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Are Cash Transfers a Silver Bullet? Evidence from the Zambian Child Grant

Sudhanshu Handa, David Seidenfeld, Benjamin Davis, Gelson Tembo and the Zambia Cash Transfer Evaluation Team

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Correspondence should be addressed to:

UNICEF Office of Research - Innocenti
Piazza SS. Annunziata, 12
50122 Florence, Italy
Tel: (+39) 055 20 330
Fax: (+39) 055 2033 220
florence@unicef.org
www.unicef-irc.org

ACRONYMS AND ABBREVIATIONS

AIR	American Institutes for Research
ARI	acute respiratory illness
CGP	Child Grant Programme
CT(S)	cash transfer(s)
CT-OVC	cash transfer for orphans and vulnerable children
CWAC	community welfare assistance committee
DD	difference in differences
ECD	early child development
FANTA	Food and Nutrition Technical Assistance
IYCF	infant and young child feeding
MCDMCH	Ministry of Community Development, Mother and Child Health
MICS	Multiple Indicators Cluster Survey
ORS	oral rehydration salts
RCT	randomized controlled trial
SD	standard deviation
T / C groups	treatment / control groups
ZMW	Zambian kwacha currency

ARE CASH TRANSFERS A SILVER BULLET? EVIDENCE FROM THE ZAMBIAN CHILD GRANT

Sudhanshu Handa, David Seidenfeld, Benjamin Davis, Gelson Tembo and the Zambia Cash Transfer Evaluation Team[1]

Abstract. Accumulated evidence from dozens of cash transfer programmes across the world suggest that there are few interventions that can match the range of impacts and cost-effectiveness of a small, predictable monetary transfer to poor families in developing countries. These results lead many policymakers to consider cash transfer programmes the 'gold-standard' in anti-poverty policy with some even advocating for benchmarking all development interventions against what would have been accomplished with a direct cash transfer. However, the benchmarking argument rests on the accumulated evidence from many programmes that highlights the range of potential benefits of cash transfers, while each individual study typically focused on only one programme and one outcome. This article is the first to provide comprehensive impact results of an unconditional cash transfer from one programme, covering many outcomes in poverty, social and economic domains. We implement an experimental design to evaluate the Zambian Government's Child Grant, an unconditional cash transfer to families with small children in three of the poorest districts of Zambia. We document the broad impacts of the programme, including on consumption, livelihood strengthening, material welfare of children, young child feeding, investment in assets, productive activities and housing after two years, making this one of the first studies to demonstrate both protective and productive impacts of a national unconditional cash transfer programme. However impacts in areas such as child nutritional status and schooling depend on initial conditions of the household, suggesting that cash alone is not enough to solve all constraints faced by these poor, rural households. Even an unconditional cash transfer programme with a wide range of impacts does not produce effects for all outcomes, suggesting that complementary programmes to achieve specific outcomes will still be necessary even in the most successful cases.

Keywords: Zambia, cash transfers, children, RCTs

JEL classification: I32, I38, J13

[1] The impact evaluation of the Zambian Child Grant and Multiple Categorical Targeted Programs are being implemented by the American Institutes for Research and the University of North Carolina under contract to UNICEF-Zambia. The evaluation is commissioned by the Ministry of Community Development, Mother and Child Health, Government of Zambia, with support from DFID, Irish Aid, and UNICEF-Zambia. The results that appear in this article are the culmination of over three years of intellectual, technical, financial and operational efforts of a large and dedicated team, all of whom made important contributions that led to the success of the evaluation. The corresponding authors for this article are Sudhanshu Handa (shanda@unicef.org) and David Seidenfeld (dseidenfeld@air.org). The members of the evaluation team, listed by affiliation and then alphabetically within affiliation are: American Institutes of Research (Juan Bonilla, Cassandra Jesse, Leah Prencipe, David Seidenfeld); FAO (Benjamin Davis, Josh Dewbre, Silvio Diadone, Mario Gonzalez-Flores); UNICEF-Zambia (Charlotte Harland Scott, Paul Quarles van Ufford); Government of Zambia (Vandras Luywa, Stanfield Michelo); DFID-Zambia (Kelley Toole); Palm Associates (Alefa Banda, Liseteli Ndiyoi, Gelson Tembo, Nathan Tembo); University of North Carolina (Sudhanshu Handa, Amber Peterman).

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1 INTRODUCTION

The excitement about cash transfer programmes as the silver bullet to alleviate poverty, combined with the abundant evidence demonstrating their impact, motivates a burgeoning movement to use cash transfers as the benchmark or gold standard for assessing and comparing all other poverty programmes. Since the ground-breaking experiences of Progresa (Mexico), Bolsa Familia (Brasil) and the Child Support Grant (South Africa) in the mid-1990s, literally dozens of developing countries have made direct cash payments to poor families part of their economic development strategy. This explosion of cash transfers (CTs), dubbed 'the quiet revolution' (Barrientos & Hulme 2008), has been accompanied by an equally impressive initiative to evaluate their impact using a range of sophisticated designs, including social experiments (World Bank, 2009; Davis et al, 2014). Consequently, there is unprecedented accumulated evidence on the potential impacts of these programmes.

Evidence based on independent studies from different programmes across the world demonstrates that cash transfers can have an impact on a wide range of development domains. A recent review by Baird et al. indicates for example that both conditional and unconditional cash transfers have significant impacts on children's schooling. Beyond schooling, impacts of national cash transfer programmes, whether conditional or unconditional, have been reported for consumption (Kenya CT-OVC Evaluation Team 2012; Hoddinott & Skoufias 2004 for Mexico), children's health (Luseno et al. 2012 for Malawi), intra-household decision-making (Handa et al. 2009 for Mexico), child nutrition (Behrman & Hoddinott 2005 for Mexico), asset accumulation and economic productivity (Covarrubias et al. 2012 for Malawi), and even HIV prevention (Handa et al. 2014 for Kenya; Cluver et al. 2013 for South Africa). Reviews of the evidence based primarily on Latin America can be found in Handa & Davis (2006) and World Bank (2009).[2]

But does this evidence mean that cash transfers are the silver bullet or best solution to alleviating poverty? Moreover, the benchmarking strategy might produce unrealistic standards regarding the impacts that a single programme can produce because the argument hinges on a literature that limits reporting of results to one specific indicator or topical area, and non-significant results are rarely put up for peer review. The impact of any particular programme will depend on design and implementation features such as whether it is conditional on a specific outcome, the target group, the level of transfer, the frequency and regularity of payment, and the constraints and institutional systems facing the household. This last point, that impact of a specific programme in a particular country depends on other factors, has not been fully appreciated by those who propose to benchmark all development aid against cash transfers. The movement towards benchmarking and designating CTs as the 'gold-standard' must distinguish between the evidence on the extensive benefits of cash transfers across a range of programme parameters and contexts, versus the benefits of a particular programme in a specific context.

[2] There is also a small cottage industry of published work based on small scale cash transfer experiments from Africa (e.g. Baird et al. 2011) but these do not reflect design parameters and other complexities of implementing actual national programmes and so their generalizability (or external validity) is unclear.

In this paper we present comprehensive results from a rigorous evaluation of what we believe is a generic cash transfer programme that fulfills many of the essential features that benchmarking proponents propose. We distinguish this paper from previous CT studies by reporting on a wide range of indicators for the programme, rather than limiting our reporting of results to one specific indicator or topical area as most published papers do, and in particular, we report on well-being, social and productive domains in the same article. This approach allows the readers to appreciate the full potential of an unconditional cash transfer in a poor, rural context in Africa.

Our results are based on the evaluation of the Zambian Child Grant Program, an unconditional cash transfer programme run by the Ministry of Community Development, Mother and Child Health (MCDMCH) that provides a flat transfer to any family with a child under 5 years of age in three of the poorest districts in Zambia (Kalabo, Shangombo and Kaputa). What is appealing about this programme is its simplicity: the programme is universal but geographically targeted to poor districts, all households receive the same amount irrespective of household size, and the cash is unconditional. Such a programme can be implemented with minimal capacity and administrative oversight and can thus be implemented virtually anywhere in the developing world; as such it provides an interesting case study to assess the possibility of cash transfers as the silver bullet for poverty alleviation.

