



HHS Public Access

Author manuscript

J Popul Econ. Author manuscript; available in PMC 2019 August 08.

Published in final edited form as:

J Popul Econ. 2016 October ; 29(4): 1083–1111. doi:10.1007/s00148-016-0596-x.

Unconditional Government Social Cash Transfer in Africa Does not Increase Fertility

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Abstract

Among policymakers, a common perception surrounding the effects of cash transfer programmes, particularly unconditional programmes targeted to households with children, is that they induce increased fertility. We evaluate the Zambian Child Grant Programme, a government unconditional cash transfer targeted to households with a child under the age of five and examine impacts on fertility and household composition. The evaluation was a cluster randomized control trial, with data collected over four years from 2010 to 2014. Our results indicate there are no programme impacts on overall fertility. Our results contribute to a small evidence base demonstrating that there are no unintended incentives related to fertility due to cash transfers.

Keywords

Fertility; unconditional cash transfers; Zambia; Africa; J1; I1; I3

1. Introduction

Among policymakers, a common perception surrounding welfare programmes, and unconditional cash transfers (UCTs) in particular, is that transfers targeted to households with young children will incentivize families to have more children (Goldblatt, 2003; Hochfeld & Plagerson, 2011; Holmqvist, 2011; Richter, 2009). Unintended incentives are a possibility when cash payments are given with the qualifying condition of having an infant or pregnant woman in the household. Even when the targeting formula is complex and does not solely link eligibility with young children, the mere perception that transfers are given to

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COMPLIANCE WITH ETHICAL STANDARDS

Conflicts of Interest: The authors declare that they have no conflict of interest.

households with young children may be an incentive for current recipients to have additional children in an effort to maintain eligibility, or for non-beneficiaries to qualify for future payments. These concerns are particularly relevant in poor countries with high fertility where transfers may be a significant proportion of household monthly consumption.

A recent review from the World Bank documents the extraordinary rise of cash transfer programmes, both conditional and unconditional, in the developing world (World Bank, 2015). Such programmes are more established in Latin America and the Caribbean where they tend to be conditional, while in sub-Saharan Africa (SSA), programmes are much newer and, with the exception of the richest southern African countries, not yet taken to scale. Interestingly, the largest programme on the continent is South Africa's Child Support Grant, an unconditional cash transfer programme targeted to poor households with children. This is precisely the kind of programme that would generate objections based on fertility fears, and such concerns were indeed raised in the debate around its introduction (Lund 2008). In Africa, one of the key barriers to the scale-up of UCTs is the notion held by politicians, and even the general public, that such programmes will induce the poor to have more children. The hard evidence on this question is scanty. There are only two published studies based on experimental designs which address the issue (Stecklov, Winters, Todd, & Regalia, 2007; Todd, Winters, & Stecklov, 2012). Stecklov et al. (2007) examined the fertility impacts of three Latin American conditional cash transfers (CCTs): the Honduran *Programma de Asignación Familiar* [Family Assistance Programme (PRAF)]; the Nicaraguan *Red de Protección Social* (RPS) [Social Protection Network] programme; and Mexico's *PROGRESA* (later called *Oportunidades*). They find an increase in the probability of a birth among PRAF beneficiaries, which they conclude is linked to the fact that the size of the transfer could be increased immediately by the birth of a child (a loophole which was subsequently closed). There were no fertility effects of the Nicaraguan RPS or Mexican PROGRESA programmes, where the value of transfers was not immediately affected by the addition of a child. Todd et al. (2012) further examined the Nicaraguan RPS programme and found that it increased birth spacing, a short-run indicator of fertility. The evidence from Africa is based on two studies (one unpublished) which together assess the fertility impacts of the Kenya Cash Transfer for Orphans and Vulnerable Children (CT-OVC), the Malawi Social Cash Transfer Programme (SCTP) (Stecklov & Winters, 2011), and the South African Child Support Grant (CSG) (Rosenberg et al., 2015). None of these studies shows any significant positive impact of the UCTs on fertility. In the case of Kenya, the transfer is flat and independent of household size, while in South Africa¹ and Malawi there is a household cap on the transfer size.

The current study uses evaluation data from the Zambian Child Grant Programme (CGP), a large-scale UCT targeted to households with a child under the age of five and evaluates the impact of transfers on fertility and child-fostering decisions. The overall goal of the CGP is to reduce extreme poverty and break the intergenerational transmission of poverty. The CGP is geographically targeted at rural districts with high rates of poverty and child mortality. It provides a flat monthly cash transfer of approximately USD12 (irrespective of household

¹In the South African CSG, there is no cap on biological children and a cap after six non-biological children.

size) to all qualifying households in programme areas, with no additional eligibility criteria. While influencing fertility is not a goal of the CGP programme, ex-ante expected impacts of a UCT on fertility are ambiguous. The programme may decrease fertility through the ‘quality-quantity’ trade-off, or increase childbearing in order to maintain eligibility in the programme. In the current study, using data from a large multi-site social experiment, we examine the impacts of the CGP on 1) fertility and contraceptive use at the woman-level; 2) count of children aged zero to four years at the household-level; and 3) heterogeneous impacts on these outcomes by woman’s age and whether or not the woman is the intended transfer recipient. The evaluation is a social experiment with data collected at baseline in 2010 and subsequently after 24, 30, 36 and 48 months in 2012, 2013 and 2014. Our results indicate there are no programme impacts on overall fertility, and these findings are robust to a number of different specifications and sensitivity analyses. The results contribute to the small literature that rigorously documents the fertility impacts of UCTs in developing countries. The remainder of this paper is organized as follows: Section 2 presents the theoretical framework and reviews existing empirical evidence; Section 3 describes the programme, data, and analysis methodology; Section 4 summarizes results; and Section 5 concludes.

