, Inc.

Faye, M., P. Niehaus, and C. Blattman (2015). Worth Every Cent, To Help the Poor, Give Them Cash. *Foreign Affairs*, October 2015. Available at: https://www.foreignaffairs.com/articles/2015-10-12/worth-every-cent

FEWS NET (2016). Karamoja, Uganda. Enhanced market analysis 2016. Washington DC, FEWS NET.

Filmer, D. (2004). 'If You Build It, Will They Come?' School Availability and School Enrollment in 21 Poor Countries. *World Bank Policy Research Working Paper*, Washington DC, World Bank.

Fiszbein, A., and N. Schady (2009). Conditional Cash Transfers: Reducing Present and Future. *Policy Research Report*. Washington DC, World Bank.

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

Gaarder, M. (2012). Conditional versus Unconditional Cash: A Commentary. *Journal of Development Effectiveness*, 4(1): 130–133.

Gahvari, F. (1994). In-kind Transfers, Cash Grants and Labor Supply. *Journal of Public Economics*, Vol. 55: 4950−504.

García, M., and C. Moore (2012). The Cash Dividend: The Rise of Cash Transfer Programs in Sub-Saharan Africa. *World Bank Report no. 67208*. Washington DC, World Bank.

Garcia, 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): 921-965.

Gayfer, J., J. Barnes, M. Jennings and A. Kayondo (2012). Formative Evaluation of World Food Programme's Livelihoods Programme, Karamoja, Uganda. Department for International Development (DFID) Report.

Gentilini, U. (2014). Our Daily Bread: What is the Evidence on Comparing Cash versus Food Transfers? *Social Protection and Labor Discussion Paper* n.1420. Washington DC, World Bank.

Gertler, P., S.W. Martinez, and M. Rubio-Codina (2012). Investing Cash Transfers to Raise Long-Term Living Standards. *American Economics Journal: Applied Economics*, 4(1): 164-92.

Gertler, P. (2004). Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment. *The American Economic Review*, 94(2): 336–341.

Gilligan, D., J. Hoddinott, V. Moreira, A. Peterman, and S. Roy (2011). WFP/UNICEF/IFPRI Impact Evaluation of Cash and Food Transfers at Early Childhood Development Centers in Karamoja, Uganda: Baseline Report. Submitted to: World Food Programme, Rome; World Food Programme, Kampala; and UNICEF, Kampala, July 2011.

Gilligan, D., A. Margolies, E. Quiñones, and S. Roy (2013). WFP/UNICEF/IFPRI Impact Evaluation of Cash and Food Transfers at Early Childhood Development Centers in Karamoja, Uganda: Final Impact Report. Submitted to: World Food Programme, Rome; World Food Programme, Kampala; and UNICEF, Kampala, May 2013.

Gilligan, D.O., M. Hidrobo, J. Hoddinott, S. Roy, and B. Schwab (2014). Much Ado about Modalities: Multicountry Experiments on the Effects of Cash and Food Transfers on Consumption Patterns. *IFPRI Conference Paper,* International Food Policy Research Institute.

Gilligan, D. O. and S. Roy (2015). Resources, Stimulation, and Cognition: How Transfer Programs and Preschool Shape Cognitive Development in Uganda, *IFPRI Conference Paper*, International Food Policy Research Institute.

Gitter, S.R., and B.L. Barham (2008). Women's Power, Conditional Cash Transfers, and Schooling in Nicaragua. *World Bank Economic Review* 22(2): 271–290. doi:10.1093/wber/lhn006

Glewwe, P., and A.L. Kassouf (2012). The Impact of the Bolsa Escola/Familia Conditional Cash Transfer Program on Enrollment, Dropout Rates and Grade Promotion in Brazil. *Journal of Development Economics*, vol. 97(2): 505-517.

de Groot, R., T. Palermo, S. Handa, L.P. Ragno, and A. Peterman (2017). Cash Transfers and Child Nutrition: Pathways and Impacts. *Development Policy Review*, 35(5): 621-643.

Gul, F., and W. Pesendorfer (2001). Temptation and Self-Control. *Econometrica,* 69(6): 1403-1435.