We summarize results of the CGP based on a cluster randomized controlled trial (RCT) using data from baseline and 24-months post intervention; in some cases we refer to 30-month impacts where they differ from those at 24-months. The technical details of the evaluation are provided in the publically available evaluation report (www.cpc.unc.edu/projects/transfer/zambia) and so we keep our discussion of those issues to a minimum in order to focus on what we believe is the main contribution to the current discussion on benchmarking of CTs: the wide range of impacts across many different domains (both social and economic) based on one, relatively easy to implement programme.

2 THE ZAMBIAN CHILD GRANT PROGRAMMME

In 2010, the MCDMCH started implementing the CGP in the three districts with the highest rates of child mortality: Kalabo, Kaputa, and Shongombo. All three districts are near the Zambian border with either the **Democratic Republic of Congo** (Kaputa) or Angola (Shongombo and Kalabo) and require a minimum of two days of travel by car to reach from the capital, Lusaka. Because Shongombo and Kalabo are cut off from Lusaka by a flood plain in the rainy season, they can be reached only by boat during some months of the year. These districts represent some of the most remote locations in Zambia, making them a challenge for providing support services, and are among the most underprivileged communities in Zambia.

The CGP targets any household with a child under 5 years of age. Recipient households receive approximately US$12 per month irrespective of family size, an amount deemed sufficient to purchase one meal a day for an average sized household for one month. Payments are made every other month through a local paypoint manager, and there are no conditions to receive the money. The overarching objective of the CGP is to reduce extreme poverty and the intergenerational transfer of poverty. The specific objectives relate to five outcome areas: food security, young child nutrition and health, education for school-age children, and livelihoods strengthening.

3 EVALUATION DESIGN AND IMPLEMENTATION

The CGP impact evaluation relies on an RCT to estimate the effects of the programme on recipients. Communities designated by Community Welfare Assistance Committees (CWACs) were randomly assigned to either the treatment condition to start the programme in December 2010 or to the delayed control condition to start the programme at the end of 2013. A delayed-entry control group was ethically feasible because the MCDMCH did not have sufficient resources or capacity to deliver the programme to all eligible households immediately. This study includes several levels of random selection, including CWACs within districts and households within CWACs. It is a multisite RCT because random assignment of CWACs occurs within each of the three districts. The Ministry conducted the first step of the randomization process by selecting and ordering 30 CWACs within each district (out of roughly 100 CWACs in each district) through a lottery held at the Ministry headquarters in June 2010 with Ministry staff from the three districts participating (*random selection*). This process created transparency and understanding about how the communities were selected for everyone involved in implementing the programme. After the 90 CWACs were randomly selected (30 from each district) for the study, CWAC members and Ministry staff identified all households meeting the demographic eligibility criterion.

This process resulted in more than 100 eligible households in each CWAC; 28 households were then randomly sampled from each CWAC for inclusion in the study (see AIR 2013 for a detailed description of the random sampling procedure and power calculations to determine optimal sample size). Baseline data were collected for the 28 randomly sampled households in each randomly selected CWAC in each district (30 CWACs per district) and located in one of the three geographically targeted districts. The final study sample size was more than 2,500 households. The baseline data collection began *before* CWACs were randomly assigned to treatment or control conditions. Neither the households nor the enumerators knew who would benefit initially and who would have to wait. *Random assignment* to study arm occurred after the baseline data collection was complete, with the Ministry's Permanent Secretary flipping a coin to determine whether the first half of the list of randomly selected CWACs would be in the treatment or the delayed control condition. This process was conducted in public with local officials, Ministry staff, and community members present as witnesses.

Baseline data collection occurred in October-November 2010 which is the beginning of Zambia's lean season, and field work in the three districts occurred within a few weeks of each other. First payments to treatment CWACs began in January 2011 and payments have been systematic and on-time every two-months during the evaluation period. The 24-month follow-up was collected at the exact same time of the year in 2012. A 30-month follow-up was conducted in June-July 2013 (the harvest season) to look at topics related to seasonality and consumption smoothing.

4 CONCEPTUAL FRAMEWORK

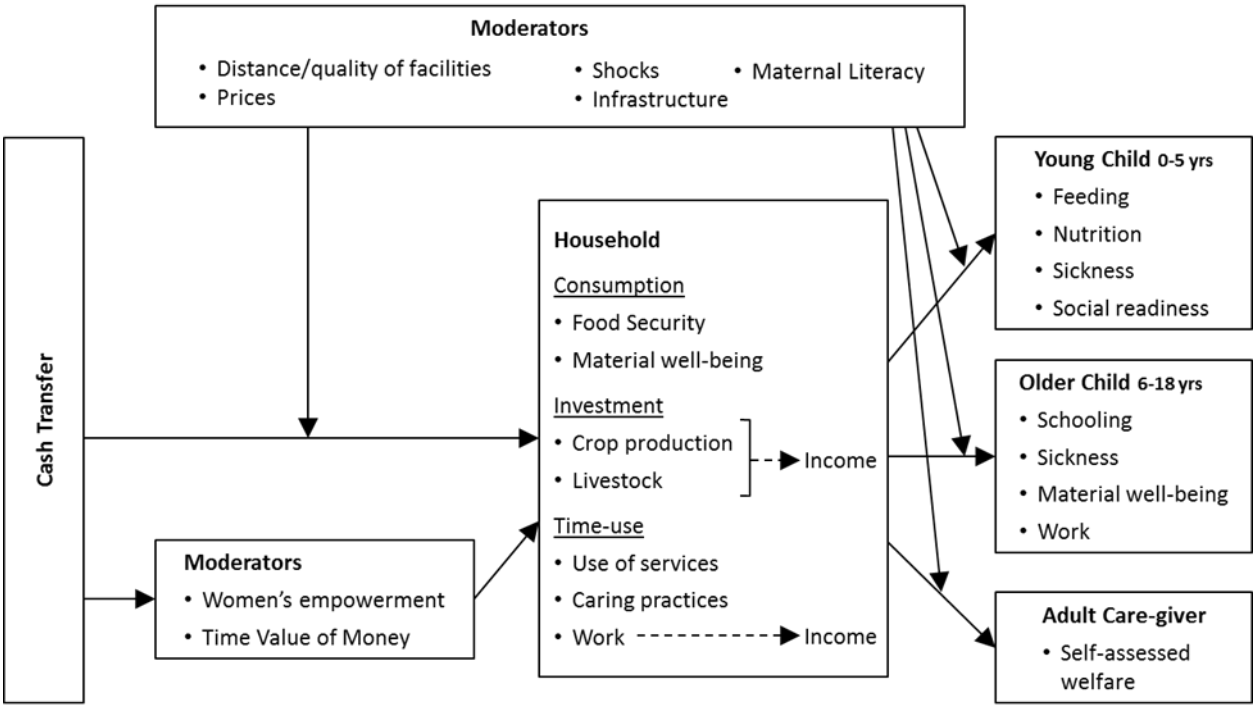
The CGP provides an unconditional cash transfer to households with a child under age 5 and eligible households are extremely poor, with 95 per cent falling below the Zambian national extreme poverty line and having a median household per capita daily consumption of approximately 20 U.S. cents. Tracking the full impacts of an unconditional CT requires collection of a wide range of information since households are free to spend the money as they wish. In the case of the CGP,

given both the eligibility criterion (having a young child), the messaging around the purpose of the grant, and the fact that households are so poor that the marginal propensity to consume is possibly close to 100 per cent, we expect the immediate impact of the programme to be to raise spending levels, particularly basic spending needs for food, clothing, and shelter, some of which will influence children's health, nutrition, and material well-being. Once immediate basic needs are met, the influx of new cash may then trigger further responses within the household economy, for example, by providing room for investment and other productive activity, the use of services, and the ability to free up older children to attend school.

To guide the evaluation questions and help understand the timing of impacts, Figure 1 presents a conceptual framework designed to communicate to policy-makers and programme managers how the CGP could affect household activity, the causal pathways involved, and the potential moderator and mediator factors. The diagram is read from left to right and shows that we expect a direct effect of the cash transfer on household consumption (food security, material well-being), on the use of services, and possibly even on productivity. The impact of the cash transfer may be weaker or stronger depending on local conditions in the community. These moderators include access to markets and other services, prices, and shocks. Moderating effects are shown with dotted lines that intersect with the solid lines to indicate that they can influence the strength of the direct effect.