2. Background

2.1 Economic theory and fertility decisions

In traditional economic models of fertility, couples maximize their utility, which subject to budget constraints they derive from consuming goods (including children) and leisure time, and in doing so make a quantity versus quality trade-off when deciding how many children to have (Becker, 1960; Becker & Lewis, 1974). In this model, children are consumption goods that provide utility to parents (the couple), as well as production goods insofar as they provide monetary income through various forms of labour. Relative preferences of the couple, influenced by religion, age and other cultural factors, determine the shape of the indifference curves. Quality of children in this framework refers to inputs related to children’s care such as where to send a child to school, spending on health care and other investment decisions. As children are considered normal goods, increases in income lead to an increase in the average amount spent on each child, and Becker hypothesized that the quantity elasticity of children demanded is small compared to the quality elasticity (Becker, 1960). This means that increases in income will induce greater investments in children (quality) than increases in quantity of children, and recent empirical work from the United States has confirmed that, within areas with similar housing prices, family size is increasing with husband’s income (Black, Kolesnikova, Sanders, & Taylor, 2013). Further, Becker hypothesized that declines in child mortality would induce corresponding declines in births.

SSA was the last region globally to begin to experience an onset of the fertility transition, however the pace of fertility decline has continued to be slower than that in Asia and Latin America during the 1970s and has even stalled in some countries that have a total fertility rate (TFR) of approximately five (Bongaarts & Casterline, 2013). In an earlier study, Bongaarts hypothesized that poorly performing economies, rising mortality, and lower priorities assigned to family planning programmes over the period studied (1992–2004)

contributed to an apparent stagnation in the fertility decline in the late 1990s and early 2000s (Bongaarts, 2008). Given these contributing factors, as well as the aforementioned perceptions of policymakers regarding fertility incentives of targeted anti-poverty programmes, it is important to examine how large-scale poverty-targeted social protection programmes impact fertility decisions at the household level.

Todd et al. (2012) argue that Becker's framework is useful for examining broader shifts in fertility occurring in demographic transitions, but is less useful in examining short-term fertility outcomes. At the couple level, fertility decisions are made sequentially and are subject to inherent uncertainty (Namboodiri, 1972), biological supply (Easterlin, 1975), and dependent on risks that households face (Cain, 1981). A study from Indonesia suggests that, in the face of risks, children may be a means to smooth consumption (i.e., a negative income shock may cause households to shift from quality to quantity of children (Kim & Prskawetz, 2010)). To this end, cash transfers may impact fertility as couples respond to changes in their economic or social situation by updating their goals for quality and quantity of children (i.e., an income effect) (Todd et al., 2012). Relatedly, period-specific decisions, such as whether to use contraceptives, determine births and these short-run decisions can be altered by perceptions of a link between transfer and presence of children in the household (Stecklov et al., 2007; Todd et al., 2012).

Transfers received by the household alter the cost of having children, and these costs vary based on eligibility requirements (i.e., attendance at health visits and other requirements in CCTs which require additional transportation and/or time costs and eligibility based on number of children in the household in both CCTs and UCTs). Stecklov and colleagues (2007) identify the following as the three key factors which influence the impact of an UCT on fertility: 1) the transfer amount, 2) the manner in which the UCT alters absolute and relative costs associated with having or investing in children, and 3) influence of the programme on supply-side factors (Stecklov et al., 2007). A key assumption in their theoretical framework is that UCT programmes may incentivize increased fertility if they influence total income and the design of the UCT is such that "there is a greater emphasis on reducing the costs associated with the quantity of children relative to the costs of reducing quality" (p 130). Indeed, improvements in the health and education of children as a result of UCT programmes are evidence of increases in the "quality" of children, in Becker's terminology. Preferences for children may also vary over time and are subject to constraints related to fecundity, mother's time and the costs of having children (child care, schooling, etc.). Bor (2013) argues that poorer individuals may partake in greater risks of pregnancy related to unprotected sex, given that the costs of childbearing are borne in the future, and existing evidence that those living closest to subsistence level may have shorter time horizons as short-term needs compete with long-term planning (Lawrance, 1991).

Given the lack of supply-side components of most UCTs in Africa (both conditions related to health and schooling but also a lack of simultaneous investments in health and education infrastructure), the third key factor (supply-side) in the framework of Stecklov et al. is less important in the African context as regards being a mediator (i.e., along the causal pathway) of programme impacts. That is to say, in the Latin American context, where services were generally available and benefits were conditional upon school attendance and health check-

ups, the programmes' impacts on health and schooling outcomes was probably mediated through increased access to services. We argue that in the African context, supply-side factors can moderate programme impacts, and indeed a previous study of Zambia's CGP found positive impacts on skilled attendance at birth in communities where health facilities were available (but no overall impacts (Handa, Peterman, Seidenfeld, & Tembo, 2016), whereas in Latin America, services were more widespread and thus less likely to moderate impacts. Specifically, the supply of contraceptives (including condoms) and other family planning services such as counselling and access to information are important moderators of potential programme impacts.