Gul, F., and W. Pesendorfer (2004). Self-Control and the Theory of Consumption. *Econometrica,* 72(1): 119-158.

Handa, S., L. Natali, D. Seidenfeld, G. Tembo, and B. Davis (2018). Can Unconditional Cash Transfers Raise Long-term Living Standards? Evidence from Zambia. *Journal of Development Economic*s, Vol.133: 42-65.

Handa, S., S. Daidone, A. Peterman, B. Davis, A. Pereira, T. Palermo, and J. Yablonski (2017). Myth-busting? Confronting Six Common Perceptions about Unconditional Cash Transfers as a Poverty Reduction Strategy in Africa. *Innocenti Working Paper* 2017-11, UNICEF Office of Research.

Handa, S., D. Seidenfeld, B. Davis, G. Tembo, and the Zambia Cash Transfer Evaluation Team (2016a). The Social and Productive Impacts of Zambia's Child Grant. *Journal of Policy Analysis and Management,* 365(2): 357-387.

Handa, S., L. Natali, D. Seidenfeld, G. Tembo, and the Zambia Cash Transfer Evaluation Team (2016b). 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): 346-367.

Handa, S., A. Peterman, D. Seidenfeld, and G. Tembo (2016c). Income Transfers and Maternal Health: Evidence from a National Randomized Social Cash Transfer Program in Zambia. *Health Economics*, 25(2): 225-236.

Handa, S., B. Martorano, C.T. Halpern, A. Pettifor, and H. Thirumurthy (2016d). Time Discounting and Credit Market Access in a Large-Scale Cash Transfer Program, *Journal of African Economies*, 25(3): 367-387.

Handa, S. (2002). Raising Primary School Enrolment in Developing Countries: The Relative Importance of Supply and Demand. *Journal of Development Economics*, 69(1): 103-128.

Hanlon, J., A. Barrientos, and D. Hulme (2010). *Just Give Money to the Poor: The Development Revolution from the Global South*. Sterling, Kumarian Press.

Harmer, A. (2012). Cash and Voucher for Work in Food Security Programmes. Moroto, Uganda, GIZ.

Haushofer, J., and J. Shapiro (2016). The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya. *Quarterly Journal of Economics,* 131 (4): 1973–2042.

Hidrobo, M., A. Peterman, and L. Heise (2016). The Effect of Cash, Vouchers, and Food Transfers on Intimate Partner Violence: Evidence from a Randomized Experiment in Northern Ecuador. *American Economic Journal: Applied Economics,* 8(3): 284−303.

Hjelm, L., S. Handa, J. de Hoop, and T. Palermo (2017). Poverty and Perceived Stress: Evidence from Two Unconditional Cash Transfer Programs in Zambia. *Social Science & Medicine*, 177: 110−117.

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

de Hoop, J. and F.C. Rosati (2014b) Does Promoting School Attendance Reduce Child Labour? Evidence from Burkina Faso's BRIGHT Project. *Economics of Education Review,* 39: 78−96.

Hulme, D., and T. Arun (eds.) (2009). *Microfinance: A Reader.* Abingdon, Routledge Press.

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): 349–373.

Karlan, D., R. Osei, I. Osei-Akoto, and C. Udry (2014). Agricultural Decisions after Relaxing Credit and Risk Constraints, *Quarterly Journal of Economics*, 129(2): 597–652.

Karlan D, A.L. Ratan, and J. Zinman (2014). Savings By and For the Poor: A Research Review and Agenda. *Review of Income and Wealth,* 60(1): 36–78.

Kazianga, H., D. de Walque, and H. Alderman (2012). Educational and Child Labour Impacts of Two Food-for-Education Schemes: Evidence from a Randomised Trial in Rural Burkina Faso. *Journal of African Economies*, 21(5): 723–760.

Kenya CT-OVC Evaluation Team (2012). The Impact of Kenya's Cash Transfer for Orphans and Vulnerable Children on Human Capital. *Journal of Development Effectiveness*, 4(1): 38–49.

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: 63−80.

Kilingsworth, M. (1983). *Labor Supply*. Cambridge University Press.