The next step in the causal chain is the effect on children, who are separated into older and younger children because of the programme's focus on very young children and because the key indicators of welfare are different for the two age groups. The diagram illustrates to a non-technical audience that any potential impact of the programme on children must work through the household through spending or time allocation decisions (including use of services), and that the link between the household and children can be moderated by environmental factors, such as distance to schools or health facilities, and household-level characteristics themselves, such as the mother's literacy. Indeed, from a theoretical perspective, some factors cited as mediators may actually be moderators, such as women's bargaining power. The figure essentially maps out the household production model of decision-making (Becker 1965; Thomas & Strauss 2008), with the 'outcomes' in the middle of the figure representing the inputs in the home production function which yield the outputs of children's human capital on the far right. However, we do not impose this structure in our estimation of treatment effects, but rather consider each outcome individually, beginning with the household and then moving to children—this approach is more transparent for policy-makers and development partners to follow.

Figure 1. Conceptual Framework for Impact Evaluation of Child Grant Program



5 BALANCE AND ATTRITION

We checked for balance across the two study arms by testing for differences in means in 42 indicators, both potential outcomes as well as basic household characteristics such as family size. Only two of the 42 indicators were statistically significantly different at the 5 per cent level, and they represented very small standard deviations (SD) differences.[3]Ninety-one per cent of the households from baseline remain in the 24-month follow-up sample. Table 1 indicates that 72 per cent of the missing households come from Kaputa. Most of the attrition in Kaputa occurred because the Cheshi lake is drying up, forcing households that relied on the lake for fishing and farming at baseline to move their homes as they follow the edge of the lake inward. Entire villages disbanded, with households spreading out to new areas and building new homes in remote swampy areas that were difficult to locate or reach by vehicle on land. This problem in Kaputa affected treatment and control households equally.

We investigate attrition at the 24-month follow-up by testing for similarities at baseline between (1) treatment and control groups for panel households only (differential attrition) and (2) all households at baseline and the remaining households at the 24-month follow-up (overall attrition). We do not find any significant differential attrition at the 24-month follow-up, meaning that the benefits of

[3] The head of household in the treatment group had 0.5 years more education than the control group (5.7 years compared to 5.2 years). Also, children over 5 years old in the treatment group are on average 0.25 years older than the average child over 5 years old in the control group (9.84 years compared to 9.59 years). Note that we would expect at least two significant differences in comparing 42 indicators from otherwise equivalent groups as a result of sampling error. The sample size in the study is quite large, with 2,500 households, more than 5,000 children ages 5 and under, and more than 5,000 children between the ages of 6 and 18; thus, we have power to detect very small and substantively meaningless differences, similar to what occurred in the PROGRESA evaluation (Behrman & Todd 1999). The full t-test results on all 42 indicators are reported in AIR 2011.

randomization are preserved. We find small differences between the study population at baseline and those that remain at the 24-month follow-up; the remaining households are less likely to have experienced a shock, especially flooding or drought at baseline, and they consume a higher proportion of maize over cassava. The differences from overall attrition are primarily driven by the lower response rate in Kaputa district; households in Kaputa tend to eat more cassava (a tuber) as their staple food instead of maize, which is more common in Kalabo and Shangombo, while the ecological changes in Kaputa region, especially the lake drying up, explain why we find that a slightly smaller percentage of remaining households reported experiencing a shock such as drought or flooding (16 per cent) compared with the entire baseline sample (19 per cent).

We find no difference in baseline characteristics between the treatment and control households that remain in the study at the 24-month follow-up, meaning that there is no differential attrition. Table 2 shows the household response rates at the 24-month follow-up by treatment status for each district. The response rates are balanced between the treatment and control groups. We test all the household, young child, and older child outcome measures and control variables for statistical differences at baseline between the treatment and control groups that remain in the 24-month follow-up analysis. None of the 42 indicators is statistically different, demonstrating that on average, people missing from the 24-month follow-up sample looked the same at baseline regardless of whether they were from the treatment or control group. The similarity of the characteristics of people missing in the follow-up sample between treatment statuses allays the concern that attrition introduced selection bias.

6 RESULTS

6.1 Estimation Plan

The impact results we report below are derived using multivariate difference-in-differences (DD) estimation with robust standard errors clustered at the CWAC level. We choose to use the panel sample only though, due to the randomness of attrition; results are the same if we use the unbalanced panel. We include a standard set of control characteristics measured at baseline, including household size, recipient age, education and marital status, district, household demographic composition and a vector of cluster-level prices. For individual outcomes we also include the age and sex of the child. The key assumption underpinning the DD is that there is no systematic unobserved time-varying difference between the T and C groups. In practice, the random assignment to T and C, the geographical proximity of the samples, and the rather short duration between pre- and post- intervention measurements makes this assumption quite reasonable.

In addition we have done a detailed analysis of changes in village prices (measured through a community questionnaire) over time across the two arms and find no differences, which provides additional assurance that the assumption of parallel trends is maintained in the study (AIR 2013). We use inverse probability weights to account for the 9 per cent attrition in the follow-up sample, though these are not necessary since the results are the same if we exclude the weights (Wooldridge 2010). We present impact estimates around five main domains of interest: consumption, older child outcomes, younger child outcomes, women and livelihoods strengthening.

6.2 Ex-ante Analysis

At baseline, we ran simulations to see where we might expect impacts of the CGP using the procedure described in Kenya CT-OVC Evaluation Team (2012). This entailed estimating the relationship between total per capita expenditure and different outcomes controlling for a standard set of covariates and using the resulting coefficients to predict the outcome given an increase in per capita expenditure equal to the size of the transfer. We used this analysis to demonstrate to programme managers and development partners the likely areas where impacts might be expected, since with an unconditional cash transfer the potential areas of impact are quite large. This approach assumes of course that all the cash transfer is spent, and that cash does not shift the Engel Curve but simply moves households along their pre-existing curve. Details of the procedure as applied to the CGP are discussed in AIR 2011 and comparisons of actual versus predicted impacts are presented in AIR 2013. In the discussion below we will occasionally refer to these ex-ante predictions in our interpretation of results when they are insightful.

6.3 Consumption, Poverty and Food Security

As expected among very poor households, virtually all programme income is consumed. The CGP increased total per capita consumption spending by ZMW 15.18 per month, greater than the average per capita value of the transfer of ZMW 12 (though not significantly so). Figure 2 shows a clear shift to the right in the density of consumption spending (excluding the top 1 per cent) among T households in 2012. The vertical line in these graphs is the severe poverty line as defined by the Central Statistics Office in 2010 (ZMW 96.37) inflated to 2012 units. The majority of the increased spending goes to food (ZMW 11.60), 76 per cent of additional spending, followed by health and hygiene (ZMW 1.08) at 7 per cent, clothing at 6 per cent, and transportation/communication at 6 per cent. In contrast, there is no programme impact on education, domestic items, or alcohol/tobacco spending. Impact estimates for total consumption and eight broad consumption categories (measured in per capita monthly Zambia Kwacha inflated to 2012 values) are shown in Table 3. The subsequent rows of Table 4 show the distribution of the increased spending by category.

There is a clear shift away from roots and tubers (primarily cassava) and toward protein (dairy, meats), indicating improvement in diet diversity among CGP recipients. Table 4 breaks down the programme impacts by detailed food groups. The overall increase in food spending is ZMW 11.60 as reported in Table 4; the largest share goes to cereals (ZMW 4.54), followed by meats, including poultry and fish (ZMW 2.44), fats such as cooking oil (ZMW 1.76) and sugars (ZMW 1.28).