In addition to preferences regarding total number of children desired, cash transfers may influence the timing of childbearing by encouraging longer birth intervals, which has positive impacts on newborn, child, and maternal health (Rafalitnanana & Westoff, 2000; Rutstein, 2005). Birth spacing may increase due to increased contraceptive use (described above) or through increases in exclusive breastfeeding (if programmes impact infant feeding practices), which leads to lactational amenorrhea, particularly during the first six months post-partum (Vekemans, 1997). Previous research has demonstrated impacts of cash transfers on infant and young child feeding practices (Macours, Schady, & Vakis, 2012), but to our knowledge none have examined impacts on exclusive breastfeeding. It is possible that CCTs in Latin America would be more likely to influence breastfeeding practices as compared to typical UCTs in Africa, given complementary components of the programme including health and nutrition information sessions. In addition, since breastfeeding is widespread in Africa, there is less variation and opportunity for programme impact.

Finally, if cash transfers increase the ability of women to avail themselves of existing health services (through increased ability to afford transport and service-related costs), then they may increase women's and couples' ability to achieve desired family size. Use of modern contraceptives not only helps couples achieve ideal family sizes, but also has broad, positive impacts on perinatal outcomes, child survival, and maternal health (Ahmed, Li, Liu, & Tsui, 2012; Cleland, Conde-Agudelo, Peterson, Ross, & Tsui, 2012). In Zambia over one in four women (28%) aged 35 years or over reported having more children than their ideal (Upadhyay & Karasek, 2012), and approximately 35% of all pregnancies are unintended in Africa (Sedgh, Singh, & Hussain, 2014). According to Demographic and Health Survey (DHS) data from 2007, the ideal number of children was 5.1 and 5.7 among women and their partners, respectively, in Zambia (Upadhyay & Karasek, 2012). By 2013, the ideal number of children was 4.7 as reported by women compared to 5.0 among men (Central Statistical Office (CSO) et al., 2014). Therefore, irrespective of other pathways, there exists an opportunity of an 'income effect,' whereby if income constrains use of contraceptives, women may increase their use of family planning due to the transfer programme.

2.2 Existing evidence on cash transfers and fertility

Given the critical importance of the early childhood environment in determining nutritional and educational outcomes (Dewey & Begum, 2011; Leroy, Ruel, Habicht, & Frongillo, 2014), cash transfers often target households with very young children. In various settings, this has led to concern that such programmes will incentivize more childbearing (Richter,

2009; Wilcox, Robbennolt, O’Keeffe, & Pynchon, 1996). However, studies from Kenya, Malawi, and South Africa (UCTs), as well as in Mexico and Nicaragua (CCTs) have found no impacts of cash transferson fertility (Rosenberg et al., 2015; Stecklov & Winters, 2011; Stecklov et al., 2007; Todd et al., 2012). Only two studies to date have suggested a positive link between transfers (both CCTs) and fertility. These include a study examining the PRAF in Honduras, which led to a two to four percentage point increase in the probability of a birth over the two-year period examined (Stecklov et al., 2007), and a study using non-experimental data to compare households who were enrolled early (1997/1998) in *Oportunidades* in Mexico (called PROGRESA at the time) versus those enrolled later (2004 or later), finding a five per-cent increase in fertility over pre-programme levels (Arenas, Parker, Rubalcalva, & Teruel, 2015). In the Honduran programme, the amount of the transfer could be increased post-enrolment by the addition of a pregnant woman or new child (for a brief period until this loophole was closed), and thus the behavioural link is stronger.

The Mexican study utilized a somewhat selected sample (i.e., only non-indigenous women living in rural communities with low and medium levels of community poverty) in order to construct a reasonable comparison group, and this may decrease generalizability of the findings, particularly if cash transfers are targeted in communities with high levels of poverty, where dynamics surrounding fertility decisions as well as access to contraception may be different. Further, during the course of Brazil’s *Bolsa Familia* programme, a CCT launched in 2003 which by 2015 covered approximately 14 million households (50 million individuals) (Falcão Silva, 2015), or approximately 25% of the Brazilian population, total fertility rates dropped from 2.35 in 2000 to 1.90 in 2010 nationally according to census data. Larger decreases were found among the poorest households, including a decrease from 5.10 to 3.56 among those with monthly per capita household income less than 70 reales, and from 3.47 to 3.64 among those with monthly per capita household incomes of 70–140 reales (Ministry of Social Development and Fight against Hunger, 2012). This suggests that a national CCT covering a quarter of the population did not reverse downward fertility trends; however, the causal relationship cannot be identified, as no experimental study was conducted to examine fertility impacts.