Laibson, D. (1997). Golden Eggs and Hyperbolic Discounting. *Quarterly Journal of Economics,* 112(2): 443-477.

Lawlor, K., S. Handa, D. Seidenfeld, and The Zambia Cash Transfer Evaluation Team (2015). Cash Transfers and Climate Resilient Development: Evidence from Zambia's Child Grant Programme. *Innocenti Working Paper*, 2015-03. Florence: UNICEF Office of Research−Innocenti.

Leonesio, M. (1988) In-Kind Transfers and Work Incentives. *Journal of Labor Economics*, 6(4): 515-529.

Löwe, A., and S. Phiona, (2017). Creating Opportunities for Young People in Northern Uganda's Agriculture Sector. London, Overseas Development Institute.

Manley, J., S. Gitter, and V. Slavchevska (2013). How Effective Are Cash Transfers at Improving Nutritional Status? *World Development*, 48: 133-155.

Margolies, A., and J. Hoddinott (2012). Mapping the Impacts of Food Aid: Current Knowledge and Future Directions. *WIDER Working Paper*, No. 2012/34.

McKenzie, D. (2012). Beyond Baseline and Follow-up: The Case for More T in Experiments. *Journal of Development Economics* 99: 210-221.

Miller, C., M. Tsoka, and K. Reichert (2010). Impacts on Children of Cash Transfers in Malawi. In: S. Handa, S. Devereux and D. Webb, eds. *Social Protection for Africa's Children*. London, Routledge Press, pp. 96–116.

Musonda, B., and A. Kaba (2011). The SACMEQ III Project in Zambia: A Study of the Conditions of Schooling and the Quality of Education. Gabarone, SACMEQ. Available at: http://www.sacmeq.org/sites/default/files/sacmeq/reports/sacmeq-iii/national-reports/s3_zambia_final.pdf online.

Nagler, P., and W. Naudé, (2014). Non-Farm Entrepreneurship in Rural Africa: Patterns and Determinants. *IZA Discussion Paper* No. 8008.

Newson, R. (2003). Stata tip 1: The eform() option of regress. Stata Journal 3: 445.

Njuki, J., and P. Sanginga (2013). *Women, Livestock Ownership and Markets Bridging the Gender Gap in Eastern and Southern Africa*. London, Routledge.

Novella, R., L. Ripani, G. Cruces, and M.L. Alzúa (2012). Conditional Cash Transfers, Female Bargaining Power and Parental Labour Supply, *IDB Working Paper Series*, IDB-WP-368.

Orazem, P.F., P. Glewwe, and H. Patrinos, H. (2008). The Challenge of Education. Prepared for Copenaghen Consensus 2008. Available at: http://www.copenhagenconsensus.com/sites/default/files/cp_educationcc08vol2.pdf

Pace, N., S. Daidone, B. Davis, and L. Pellerano (2016). Does "Soft Conditionality" Increase the Impact of Cash Transfers on Desired Outcomes? Evidence from a Randomized Control Trial in Lesotho. *Working Papers* 2016:33, Department of Economics, University of Venice Ca' Foscari.

Palermo, T., S. Handa, A. Peterman, and L. Prencipe, on behalf of the Zambia CGP Evaluation Team (2016). Unconditional Government Social Cash Transfer in Africa Does Not Increase Fertility. *Journal of Population Economics,* 29(4), 1083-1111.

Pande, R., S. Cole, A. Sivasankaran, G. Bastian, and K. Durlacher (2012). Does Poor People's Access to Formal Banking Services Raise Their Incomes? EPPI-Centre, Social Science Research Unit, Institute of Education, University of London.

Parker, S.W., and E. Skoufias (2000). The Impact of PROGRESA on Work, Leisure, and Time Allocation. *IFPRI Report*. Washington DC, International Food Policy Research Institute.

Paxson, C., and N. Schady (2010). Does Money Matter? The Effects of Cash Transfers on Child Development in Rural Ecuador. *Economic Development and Cultural Change* 59(1): 187–229.

Paxson, C., and N. Schady (2007). Does Money Matter? The Effects of Cash Transfers on Child Health and Development in Rural Ecuador. *Policy Research Working Paper Series* 4226, Washington DC, World Bank.