Given the significant programme impact on consumption we expect to see reductions in consumption based poverty measures. Beginning with the severe poverty line, the programme reduces the headcount rate by 5.4 percentage points; however, the largest programme impacts are found for the poverty gap (10.9 percentage points) and squared poverty gap (10.8 percentage points), which account for the distribution of individuals below the line rather than whether individuals moved above the line (Table 5).[4] For programmes that reach people at the very bottom of the income distribution, these last two indicators are better measures of changes in welfare because

[4] In column 1 we provide the simple (unadjusted) impact estimates. These, as well as the means in the table, are weighted by household size to be representative of the population of individuals living in beneficiary households.

it is highly unlikely for a programme to provide sufficient funds to lift people at the very bottom of the distribution above the poverty line. However, a significant positive movement below the line will show up in the poverty gap and squared poverty gap indicators. Thus, this pattern of results is evidence of both the highly successful targeting approach of the CGP as well as its impact on welfare.

Virtually all CGP recipients are below the moderate poverty line (99 per cent), and the impact of the programme on the poverty headcount using the moderate poverty line, although statistically significant, is very tiny at 1.7 percentage points. However, the impacts on the poverty gap and squared poverty gap continue to be large simply because these indicators account for the distribution of individuals below the line. Notice that among the control group, there is also a clear trend of improvement in terms of the poverty gap and squared poverty gap, although the gains in monetary welfare among the CGP participants is an order of magnitude larger in terms of percentage change from baseline.

One of the goals of the CGP is to improve the food security of beneficiary households and specifically increase the percentage of households eating two or more meals per day. The CGP increases the percentage of households eating two or more meals per day by 8 percentage points, an effect size that is possibly subject to a ceiling effect since almost everyone in the T group is eating two or more meals per day (97 per cent) at follow-up. We also implemented the Food and Nutrition Technical Assistance Project's (FANTA) food security access scale, a 10-item scale with greater values indicating more food insecurity (Coates et al 2007). We find that the programme reduces a household's food insecurity score by 2.5 points, a 20 per cent decrease from the control group's score. The programme increases the number of households that are not severely food insecure by 18 percentage points (36 per cent in the treatment group versus 16 per cent in the control group), a 113 per cent improvement over the control group. The CGP also has a strong impact on perceptions of well-being. Twice as many CGP households (71 per cent) as control households (35 per cent) do not consider themselves very poor, and five times more CGP households (60 per cent) than control households (12 per cent) report being better off now than they were 12 months ago. The FANTA score and subjective indicators are therefore consistent with the aggregate consumption expenditure results, a relevant finding given the increasing importance being given to the relationship between psychological and material welfare in the literature on poverty (Grant 2005). Table 6 shows the impacts of the programme on several of these food security indicators.

We mentioned earlier that a 30-month follow-up was conducted as part of the evaluation of the CGP to understand the consumption smoothing potential of the programme. Here we present preliminary data from this survey to demonstrate the impact of the CGP on consumption over the agricultural cycle. Overall, the programme manages to get households to their 'ideal' level of consumption, and this level is higher than the level of consumption among C households during their peak consumption period. First, the relative increase in consumption at 24-months among T is very large compared to C – these are the large impacts reported in Table 3. Second, during the harvest season (June-July), consumption among T does not increase any further while consumption in C increases. Thus, T households are able to smooth their consumption over the agricultural season as a result of the programme. Third, the overall level of consumption in T is higher at 24-months (planting season) than the level in C during at 30 months (harvest season). Figures 3 and 4 show the level of

total and food consumption across the three survey rounds by study arm – figures are rebased to the 2010 kwacha and so are not strictly comparable to those in Table 3.[5]

These same features of the data also hold for food consumption. We see that food consumption for T households essentially stabilizes over the agricultural season but continues to increase for C households, while the overall level of food consumption remains higher among T households. Consequently we conclude that the CGP has had an important impact on both overall levels of total and food consumption as well as on enabling households to smooth consumption over the season.

6.4 Younger Children

In this section, we report programme impacts on a series of young child indictors covering health, use of services, nutritional status, and early childhood development. While these indicators represent some of the most important objectives of the programme, it is important to remember that the grant is unconditional, and the supply of health services to these communities is very weak. A separate health facility survey (N=31 facilities), conducted as part of the evaluation, provides insights into the level of services available to households in the study districts. For example, only 6 per cent of the facilities have electricity, less than 10 per cent have clean water, and one-quarter have a laboratory for testing. There is no full-time medical doctor at any facility, and 10 per cent of facilities have a registered nurse. Drug availability is also quite low, with oral rehydration salts (ORS) available in only 45 per cent of facilities. This is an exceptionally inexpensive item that helps to treat diarrheal disease, one of the leading causes of child mortality in developing regions.[6]

We see a strong programme impact on the prevalence of diarrhea in the previous 2 weeks—a decline of 4.9 percentage points—and a smaller effect of 3.6 percentage points on acute respiratory illness (cough), although not statistically different from zero (Table 7). However, the only statistically significant programme effect on the use of health services is for the treatment of acute respiratory illness (ARI) and indicates a *reduction* in curative care for ARI among children in the programme, an opposite effect from what we expect (Table 8). Note though that this is estimated on the 7 per cent of children that reported ARI in the reference period, hence a small and select sample.

The CGP has induced an improvement in the weight of young children (Table 9), with effects on weight-for-height and weight-for-age of about 0.12 standard deviations, although these are just outside the levels of statistical significance, all measured in standard deviations or *z*-scores using the World Health Organization reference tables. The table also shows a large and statistically significant impact of the CGP on Infant and Young Child feeding (IYCF)—an increase of 22 percentage points or an 88 per cent increase over the baseline mean. This result is consistent with the consumption expenditure effects reported earlier. Since feeding is an important determinant of weight, we checked whether there are noticeable impacts of the CGP on weight among children 6 to 24 months but do not find any statistically significant effects. In addition, because stunting is such a critical child development indicator we explored whether the CGP affected stunting among any sub-groups.

[5] In 2012 the Zambian government rebased their currency by a factor of 1,000; so that 1,000 2011 Kwacha was equal to 1 2012 Kwacha.
[6] Cotrimoxazole and penicillin, used to treat infections, were actually available at the time of the survey in only 23 per cent of the facilities. Fansidar and coartem (common malaria treatments) were unavailable in the majority of facilities, although over half of the facilities claimed they normally carried both of these drugs. Although three-quarters of facilities normally carry aspirin or paracetamol, which are most commonly used to treat minor pain and fever, less than forty per cent actually had these items in stock.

There is significant heterogeneity in treatment effects among children living in households with a protected water source, and whose mother had more years of schooling (Seidenfeld et al 2014), suggesting these are complementary inputs to cash in determining child height.

To round off this section we report impacts on a set of early childhood development indicators using items developed and tested by UNICEF as part of its global Multiple Indicators Cluster Survey (MICS) 4 programme. We administered this module to children aged 3 through 7 in our sample and constructed the six recommended indicators from the MICS Child Development Indicator list (indicators 6.1 and 6.3–6.7).[7] 'Support to Learning' measures whether an adult played with the child, counted, named or drew things with the child, sang songs or told stories to the child, read books or looked at pictures with the child, or took the child outside the compound. 'Learning Materials' refers to whether the child possesses at least three books or whether the child plays with homemade or store-bought toys or objects around the home, such as pots, bowls, rocks, or sticks. 'Adequate Care' measures whether the child was ever left alone for more than 1 hour or left in the care of someone less than 10 years old. 'School Attendance' includes any sort of formal programme, including preschool and daycare. Finally, the ECD Index is a 10-item scale that covers four developmental domains: physical (both gross and fine motor), language and cognition, socio-emotional, and approaches to learning.

Households in the CGP show significantly higher support for learning and learning materials as well as attendance at a formal educational programme (Table 10). The overall ECD Index Score also increases noticeably, although it remains just outside statistical significance. It is interesting to note the strong increase over time in the mean level of playthings and adequate care in both study arms.

6.5 Older Children

Although the CGP targets households with children under age 5, older children might benefit from living in a household that receives the programme, depending on how the money is spent. The conceptual framework in section II demonstrates how the cash might have an impact on certain areas, such as children's material well-being, education, and health. Our ex-ante simulations using baseline data suggested that material well-being would likely improve and that there could be a small change in school attendance for secondary-school age children only, but we did not expect impacts for other older-child-related indicators (AIR 2011).