An evaluation of South Africa’s CSG – the most similar to the programme examined in our analysis due to its unconditional nature and targeting criteria based on presence of a child in the household (target age ranges differ and the CSG is means-tested) – found that receipt of the transfer did not increase pregnancy rates, and, in fact, recipients had lower second pregnancy rates than non-recipients (Hazard rate=0.662; 95% CI: 0.58, 0.75) (Rosenberg et al., 2015). Existing evaluations of impacts of the CSG on adult women’s fertility lacked a randomized experimental design due to the fact that the programme has been scaled up for a long period of time (though eligibility criteria have changed), and the aforementioned study used quasi-experimental matching methods to evaluate programme impacts, namely inverse probability weighting and marginal structural models. In other countries in the region, evaluations of Kenya’s CT-OVC and Malawi’s SCTP using experimental cluster randomized designs found no impacts on childbearing as measured by presence in the household of children aged 0–17 years (surveys did not ask women directly about fertility as it was unrelated to programme objectives) using difference-in-differences (DD) estimators and propensity score matching (Stecklov & Winters, 2011). Furthermore, one of the

aforementioned studies also examined contraceptive use, and did find (somewhat mixed) evidence to suggest that CCTs increased contraceptive use in Mexico and Nicaragua (Stecklov et al., 2007).

2.3 Impacts on fertility-related behaviours of adolescents in cash transfer households

Finally, it is worth noting that cash transfers may impact overall fertility by delaying sexual debut and marriage of adolescents in beneficiary households. A growing body of evidence has recently demonstrated this potential both for CCTs and UCTs across Latin America and SSA (Gulemetova-Swan, 2009; McQueston, Silverman, & Glassman, 2013; Stecklov et al., 2007). Kenya's CT-OVC led to delayed sexual debut and first pregnancy, but no delays in marriage among young people aged up to 25 years (Handa, Halpern, Pettifor, & Thirumurthy, 2014; Handa et al., 2016), and South Africa's CSG was found to be protective against sexual debut and pregnancy among females (Bor, 2013; Heinrich, Hoddinott, & Samson, 2015; Heinrich et al., 2012). Further, an evaluation of a scale-up of the Malawi SCTP found that among poorer households (defined at baseline), but not in the overall sample, the programme reduced the probability of pregnancy by 4 percentage points among females aged 13 to 19 at baseline (Malawi SCT Evaluation Team, 2015). Finally, a pilot cash transfer programme (with conditional and unconditional treatment arms) in Malawi (*Zomba Cash Transfer Programme*), which distributed cash transfers to female adolescents and their households, found that the transfers led to delays in marriage, pregnancy, and sexual debut, however these findings varied by group examined (in-school versus drop-outs at baseline – findings were driven by the latter group), as well as the unconditional versus conditional transfers; impacts on reduced marriage and pregnancy were only found in the UCT treatment arms (S. Baird, Chirwa, McIntosh, & Ozler, 2010; S Baird, McIntosh, & Özler, 2011) (S. Baird et al., 2010; S Baird et al., 2011).

2.4 Study aims and hypothesis

In the current study, we test whether a large, government-run UCT in Zambia targeted to households with young children increased childbearing over a four-year period. We hypothesize that women may increase their childbearing in response to receiving cash benefits if they believe that eligibility is determined by maintaining very young children in the household or if they believe the transfer amounts may increase with more children. This increase in childbearing could be either a quantum effect (i.e., women increase the total number of children demanded) or a tempo effect, whereby women shorten birth intervals. Furthermore, women who do not yet have children but are living in households receiving cash may initiate childbearing in an effort to secure their own cash benefits.

3. Data and Methodology

3.1 Description of the Child Grant Programme (CGP)

In 2010, the Zambian Ministry of Community Development, Mother and Child Health started implementing the CGP, with the goals of reducing extreme poverty and breaking the inter-generational cycle of poverty. Specific objectives of the programme included supplementing household income, increasing school attendance, reducing under-five mortality and morbidity, increasing household asset ownership, and increasing the number

of households having a second meal per day (American Institutes for Research, 2011). The CGP was implemented in three districts (Kalabo, Shangombo, and Kaputa) with high rates of mortality, morbidity, stunting and wasting among children aged zero to three years. Households with a child aged zero to five years at programme initiation were targeted for the programme, and transfers were distributed bi-monthly to the primary female adult in the household caring for a child in the targeted age range. The transfer was a fixed monthly sum of approximately USD 12 irrespective of household size, an amount deemed sufficient to purchase on average one meal a day for everyone in the household for one month. Households “age-out” or graduate from the programme after the index child turns five. Officially, a CGP household would re-qualify when the index child has a sibling under age five years. However, in practice this was not applied consistently, and very few households re-qualified this way. Further, relatively few children turned five between end-2010 (start of programme) and end 2013. Graduation began in earnest in 2014 and then the government decided to stop any enrolment into the CGP. Graduation continues in 2015 but without new enrolments into the CGP, which is gradually being phased out. To date, findings from a rigorous impact evaluation of the programme have demonstrated strong impacts on consumption, food security, asset ownership, school attendance, agricultural production, and operation of non-farm enterprises.