Peterman, A., T. M. Palermo, S. Handa, and D. Seidenfeld. (2017). List Randomization for Soliciting Experience of Intimate Partner Violence: Application to the Evaluation of Zambia's Unconditional Child Grant Program. *Health Economics* 27(3):622–8.

Peterman, A., S. Roy, B. Schwab, M. Hidrobo, and D. Gilligan (2015). Measuring Women's Decision Making: Indicator Choice and Survey Design Experiments from Cash and Food Transfer Evaluations in Ecuador, Uganda and Yemen. *IFPRI Discussion Paper,* Washington DC, International Food Policy Research Institute.

Platteau, J.P. (2000). *Institutions, Social Norms, and Economic Development*. Abingdon, Routledge Press.

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): 433-442.

Ravallion, M., and Q. Wodon (2000). Does Child Labour Displace Schooling? Evidence on Behavioral Responses to an Enrollment Subsidy. *Economic Journal,* 110: C158–C175.

Resilience Analysis Unit (RAU) (2015). Resilience Context Analysis Resilience to Food Insecurity and Malnutrition in Karamoja, Uganda. Republic of Djibouti, Intergovernmental Authority on Development.

Riddell, A. (2003). The Introduction of Free Primary Education in sub-Saharan Africa. *Background paper for EFA Global Monitoring Report 2003*, 4.

Rijkers, B., and R. Costa (2012). Gender and Rural Non-Farm Entrepreneurship. *World Development*, 40 (12): 2411–2426.

Roelen, K., S. Devereux, A.J. Abdulai, B. Martorano, T. Palermo, and P. Ragno (2017). How to Make 'Cash Plus' Work: Linking Cash Transfers to Services and Sectors. *Innocenti Working Paper,* 2017-10, UNICEF, Innocenti Office of Research.

Rubalcava, L., G. Teruel, and D. Thomas (2009). Investments, Time Preferences, and Public Transfers Paid to Women. *Economic Development and Cultural Change*, 57(3): 507-538.

Sadoulet, E., F. Finan, A. De Janvry, and R. Vakis (2004). Can Conditional Cash Transfer Programs Improve Social Risk Management? Lessons for Education and Child Labor Outcomes. *SP Discussion Papers* no. 0420, International Bank for Reconstruction and Development.

Samson, M., C. Heinrich, S. Kaniki, F. Regalia, K. Mac Querie, Muzondo, T., I. van Niekerk and M. Williams (2010). Impacts of South Africa's Child Support Grants. In: S. Handa, S. Devereux and D. Webb, eds. *Social Protection for Africa's Children*. London, Routledge Press, pp. 117–145.

Schady, N., and M.C. Araujo (2006). Cash Transfers, Conditions, School Enrollment, and Child Work: Evidence from a Randomized Experiment in Ecuador. Washington DC, World Bank.

Schubert, B., and R. Slater (2006). Social Cash Transfers in Low-income African Countries: Conditional or Unconditional? *Development Policy Review*, 24(5): 571–578.

Schultz, T.P. (2004). School Subsidies for the Poor: Evaluating the Mexican Progresa Poverty Program. *Journal of Development Economics*, vol. 74(1): 199-250.

Seidenfeld, D., S. Handa, G. Tembo, M. Stanfield, C. Harland-Scott, and L. Prencipe (2014a). The Impact of an Unconditional Cash Transfer on Food Security and Nutrition: The Zambian Child Grant Program, IDS Special Bulletin on Child Nutrition in Zambia.

Seidenfeld, D., and S. Handa (2011). Results of the Three-year Impact Evaluation of Zambia's Cash Transfer Program in Monze District. Final Report. Washington DC, American Institutes for Research.

Skoufias, E., S.W. Parker, J.R. Behrman, and C. Pessino (2001). Conditional Cash Transfers and Their Impact on Child Work and Schooling: Evidence from the PROGRESA Program in Mexico [with Comments] *Economia* 2(1): 45-96.

Skoufias, E., M. Unar, and T. Gonzalez de Cossio (2013). The Poverty Impacts of Cash and In-kind Transfers: Experimental Evidence from Rural Mexico. *Journal of Development Effectiveness*, 5(4): 401-429.