The CGP has a large impact on children's material well-being, indicating that recipients use some of the transfer to purchase items deemed necessary for supporting orphans and vulnerable children (Table 11). The material well-being indicator is a scale from 0 to 3; a child gets a point for having a shared blanket, a second set of clothing, and shoes.[8] The CGP increases children's material well-being by 34 percentage points. This impact is largely due to the increase in the number of children with shoes in recipient households compared with those in non-recipient households. At baseline, only 11 per cent of the children aged 6 to 17 had all three items. Two years later, 61 per cent of the children in recipient households has a blanket, a change of clothing, and shoes, whereas only 26 per cent of the children in non-recipient households has all three items. We suspect that the control

[7] See http://www.childinfo.org/mics4_tools.html.
[8] The material well-being scale is a recommended indicator to measure care and support for orphaned and vulnerable children. UNICEF (2005).

group's growth results from the bumper harvests that occurred during the study period and general economic improvement of the country.

Table 11 shows the impact of the programme on each item that makes up the material well-being scale as well as having all three items. The programme has an impact on both shoes and blanket ownership, with shoes dominating this effect with a 33 percentage point increase (20 percentage point increase for blankets and 8 percentage point increase for clothing). A ceiling effect occurs for clothing because 97 per cent of children in recipient households and 89 per cent of children in non-recipient households own a second set of clothing two years into the programme. Therefore, there is little room for recipient households to improve more than non-recipients on this indicator, yet the difference is still significant. This study asks about a second set of clothing, but perhaps children in recipient households own more clothing than children in non-recipient households, an indicator not captured here.

In line with our ex-ante predictions using baseline data, we do not find strong across-the-board impacts on education outcomes. We investigate education outcomes related to enrollment and attendance at three age groups: 6-8 years to see if the programme has spurred 'on-time' school participation among younger children, 6-13 representing primary school-age children and 14-17 to cover secondary school-age children (Table 12). There is some suggestion of improvements relative to the control group, with large and significant increases in the likelihood of full attendance in the previous week among primary school-age children (almost a 10 per cent increase over the baseline mean for children age 6-13 years). Here again, we looked for and found heterogenous treatment effects according to the educational attainment of mothers (results available upon request); the lower a mother's level of completed education, the greater the impact that CGP has on her children's education. Children living in a beneficiary household are 1 percentage point more likely to ever enroll in school and 2 percentage points more likely to enroll on time (age 6-8 years), for every year less of education their mother has. Note that there is a positive relationship between mother's education and school enrolment in the sample hence better-educated mothers were already enrolling their children in school at baseline.[9] However, it seems that the CGP enabled or motivated less educated mothers who did not enroll their child in school at baseline to start enrolling their child in school, leading to a programme impact on education for children with less educated mothers.

We also investigate health outcomes for older children with respect to morbidity, treatment seeking, and chronic illness. Again in line with our ex-ante simulations based on expenditure elasticities, we did not find any impacts on these health outcomes for children over age 5, despite the fact that the programme increased spending in health, mostly user fees and drugs. Among this age group, illness is a rare event, with only 10 per cent of the children reporting that they were ill or injured in the previous 2 weeks. Of the 10 per cent who reported being ill, 80 per cent sought treatment. This rate of treatment is up from baseline by roughly 20 percentage points, but occurs evenly in both the treatment and the control groups.

[9] Enrollment rates among children 6-13 at baseline are 58 and 75 per cent for those with mother's with and without complete primary schooling respectively.

6.6 Impacts on Women

Although the CGP is targeted toward children under age 5, because cash is in most cases given directly to women (95 per cent of cases), there is potential for impacts on women-level outcomes. As demonstrated in the conceptual framework, these impacts depend on many factors, including power relations in households and characteristics of women, such as how future looking they are in determining consumption patterns. The following section explores trends and the impact of CGP on bargaining power, savings, future outlook, and women's health.

To explore bargaining power among sample households, we asked decision-making questions in nine domains: children's health, children's schooling, spending of own income, spending of partner's income, major household purchases, daily household purchases, spending on children's clothes and shoes, visits to family and relatives, and own health. These questions were asked of one woman per household (typically a mother or caregiver of a target child), and they allowed the respondent to answer whether a decision is typically made by herself, by her partner, jointly, or by someone else in the household. To explore impacts, we construct two indicators. The first is a count or summation, giving 1 point to each time the woman indicates having sole decision-making power in a domain (ranges from 0 to 9). The second is an index constructed by factor analysis, which weights indicators differently on the basis of their variation within the sample and correlation between each other. Table 12 shows the impact of the programme on the count indicator and the index of sole decision making. Results indicate that the programme has no measurable impact on sole decision making, a finding that remains unchanged even when we consider sole or joint decision making. A more detailed analysis (not shown here) indicates a notable increase over time for both treatment and control groups in all decision-making domains. For example, the percentage of women responding that they alone have decision-making power about their child's health increases from approximately 56 per cent to approximately 70 per cent in both treatment and control groups. Similar gains can be seen across other decision-making domains. However, the lack of measurable impact indicates that transfers are seen as common household resources and are not necessarily changing women's bargaining power within households after 24 months.

Table 13 shows indicators of savings and future outlook as reported by the female respondents answering bargaining-power questions for each household. Results indicate that at baseline, approximately 16 per cent of households had any savings in the previous 3 months. However, by the 24-month follow-up, this percentage increased to 47 per cent, while control households increased by a smaller fraction to 22 per cent. As expected, we find a large and significant programme impact on any savings and similarly on the amount of savings reported in ZMW. These impacts demonstrate that households not only are using the transfer for immediate consumption but also are saving a portion of the transfer. We also find significant impacts on future outlook. At baseline, 61 per cent of households believed that life would improve over the next 3 years, and this increases to 91 per cent among treatment households, and less so to 82 per cent among control households.

Finally, we investigated health outcomes for women age 18 and older with respect to morbidity in the previous 2 weeks, care seeking for illness, chronic illness in the previous 6 months, and self-reported health status. We do not find any impacts on morbidity, care seeking, or chronic illness, not surprisingly given the emphasis of the programme on children. We do find impacts on self-rated health status. More specifically, women in treatment households are significantly more likely to

report “good health or better” and “very good health or better” than those in control households (results available upon request), which may be an indication that women are more optimistic about their health and economic situation in programme households.

6.7 Livelihoods Strengthening

CGP beneficiaries are poor, with limited options in terms of livelihoods and with few assets with which to generate income. Beneficiary households have on average approximately half a hectare of agricultural land, a couple of chickens, basic agricultural tools, and low levels of education, and they are highly dependent on their own unskilled labour. A large majority of CGP beneficiaries are agricultural producers--almost 80 per cent produce crops, and about half have some form of livestock. At the time of the baseline survey, the beginning of the hunger season, home production accounted for almost 40 per cent of all food consumption. Most beneficiaries grow local maize, cassava, or rice, using traditional technology and very low levels of modern inputs, and have little access to credit.

We begin by looking at crop production and crop input use. Overall, in terms of direct impacts on crop activity, we find positive and significant impacts on area of land operated, overall crop expenditures, and specific expenditure on seeds (Table 14). The CGP increases the amount of operated land by 0.19 hectares (a 34 per cent increase from baseline), and the programme has led to an increase of 18 percentage points in the share of households with any input expenditure, from a baseline share of 23 per cent (not shown in Table 14). We also see a positive impact on ownership of agricultural tools, with significant increases in the number of axes, hoes and hammers among recipient households (results available upon request). As a result, there is a large impact on the value of harvest sold of ZMW81, making the 24 month value more than double baseline. Beneficiary households sold a larger share of their crop production (an impact of 6 percentage points, from a baseline of 10 per cent) (Table 15). The increase in market participation is driven by maize production in Kaputa and by both maize and rice production in Kalabo. No impact was found on the increased consumption of home produced food.

The CGP has a positive impact on the ownership of livestock, including a 12 point increase in the share of households with chickens and 14 points for other cattle besides milk cows, as can be seen in Table 16. The impact on the share with any livestock is 21 percentage points, an increase of 49 per cent from baseline (not shown). The increase in the share of households with other cattle is particularly important as these are quite expensive and in fact represent a durable good in rural Zambia.