3.2 Data

An impact evaluation using an experimental design accompanied the CGP in order to understand impacts on the target population and provide evidence for future design and expansion of the programme. The evaluation was a collaboration between the University of North Carolina at Chapel Hill and American Institutes for Research (AIR) as part of the Transfer Project. The evaluation design took advantage of the government’s planned roll-out of the programme, and the unit of randomization (cluster) was the lowest level of programme administration, the Community Welfare Assistance Committee (CWAC). In total, 90 CWACs were randomly assigned, half (45) to treatment (starting programme in December 2010) and half (45) to comparison (however, delayed entry, which was intended to begin at the end of 2013, was delayed until 2015, when the CGP underwent changes to the targeting formulas). In October to November 2010, a baseline survey among a random sample of 2,515 beneficiary and non-beneficiary households was carried out, and follow-up surveys were conducted in October to November 2012 (24 month); June to July 2013 (30 month), October to November 2013 (36 month), and October to November 2014 (48 month). The 2,515 households at baseline provided an evaluation sample of 14,565 individuals (4,793 children aged five and under). The sample size was chosen to detect hypothesized effects for child anthropometry outcomes among children under five years of age, accounting for attrition and non-response rates. Household, community, and health facility questionnaires were administered at baseline, and follow-up household and community questionnaires were administered at 24, 30, 36, and 48 months. Household-level surveys were conducted with one main respondent (the intended CGP recipient) and included detailed modules on socioeconomic status, wealth and production activities, early childhood development, women’s empowerment, and anthropometry for all children under five years. Further, information on women’s fertility history was collected for all women aged 12 to 49 years residing in the household at 24, 36 months, and 48 months, as reported by the main

respondent. Additionally, at the 36 and 48 month follow-up surveys, information on current use of modern contraceptives was collected for the main survey respondent (one woman per household). Community-level (all waves) and health facility (baseline only) surveys were implemented to measure supply of health, education and other services and infrastructure.

Approximately 95 per cent of households in the sample fall below the extreme poverty line, compared to 85 per cent of households in the sample districts as reported in the 2010 Living Conditions and Monitoring Survey. Median household size was five individuals, and the transfer amount represented approximately 27 per cent of household monthly expenditures at baseline (American Institutes for Research, 2011).

In the 24-month follow-up a module on programme operational performance was administered. Respondents were asked about their perceptions of programme qualifying characteristics and continued receipt of the transfer. Approximately 64 per cent of respondents in both treatment and control households combined correctly identified eligibility criteria for the CGP (i.e., caring for a child under the age of five years; results not shown). Therefore, we can assume that around two-thirds of the sample is aware that having a young child in the household is the main reason they are eligible for the transfer. Among recipients, there was some mixed understanding of the cash transfer rules (conditions or co-responsibilities) related to continuing support; however the percentage of respondents recognizing there are no conditions was 87% after 24 months, 82% after 36 months, and 72% after 48 months. Among those who thought there were co-responsibilities, the most commonly cited were maintaining adequate nutrition for children and clothing them cleanly and appropriately (American Institutes for Research, 2015).

Information collected at baseline from the health facilities highlighted the extremely weak health services available in these communities. Only six per cent of the facilities have electricity, less than 10 per cent have clean water, and only one-quarter have a laboratory for testing. With respect to staffing, only 10 per cent of facilities have a registered nurse, and no facility had a full-time medical doctor. Furthermore, drug availability was low, with oral rehydration salts (ORS) available in only 45 per cent of facilities (American Institutes for Research, 2011).

Ethical review of this study was obtained by AIR in Washington, DC and the University of Zambia's Research Ethics Committee, and informed consent was obtained from all study participants. Questionnaires and reports for the CGP are available on the Transfer Project website (<http://www.cpc.unc.edu/projects/transfer>).

3.3 Measures

Our primary outcome for analysis is total fertility, or total number of children ever born to a woman. Main respondents reported on this outcome for all women in the household aged 12 to 49 years through a series of questions, including how many children born to the woman currently live in the household, how many children born to the woman currently live elsewhere, and how many have died. Additional questions assessed our secondary outcomes, including whether the woman is currently pregnant and has she had a pregnancy that miscarried, was aborted, or ended in stillbirth. We examine this combined outcome

(miscarriage/abortion/stillbirth) as it may be an indicator of pregnancy intentions (however unrealized). For example, if women are actively trying to get pregnant to maintain, increase, or obtain cash benefits, we may see an increase in miscarriage and stillbirth as pregnancies increase overall.

The hypothesized impacts on abortion are ambiguous because cash may increase access to facilities where abortions can be obtained, or they may also increase access to contraception, thus decreasing the need for abortions. We are unable to examine these outcomes separately, as they were asked in combination in the questionnaire, due to the sensitive nature of and stigma surrounding miscarriage, abortion, and stillbirth. Finally, we created a variable indicating ever been pregnant if the answer to any of these questions was 'yes' or if total fertility was reported as greater than zero. Responses to the question of miscarriage, abortion, or stillbirth may suffer from under-reporting due to the sensitive nature of this topic, but we have no reason to believe under-reporting would have systematically differed between treatment and control households. Finally, in 36 and 48 month follow-up surveys, main respondents were asked about their current modern contraceptive use; this information is not available for other females in the household.

We use additional individual- and household-level characteristics as measured at baseline in our analysis. These include highest educational attainment (in years), marital status (never married, separated or divorced, married or co-habiting), log of household size (continuous), district (Kaputa, Shangombo, and Kalabo), log of distance to nearest food market (kilometres), and contemporaneous prices for a basket of commonly-used goods (maize, rice, beans, oil, sugar, salt, soap).

3.4 Methodology

The key evaluation challenge for examining fertility outcomes is that complete fertility information was not collected at baseline. Thus, rather than implementing a DD approach, we rely on the successful randomization of the programme and conduct cross-sectional analysis only. Findings from the baseline evaluation report indicate successful randomization (American Institutes for Research, 2011). Attrition analyses were also conducted to examine general attrition and selective attrition based on individual- and household-level characteristics at baseline.