SOFA Team, and C. Doss (2011). The Role of Women in Agriculture. *ESA Working Paper* No. 11-02, Agricultural Development Economics Division, Food and Agriculture Organization.

Southworth, H.M. (1945). The Economics of Public Measures to Subsidize Food Consumption. *Journal of Farm Economics*, 27(1): 38-66.

Stefansky Huisman, C. (2011). 'Once Patriarchs and Warriors: Masculinity and Modernity in Karamoja, Uganda'. *Praxis: The Fletcher Journal of Human Security*, XXVI: 60-80.

Stewart, R., C. van Rooyen, K. Dickson, M. Majoro, and T. de Wet (2010). What is the Impact of Microfinance on Poor People? A Systematic Review of Evidence from sub-Saharan Africa. *Technical Report*. London, EPPI-Centre, Social Science Research Unit, University of London.

Stewart, R., C. van Rooyen, and T. de Wet (2012). Do Micro-credit, Micro-savings and Micro-leasing Serve as Effective Financial Inclusion Interventions Enabling Poor People, and Especially Women, To Engage in Meaningful Economic Opportunities in LMICs? London: EPPI-Centre, Social Science Research Unit, Institute of Education, University of London.

Stites, E., A. Darlington, M. Dyan, and A. Priscillar (2007). Angering Akuju: Survival and Suffering in Karamoja. A Report on Livelihoods and Human Security in the Karamoja Region of Uganda. Medford, MA, Feinstein International Center, Tufts University.

Stoeffler, Q., and B. Mills (2014). Households' Investments in Durable and Productive Assets in Niger: Quasi-experimental Evidence from a Cash Transfer Project. Paper presented at AAEA Annual Meetings, July 27−29, 2014, Minneapolis, MN.

Thaler, R., and S. Benartzi (2004). Save More Tomorrow: Using Behavioral Economics To Increase Employee Saving. *Journal of Political Economy*, 112(S1): S164-S187.

Thome, K., J. Taylor, B. Davis, S. Handa, D. Seidenfeld, and G. Tembo (2014). Local Economy-Wide Impact Evaluation (LEWIE) of Zambia's Child Grant Programme. *PtoP Project Report*, Food and Agriculture Organization; UNICEF; World Bank.

UBOS (Uganda Bureau of Statistics) (2010). Uganda National Household Survey 2009/2010. Kampala, Uganda Bureau of Statistics.

UBOS (Uganda Bureau of Statistics) and ICF International Inc. (2012). Uganda Demographic and Health Survey 2011. Kampala, Uganda Bureau of Statistics; Calverton, Maryland, ICF International Inc.

UBOS (Uganda Bureau of Statistics) (2014). Uganda National Household Survey 2012/2013. Kampala, Uganda Bureau of Statistics.

UBOS (Uganda Bureau of Statistics) and UCW (Understanding Children's Work) (2014). Understanding Children's Work and Youth Employment Outcomes in Uganda, Kampala. Available at: http://documents.worldbank.org/curated/en/229701467988904450/pdf/103603-v2-WP-PUBLIC-Uganda-report-child-labor-youth-employment20141016-154929.pdf

Vaessen, J., A. Rivas, M. Duvendack, R. Palmer-Jones, F.L. Leeuw, G. van Gils, R. Lukach, N. Holvoet, J. Bastiaensen, J.G. Hombrados, and H. Waddington (2014). The Effects of Microcredit on Women's Control over Household Spending in Developing Countries: A Systematic Review and Meta-analysis. *Campbell Systematic Reviews*.

van den Bold, M., A. Quisumbing, and S. Gillespie (2013). Women's Empowerment and Nutrition: An Evidence Review. *IFPRI Discussion Paper* 01294. Washington DC, International Food Policy Research Institute.

Yoong, J., L. Rabinovich, and S. Diepeveen (2012). The Impact of Economic Resource Transfers to Women versus Men: A Systematic Review. *Technical report*. London, EPPI-Centre, Social Research Unit, Institute of Education, University of London.

World Bank (2015). The State of Social Safety Nets 2015. Washington DC, World Bank.