In the follow-up survey we introduced a module on non-farm business activity. Single difference impact estimates (comparing treatment and control groups at 24-months), which assume baseline equivalence, indicate that beneficiary households are significantly more likely to have a non-farm enterprise (Table 17). The share of beneficiary households operating a non-farm enterprise is 16 percentage points higher than control households. Moreover the programme increased the value of total monthly revenue (ZMW 89) and profit (ZMW 35).

Finally, at baseline we administered a module on housing characteristics which we excluded at 24-months but re-introduced at the 30-month follow-up. Preliminary results from this module show significant increases in housing quality with associated implications for health outcomes. For example, the CGP led to a 15 percentage point increase in the share of households that own a latrine (67 per cent of beneficiaries—Table 18). Owning a latrine is a key element in improved household hygiene and sanitation, yet less than half of households had a latrine at baseline. The CGP seems to have helped with costs to build a latrine, such as hired labour to dig the hole and construct the latrine, cement for the platform, and bricks for the walls. Similarly, the CGP led to a three percentage point increase in the share of households with cement floors. In addition to improving their home, we also find that beneficiaries improved their daily living conditions by purchasing torches or candles to light their home instead of using an open fire. Over half of the households used open fire to light their home at baseline (57 per cent). The CGP had a 26 percentage point impact in the share of households using a purchased method to light their home such as candles or torch, with 86 per cent of beneficiary households using a purchased method—this in turn significantly reduces the exposure to household air pollution (Lim et al 2010). Interestingly, both the treatment and control households experienced a large reduction from baseline in the use of open fires to light their home and an increase in torches, which we attribute to the introduction of low-cost LED torches in rural Zambia.

7 DISCUSSION AND CONCLUSIONS

Both the challenge and the promise of unconditional cash transfers is that impacts of such programmes can be found over a broad range of indicators, including consumption, food security, subjective well-being, morbidity, health-seeking behaviour, educational outcomes, livelihoods strengthening and adolescent sexual behavior. The administrative simplicity and cost effectiveness of cash transfers has led some to suggest that cash transfer programmes can serve as the benchmark by which anti-poverty initiatives are measured. And indeed, the impacts of the Zambian CGP, documented by a RCT impact evaluation, touch a startling range of indicators. Moreover, the CGP is a relatively simple, easy to implement programme, using categorical targeting and not heavy on administrative capacity, and thus serves as an interesting example for other poor countries to follow.

However, can cash solve everything? Even in the case of the Zambian CGP, the cash transfer did not have significant impacts over all indicators. The programme did not impact spending on education, nor use of health care, nor nutritional status, nor school enrolment or attendance of older teenagers. These results suggest that the specifics of each programme matter and that the ultimate range of effects depend on programme design, availability of services, and context. The greater the magnitude of the transfer in regard to household incomes, the more likely the programme is to have more broad impacts. A programme with strong messaging or conditionality will focus spending on specific outcomes. The regularity and predictability of payments in practice is fundamental in terms of allowing households to effectively smooth consumption and manage risk (Handa et al 2013). Some impacts, such as nutritional status, are complex to move and depend on other factors beyond the cash transfer programme. And a weak supply of health services probably explains a lack of results in use of health services. Moreover, households will spend the money depending on the specific constraints that each household faces—and the distribution of these constraints will inevitably vary by context and country.

We can draw two conclusions from this discussion. First, the use of cash transfers as a silver bullet—using one instrument to address a broad range of objectives—runs the risk of not having sufficient impact on all objectives. Cash transfers usually need to be linked with specific complementary interventions—improving the supply and/or quality of services, for example—in order to maximize the potential impact on certain indicators in providing demand incentives via cash transfers. In the case of the CGP for example, the programme affected nutritional outcomes for children with access to a safe water supply. Second, the discussion suggests that using the best results from different cash transfer programmes as a basis by which to compare anti-poverty initiatives (benchmarking) may not be appropriate because this strategy does not acknowledge the variation in impacts within and across programmes. It is a hard ask for one specific cash transfer programme, in a specific context, to be the optimal approach for reaching all objectives, although the Zambian CGP comes about as close as one most optimistically could expect. If any programme is a silver bullet, it is the Zambia CGP, yet even this programme would require complementary interventions in both the demand for, and the supply of specific services in order to have significant impact across all key development indicators.

This article takes a rather different approach in reporting impact evaluation results from a cash transfer programme. Rather than choosing one domain (such as education or nutrition) and exploring impacts in great detail, we take what we believe is a more transparent approach, essentially reporting on all main evaluation questions (both social and productive), and presenting even non-significant results, which would otherwise not be published. Beyond taking a small step towards addressing publication bias, this approach allows the reader to appreciate the full range of benefits that are possible with an unconditional cash transfer in a poor rural context – we believe that given the programme parameters and context these results have very high external validity. However, our approach also has weaknesses, the most notable of which is that we do not have space to investigate any particular domain in-depth, and to get inside the black-box to understand *why* we see the impacts we do, and whether there have been changes in structural behavioral parameters. These more detailed sector analyses are part of the ongoing research agenda to understand how this simple yet powerfully effective programme affects human behaviour.

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TABLES AND GRAPHS

Table 1: Overall Attrition for CGP 24-Month Follow-Up: Household Response Rate by District

District	Response rate	Households at Baseline	Per cent of Total Missing Households
Kaputa	81	837	72
Kalabo	96	838	15
Shangombo	97	839	13
Overall	91	2514	100

Table 2: Household Response Rate by Study Arm at 24-Month Follow-Up for CGP (n = 2515)

District	Treatment	Control	N
Kaputa	82.3	80.1	837
Kalabo	96.4	95.9	838
Shangombo	96.4	96.7	839
Overall	91.9	90.6	2514

Figure 2: Distribution of Consumption Expenditures by Year and Study Arm

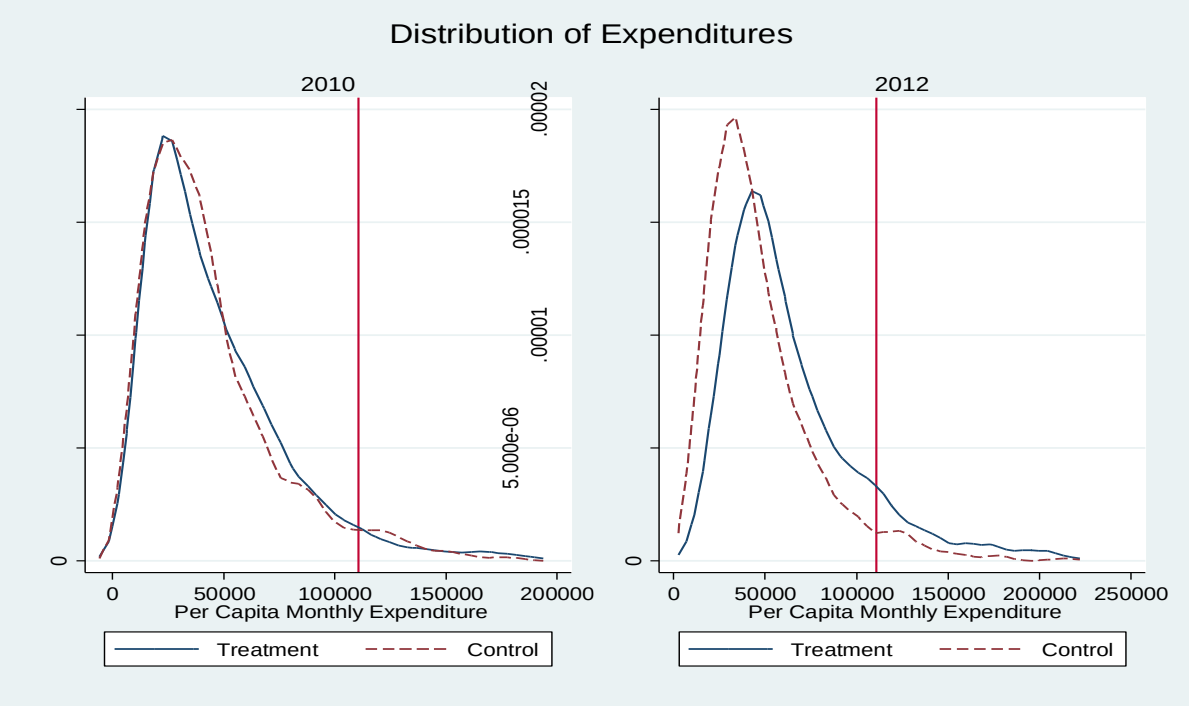

Table 3: Impact of CGP on Consumption Expenditure (ZMK)