To examine the total number of children ever born alive, we used Poisson regression models given the count distribution of number of births, which are non-negative and right skewed. The Poisson model is specified as follows (Verbeek, 2008):

$$P\{y_i = y|x_i\} = \frac{e^{-\lambda_i} \lambda_i^y}{y!} \quad (1)$$

where y_i indicates number of live births at for individual i and $\lambda_i = \exp\{X\beta\}$, X is a vector of covariates, and β is a vector of parameters to be estimated. The same control variables for equation (2) were used in these models. Appropriateness of the Poisson models was tested

using the Pearson goodness-of-fit test (estat gof command in Stata 14.1) and was found to be appropriate.

Next, we examined differences cross-sectionally between women of reproductive age residing in control and treatment households at 24, 36 and 48 months with respect to the following fertility-related outcomes using multivariate linear probability models: 1) currently pregnant, 2) ever been pregnant, and 3) ever had pregnancy ending in miscarriage/abortion/still birth. These models were specified as follows:

$$Y_i = \beta_0 + \beta_1 TREAT + X\beta + \varepsilon_i \quad (2)$$

where Y_i is the outcome of interest for woman i , $TREAT$ is a binary indicator equal to 1 if the woman lives in a treatment household and equal to zero otherwise, X is a vector of covariates, β is a vector of parameters to be estimated, and ε_i is the error term. Controls used in these models included woman's current age, highest educational attainment at baseline, marital status at baseline, log of household size at baseline, district, log of distance to nearest food market at baseline, and a basket of contemporaneous prices (maize, rice, beans, oil, sugar, salt, toilet soap). We use control characteristics measured at baseline to account for the possibility that the programme affected background characteristics. Although we collected fertility information for women aged 12 to 49, we limit our analysis to women aged 15 to 49, both for comparability of fertility outcomes with existing studies and DHS, and because fertility of girls 12 to 14 is a relatively rare event in our sample.

Next, among main respondents in the household surveys, we examined current use of modern contraception at the 36 and 48-month follow-up waves using linear probability models (contraception information was not collected before 36 months). We conduct several robustness checks, including running our analysis separately on target beneficiary women (*i.e.*, the identification card holder for the cash transfer recipient within the household) and other non-beneficiary women in both treatment and control households (average ages were 30 and 20 years, respectively, at the 24-month follow-up), as well as among women aged 24 years and under at each wave. The former is to test if impacts vary based on whether or not the specific woman in question is receiving cash directly versus an overall income effect within the household. The latter is to test if younger women aged 15 to 24 have differential impacts, which are likely to be a result primarily of delaying first births.

Finally, as a robustness check, we built a household-level panel with variables indicating the total number of children in each household aged 0–1 years and 2–4 years. We also created these variables sex-segregated. We conducted DD analyses using Poisson models for each of the aforementioned indicators. For these analysis, the appropriateness of Poisson models was again tested and found to be adequate. Household-level controls used in these models included baseline values for main respondent's current age, highest educational attainment at baseline, marital status at baseline, log household size, district, log of distance to nearest food market and a basket of contemporaneous prices.

4. Results

4.2 Summary statistics

Individual-level attrition analysis by wave are presented in Appendix 1. Panel A indicates that, in the 24-month panel, there is no evidence of selective attrition. Panels B and C indicate that there is some evidence of selective attrition based on highest school grade attained. Treatment women lost to follow-up had higher levels of education at baseline (3.4 years v. 2.6 among control).

Summary characteristics of women in our analysis sample are presented in Table 1. The average age of women studied was 28, ranging from 15 to 49 years. Approximately 60 (59) per cent of treatment (control) women were married, and 28 (27) per cent of treatment (control) women were never married at baseline. We see statistically significant differences at baseline for being separated or divorced (approximately 12 and 15 per cent of women in treatment and control households were separated or divorced; $p < 0.05$) and highest grade attained (3.4 years among control women and 3.8 years among treatment women; $p < 0.05$).

Table 2 presents means of fertility and related outcomes over the three waves examined. Columns 1 through 4 are for the overall sample for analysis, by treatment arm, and Columns 5 through 8 refer to the sub-sample of main respondents (intended recipients). Given that each wave was a cross-sectional analysis, girls younger than 15 years subsequently “aged into” the sample analysed across waves, and since the relative number of young women is larger compared to older women (so more were aging in than aging out), the sample size grew over time (from 2,669 in the 24-month survey to 3,025 in the 48-month survey). This also means that lifetime experience of some means (such as currently and ever pregnant) decreased over time since girls of 15 to 17 have often not started childbearing. At 24 months, average total number of children ever born to women in our sample was 3.24 in the sample (3.32 among control women and 3.15 among treatment women; $p > 0.10$). Among control and treatment women, respectively, means were 3.24 and 3.13 at 36 months ($p > 0.10$) and 3.31 and 3.28 at 48 months ($p > 0.10$). By 24 and 36 month follow-ups, treatment women were four and three percentage points less likely to have ever been pregnant ($p < 0.05$), but this difference was no longer significant at 48 months. At 24 and 48 months, treatment women reported being less likely to have had an abortion, miscarriage or stillbirth (3 and 2 percentage points, respectively; $p < 0.05$). As expected, Columns 5 through 8 indicate that total fertility among beneficiaries is higher than the means for all women in the full sample (3.98 and 3.91 among control and treatment women, respectively, at 24 months; means at 36 months were 4.14 and 4.16, respectively and at 48 months were 4.28 and 4.37, respectively; $p > 0.10$ for all waves). At 36 months, 39 per cent of control and 37 per cent of treatment women were using a modern method of contraception ($p > 0.10$), and this rose to 51 and 54 per cent, respectively, at 48 months ($p > 0.10$). These dramatic increases in a short period were mirrored in national statistics estimated from population-based DHS, though our estimates are slightly higher than reported levels of contraceptive use from DHS in the provinces studied.ⁱⁱ

ⁱⁱAccording to DHS conducted in 2003 and in 2007, in provinces where study districts were located, modern contraceptive use among currently married/co-habiting women increased from 16.9 to 32.5% in Northern Province and from 23 to 31.7% in Western Province.

4.3 Programme impacts on fertility outcomes

In multivariate Poisson regression models, we find no positive programme impacts at any wave on total number of births to women (Table 3, Column 1). These findings are robust to additional analyses, including among main respondents only (Column 5) and other women in treatment and control households (Appendix 2).ⁱⁱⁱ Table 4 (Column 1) indicates that the programme had a negative impact ($p < 0.10$) on fertility among women aged under 25 years at 36 months (young women in treatment households had 10 per cent fewer births [$\exp(-0.107) = 0.90$] compared to those in control households, however impacts were not significant at 24 or 48 months. Table 3 shows there are negative programme impacts on ever having been pregnant at 24 months (treatment women were 2.5 percentage points less likely to have ever been pregnant; $p < 0.10$) and on ever having had an abortion/miscarriage/stillbirth at 24 and 48 months ($\hat{\beta} = -0.027$, $p < 0.10$ at 24 months; $\hat{\beta} = -0.021$, $p < 0.05$ at 48 months) using linear probability models among the full sample.

Table 3 (Column 6) indicates that among beneficiary women, treatment equated with 2.9 percentage points more likely to be pregnant at the 24-month follow-up than control women, however this finding was only marginally significant ($p < 0.10$) and disappeared at later waves. The only other significant impact among main respondents was a negative effect on the probability of having ever had a miscarriage/stillbirth/abortion at 48 months ($p < 0.001$). Among other non-beneficiary women in treatment households, at 24 months, women were 2.5 percentage points less likely to have had a miscarriage/abortion/stillbirth ($p < 0.05$), however this impact also disappeared at later waves (Appendix 2).

4.4 Programme impacts on contraceptive use

Next we turn to an analysis of contraceptive use, a proximate determinant of fertility (Table 3, Column 9). By 48 months, contraceptive use had increased dramatically among both groups in our sample (from 37 to 54 per cent among treatment women and from 39 to 51 per cent among control women; Table 2, Columns 6 and 7), and this is in line with overall increases in contraceptive use at the national level. We find no programme impacts on modern contraceptive use at 36 or 48 months (data were not collected at earlier waves to test for impacts).

4.4 Programme impacts on household composition

Household-level attrition analyses are presented in Appendix 3. Across 24-, 36- and 48-month follow-up there is no evidence of selective attrition based on individual- and household-level characteristics, with the exception of baseline household size, as compared between treatment and control households which attrited by 48 months ($p < 0.05$; control households that attrited were larger at baseline than treatment households that attrited). There was no evidence of selective attrition based on this indicator at 24- or 36-month follow-up waves.

Nationally, modern contraceptive prevalence rates (CPR) increased from 32.7% in 2007 to 45% in 2013 among currently married/cohabiting women.

ⁱⁱⁱSample sizes for fertility-related outcomes differ due to missing values for currently pregnant and ever had a miscarriage, stillbirth or abortion. We calculated ever pregnant based on whether any of the fertility information was provided.

Appendix 4 presents summary statistics for the number of children aged 0 to 4 years living in households by treatment arm (Panel A). Panels B and C present these statistics disaggregated by gender. The mean number of children aged 0 to 1 decreases over time, as expected. The average number of children aged 0 to 4 years at baseline was 1.92 and 1.88 in control and treatment households, respectively, and 1.55 at 48 months in both treatment and control households.

Table 5 presents DD estimates from our household-level analysis of total number of children aged zero to four years living in the household using Poisson regression models. There were no impacts on total children zero to four years at any wave in the analyses where we examined males and females together (Table 5, columns 1–3). This is consistent with our findings above that the programme had no impact on fertility in the individual-women level analyses. We also replicated this analysis on sex-disaggregated variables (Columns 4–9) and found a marginally significant positive impact on the number of female children aged zero to four years after 36 months ($\hat{\beta}=0.06$, $SE=0.036$; Column 5) but no impacts after 24 and 48 months, nor any impacts among boys.

We further broke down this analysis into age groups (zero to one years and two to four years; Appendix 5, Columns 1–3), and the programme had no impact on total number of children aged zero to one year living in treatment households at any of the waves examined (Appendix 5, Panel A). Appendix 5, Panel B (Column 2) indicates that the programme had a marginally significant, positive impact on the total number of children aged two to four years living in the households at the 36-month wave ($\hat{\beta}=0.09$, $SE=0.053$); this translates to approximately 9 per cent more children in this age group in treatment households [$\exp(-0.0751)=1.09$]. There were no significant impacts on number of children aged 2 to 4 years in households at 24 or 48 months (Columns 1 and 3). In age- and sex- disaggregated analyses (Appendix 5; Columns 4–9), there was a marginally significant, positive impact on females aged two to four years living in the households at 24 months ($\hat{\beta}=0.13$, $SE=0.072$; Appendix 5, Panel B, Column 4) and a positive impact on the number of females at 36 months ($\hat{\beta}=0.18$, $SE=0.082$; Appendix 5, Panel B, Column 5). There were no programme impacts on the number of boys in this age group.