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
Total	**15.18**	46.56	67.04	48.59
	(5.07)			
Food	**11.60**	34.45	50.16	35.85
	(4.76)			
Clothing	**0.93**	1.47	2.42	1.50
	(5.71)			
Education	0.10	0.49	1.19	0.99
	(0.34)			
Health	**1.08**	2.60	4.13	2.89
	(4.22)			
Domestic	0.53	6.11	6.40	5.64
	(0.81)			
Transport/Communication	**0.86**	0.91	2.23	1.29
	(2.32)			
Other	-0.01	0.13	0.23	0.18
	(-0.11)			
Alcohol, Tobacco	0.09	0.41	0.29	0.26
	(0.68)			
N	4594			

NOTE: Estimations use difference-in-difference modeling among panel households. Robust *t*-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition, and a vector of cluster-level prices. Last two columns show mean of dependent value by study arm.

Table 4: Impact of CGP on Food Expenditure

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
Cereals	**4.54**	11.61	15.54	9.95
	(3.26)			
Tubers	-0.924	4.96	4.56	4.93
	(-1.25)			
Pulses	**1.22**	0.94	2.00	0.77
	(4.98)			
Meats	**2.44**	6.78	11.43	7.91
	(3.08)			
Fruits, Veg	0.49	7.03	8.86	8.89
	(0.56)			
Dairy	**0.76**	0.88	1.27	0.48
	(3.55)			
Baby Foods	0.02	0.01	0.03	0.01
	(0.78)			
Sugars	**1.28**	0.79	2.61	0.98
	(7.80)			
Fats, Oil, Other	**1.76**	1.45	3.87	1.93
	(6.13)			
N	4594			

NOTE: Estimations use difference-in-difference modeling among panel households. Robust *t*-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.

Table 5: Impact of CGP on Poverty Indicators

		Means			Per cent Change From Baseline	
	Programme Impact	Baseline	Treated	Control	Treated	Control
		Severe Poverty Line				
Headcount	**-0.054**	0.958	0.906	0.960	-5.43	0.21
	(-3.71)					
Poverty Gap	**-0.109**	0.632	0.483	0.607	-23.58	-3.96
	(-4.54)					
Sq. Poverty Gap	**-0.108**	0.456	0.293	0.420	-35.75	-7.89
	(-4.19)					
		Moderate Poverty Line				
Headcount	**-0.017**	0.989	0.974	0.991	-1.52	0.20
	(-2.76)					
Poverty Gap	**-0.090**	0.743	0.627	0.727	-15.61	-2.15
	(-4.71)					
Sq. Poverty Gap	**-0.102**	0.593	0.449	0.567	-24.28	-4.38
	(-4.49)					
N	4815					

NOTE: Estimations use difference-in-difference modeling among panel households. Robust *t*-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.

Table 6: Food Security Indicators

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
Eats more than one meal a day	**0.079**	0.78	0.97	0.89
	(4.02)			
Ate meat/fish => 5 times in last month	0.006	0.31	0.32	0.27
	(0.11)			
Ate vegetables => 5 times in last month	-0.006	0.61	0.74	0.74
	(-0.09)			
Food security scale (FANTA)	**2.498**	15.10	9.63	12.36
	(4.23)			
Is not severely food insecure	**0.177**	0.10	0.36	0.16
	(4.00)			
Does not consider itself very poor	**0.305**	0.41	0.71	0.35
	(-5.78)			
Better off than 12 months ago	**0.453**	0.10	0.60	0.12
	(10.51)			
N	4,549			

NOTE: Estimations use difference-in-difference modeling among panel households. Robust *t*-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.

Figure 3: Average Total and Food per-capita Expenditures - Treatment

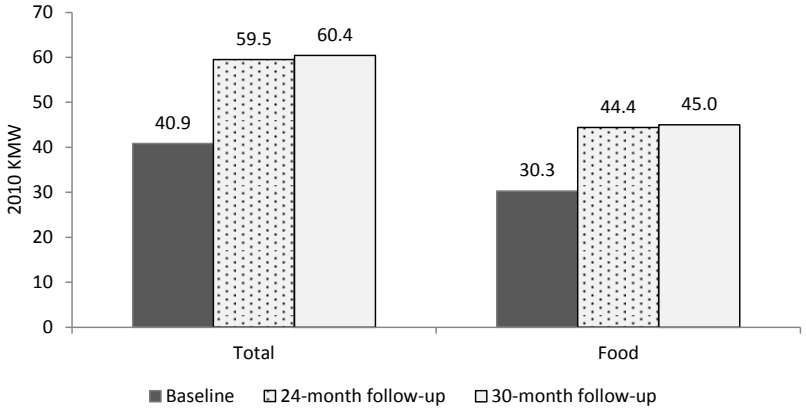

Figure 4: Average Total and Food per-capita Expenditures – Control

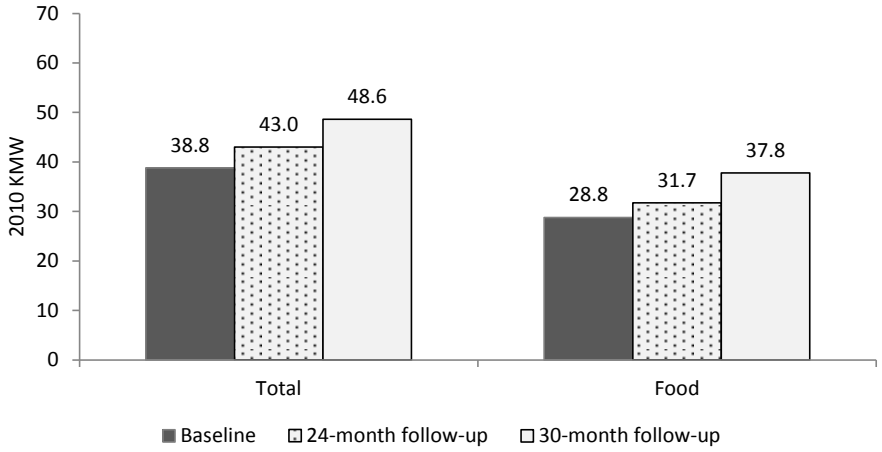

Table 7: Impacts on Morbidity Among Children 0–60 Months

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
Diarrhea	**-0.049**	0.185	0.0684	0.0925
	(-2.38)			
Fever	-0.019	0.233	0.113	0.125
	(-0.53)			
Acute respiratory illness	-0.036	0.203	0.0511	0.0832
	(-1.42)			
N	7232			

NOTES: Reference period for illnesses is 2 weeks. Estimations use difference-in-difference modeling among panel households. Robust *t*-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.

Table 8: Impacts on Use of Services Among Children 0–60 Months

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
Sought preventive care (N=7135)	-0.045	0.776	0.788	0.791
	(-1.14)			
Has birth registration document (N=7646)	-0.063	0.402	0.238	0.251
	(-0.79)			
Sought care for diarrhea* (N=972)	0.039	0.744	0.798	0.796
	(0.54)			
Sought care for fever* (N=1293)	0.012	0.726	0.848	0.823
	(0.16)			
Sought care for ARI* (N=1005)	**-0.142**	0.341	0.157	0.267
	(-2.00)			
N	7232			

* Only estimated on sample that reported this illness in the prior 2 weeks.
NOTE: Estimations use difference-in-difference modeling among panel households. Cluster robust *t*-statistics are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, and a vector of cluster-level prices.

Table 9: Impacts on Nutritional Status and Feeding

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
Weight for age *z*-score (N=6825)	0.128	-0.902	-0.900	-0.963
	(1.89)			
Weight for height *z*-score (N=6157)	0.118	-0.180	-0.0961	-0.154
	(1.74)			
Height for age *z*-score (N=6155)	0.066	-1.416	-1.445	-1.491
	(0.70)			
Young child feeding (N=1983)	**0.217**	0.317	0.596	0.434
	(3.54)			
N	7232			

NOTE: Nutritional indicators are reported for children 0 to 60 months; child feeding are reported for children 6 to 24 months as recommended in the ZDHS. Estimations use difference-in-difference modeling among panel households. Cluster robust *t*-statistics are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, and a vector of cluster-level prices.

Table 10: Impacts on Early Childhood Development Indicators

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
Support to Learning (6.1)	**0.126**	0.432	0.307	0.225
	(2.33)			
Learning Materials: Books (6.3)	**0.010**	0.0148	0.027	0.011
	(2.09)			
Learning Materials: Playthings (6.4)	-0.035	0.629	0.767	0.751
	(-0.51)			
Adequate Care (6.5)	-0.003	0.307	0.620	0.647
	(-0.04)			
ECD Score 5+ (6.6)	0.070	0.558	0.610	0.572
	(1.32)			
ECD Index Score*	0.311	4.848	5.174	4.926
	(1.62)			
School Attendance (6.7)	0.041	0.224	0.154	0.136
	(1.76)			
N	5670			

NOTES: All estimates are marginal probabilities from probit regression except those with *, which are OLS because they are continuous instead of binary variables. Statistical significance at 5 per cent or better is shown in bold. The MICS indicator number is shown in parentheses beside the indicator name.

Table 11: Child Material Needs Met at Ages 5–17

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
All needs met	**0.334**	0.11	0.61	0.26
	(5.47)			
Child has shoes	**0.331**	0.14	0.62	0.29
	(5.15)			
Child has two sets of clothing	**0.140**	0.63	0.97	0.88
	(4.47)			
Child has a blanket	**0.252**	0.58	0.96	0.78
	(6.04)			
N	8,367			

NOTE: Estimations use difference-in-difference modeling among panel households. Cluster robust *t*-statistics in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, and a vector of cluster-level prices.

Table 12: Probit Marginal Effects of CGP Impact on Schooling

	Programme Impact	Baseline	24-Month Treatment	24-Month Control
			6-8 years	
Currently enrolled (N=2929)	0.039	2,929	0.393	0.442
	(0.87)			
Full attendance (N=1176)	0.091	1,176	0.789	0.874
	(1.85)			
			6-13 years	
Currently enrolled (N=6556)	0.036	6,556	0.633	0.685
	(1.26)			
Full attendance (N=4249)	**0.070**	4,249	0.789	0.875
	(2.04)			
			14-17 years	
Currently enrolled (N=1767)	-0.035	1,767	0.709	0.791
	(-0.81)			
Full attendance (N=1290)	0.005	1,290	0.796	0.848
	(0.09)			
N	8,367			

NOTE: Estimations use difference-in-difference probit modeling among panel households. Cluster robust *t*-statistics are in parentheses. Bold indicates significance at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, and a vector of cluster-level prices.

Table 13: Women's Decision Making, Savings and Future Outlook

	Programme Impact	**Baseline**	**24-Month Treatment**	**24-Month Control**
Count indicator of sole decision making (9 domains)	0.205	3.93	4.46	4.37
	(0.90)			
Index of sole decision making (9 domains) (OLS)	0.055	-0.07	0.08	0.06
	(0.83)			
Any savings last 3 months	**0.201**	0.16	0.47	0.22
	(3.42)			
Log amount saved last 3 months (ZMW) (OLS)	**2.667**	1.74	5.29	2.31
	(5.45)			
Believes life will be better next 3 years	**0.115**	0.61	0.91	0.82
	(3.24)			
N	4,549			

NOTE: Estimations use difference-in-difference modeling among panel households. Robust *t*-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, and a vector of cluster-level prices. Sample sizes are 4498 and households. Linear probability models unless indicated otherwise.

Table 14: Impact of CGP on Crop Input Use and Land Use (ZMK)

Dependent Variable	Programme Impact (1)	Baseline (2)	24-Month Treatment (3)	24-Month Control (4)
Operated land (has)	**0.188**	0.494	0.848	0.626
	(2.065)			
Total crop exp	**30.650**	21.780	68.584	25.821
	(2.546)			
Exp seed	**9.927**	6.398	18.350	7.535
	(3.955)			
Exp hired labor	7.948	7.616	22.238	4.551
	(1.288)			
Exp pesticides	0.028	0.027	0.137	0.174
	(0.171)			
Exp fertilizer	7.965	1.401	12.467	5.215
	(1.895)			
Other crop exp	12.758	13.981	37.768	13.070
	(1.629)			
Value of harvest	145.88	393.88		
	(1.95)			
N	4,594	2,297	1,152	1,145

NOTE: Estimations use difference-in-difference modeling among panel households. Robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at p < .05. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.

Table 15: Impact of CGP on Agricultural Production

Dependent Variable	Programme Impact (1)	Baseline (2)	24-Month Treatment (3)	24-Month Control (4)
Value of sales (ZMW)	**80.751**	75.775	185.977	104.580
	(2.340)			
% of crops sold	**0.054**	0.098	0.150	0.107
	(2.683)			
Value of crops consumed at home (ZMW)	22.962	204.388	364.781	324.023
	(0.731)			
% of crops consumed at Home	-0.039	0.704	0.645	0.718
	(-0.924)			
N	3,765	1,796	1,007	962

NOTE: Estimations use difference-in-difference modeling among panel households. Robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at p < .05. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.

Table 16: Impact of CGP on Livestock Ownership (share)

Dependent Variable	Programme Impact	Baseline	24-Month Treatment	24-Month Control
	(1)	(2)	(3)	(4)
Milk cows	**0.022**	0.056	0.029	0.010
	(2.045)			
Other cattle	**0.144**	0.146	0.220	0.105
	(4.144)			
Goats	0.011	0.021	0.078	0.017
	(1.386)			
Chicken	**0.117**	0.434	0.486	0.372
	(2.324)			
Ducks	**0.026**	0.031	0.047	0.025
	(2.612)			
N	4,591	2,294	1,152	1,145

NOTE: Estimations use difference-in-difference modeling among panel households. Robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at $p < .05$. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.

Table 17: CGP Impacts on Non-farm Enterprises (NFE) (Single Difference)

Dependent Variable	Programme Impact (1)	24-Month Treatment (2)	24-Month Control (3)
HH operates NFE	**0.162**	0.470	0.300
	(4.542)		
Months in operation since Oct/12	0.481	7.580	6.953
	(1.467)		
Total monthly revenue (ZMW)[1]	**88.791**	376.865	246.469
	(3.076)		
Total monthly profit (ZMW)[1]	**34.894**	172.539	125.963
	(2.420)		
Own NFE assets	-0.024	0.316	0.337
	(-0.551)		
Value of owned assets (ZMW)95%	5.394	32.953	22.037
	(1.011)		
N	843	511	332

NOTE: Estimations use single difference modeling. Robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at p < .05. All estimations control for household size, recipient age, education and marital status, districts, and household demographic composition. 1. The highest 5% values for this outcome were discarded due to unlikely large values for this population.

Table 18: 30-Month Impacts of CGP on Housing Characteristics

Dependent Variable	Programme Impact (1)	Baseline (2)	30-Month Treatment (3)	30-Month Control (4)
Iron Sheet Roof	0.017	0.052	0.064	0.057
	(1.276)			
Cement Floor	**0.030**	0.030	0.057	0.022
	(3.893)			
Brick Wall	-0.048	0.324	0.346	0.354
	(-0.862)			
Purchased Lighting	**0.255**	0.572	0.861	0.635
	(6.408)			
Purchased Cooking	**0.060**	0.050	0.139	0.050
	(3.800)			
Clean Water	0.065	0.222	0.316	0.242
	(1.533)			
Own Latrine	**0.153**	0.443	0.668	0.568
	(2.782)			
N	4,814	2,455	1,177	1,182

NOTE: Estimations use difference-in-difference modeling among panel households. Robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates that they are significant at p < .05. All estimations control for household size, recipient age, education and marital status, districts, household demographic composition and a vector of cluster-level prices.