Finally, when examining total count of orphans aged 0 to 4 years in households, we found no programme impacts on total number of orphans living in households. However, total orphan counts (aged 0 to 4 years) in these households were very low (0.13 total per household; results not presented; available upon request).

5. Discussion and Conclusion

Cash transfers, particularly UCTs, are rapidly expanding across Africa as an important tool in governments' social protection strategies. Positive impacts on a range of outcomes have been demonstrated, however countries continue to struggle with targeting and other design decisions. A critical barrier to scaling up programmes is policymakers' fears that transfers may create unintended incentives to increase childbearing, in settings where poverty and fertility are high. In this study we have demonstrated, using a large-sample randomized control trial that an UCT targeted to households with children under the age of five had no

impacts on fertility over a four-year period. Among young women under 25 years, fertility actually decreased after 36 months, but this finding was only marginally significant and did not hold up after 48 months. This is the first study to evaluate fertility impacts of an UCT as reported from fertility histories of individual women in Africa using an experimental design. Existing studies have used either household roster information, thus missing out on children who may have died or been stillborn (Kenya and Malawi), or were conducted using non-experimental methods (South Africa). Our findings are consistent with existing literature from Kenya, Malawi, South Africa, Mexico, and Nicaragua, which also found that SCTs did not increase fertility (Rosenberg et al., 2015; Stecklov & Winters, 2011; Stecklov et al., 2007). Robustness checks found no programme impacts on the overall number of children aged 0 to 4 in these households.

From a theoretical perspective, our results suggest the income effect on the demand for quantity of children is counterbalanced by other factors, including ability to achieve smaller ideal family sizes or increases in investments (quality) in existing children. Income shocks such as UCTs may increase demand for quality of children, or alternatively it may allow women to achieve their existing preferences for fewer children, and both may induce couples to increase contraceptive use. However, if supply constraints are binding (i.e., if there is no availability of family planning at facilities or existence of staffed facilities), then programme impacts may be limited in reaching their full potential. As a nation, Zambia appears to have made great strides in addressing unmet need for contraception. Use of modern contraceptive methods has increased dramatically in Zambia over the past seven years, and thus changes in contraceptive use may be largely unrelated to the programme and instead attributed to factors at a national level. According to the Zambian DHS, modern contraceptive use among women aged 15 to 49 increased in the study areas, from 16.9 to 32.5 per cent in Northern Province and 23 to 31.7 per cent in Western Province in 2007 and 2013/2014, respectively (Central Statistical Office (CSO), Ministry of Health (MOH), Tropical Diseases Research Centre (TDRC), University of Zambia, & Inc., 2009; Central Statistical Office (CSO) et al., 2014).

From a policy perspective, our findings are important because they demonstrate that a social protection programme targeted to households with young children does not create the unintended consequence of increased fertility. Investment in early childhood is critical, as this period is a key development window, where chronic stressors (including poverty) can induce significant biological changes with implications for individuals' future trajectories (Danese & McEwen, 2012). Further, increasing evidence shows the importance of early environment on the development of adolescent and adult cognitive and non-cognitive skills, the former of which is a strong determinant of wages, schooling, and social and economic success, while the latter also directly impacts wages and schooling, but also impacts teenage pregnancy, smoking, crime and other aspects of social and economic life (Heckman, 2007).

A limitation to our study is that we do not have complete fertility information as reported by women at baseline. This prohibits the use of DD models in our fertility related analysis. However, we do have information at all waves on number of children in the households based on the rosters, and we have replicated our analysis at the individual- and household-levels and the two analysis approaches show consistent results. Another limitation is that our

fertility histories for all women aged 12 to 49 years was reported by one main (female) respondent in the household. The fertility information for women not directly reporting their own histories may be less reliable than for the main respondents. However, these concerns are assuaged by the fact that we found consistent results between both groups when examined separately, and that this is a common practice for large household surveys collecting a roster of fertility information. A limitation to our analysis of contraceptive use is that we only ask one question about current contraceptive use (“Are you currently using any method to limit or space your births?”) and record response to which type of method the respondent is currently using, if applicable. DHS and other surveys focused on reproductive health may use more comprehensive modules to assess contraceptive use, first asking about knowledge of methods and then prompting for use of a variety of types.

In conclusion, we have demonstrated that a large-scale government UCT programme targeted to households with young children in a high poverty and high fertility setting does not increase fertility. These findings should assuage the fears of policymakers who want to invest in this key development window for children but fear unintended consequences as a result of the programme. This conclusion is based on mean differences comparing groups of a randomized treatment and comparison women. Therefore, it is still possible that there are individual women who hold the belief that they must have additional children to maintain or gain eligibility; however, on a population level, there is no evidence of this dynamic after four years of programme implementation. A key issue in the manifestation of unintended incentives within programmes may be linked to programme enrolment and extension rules, as well as overall programme sensitization efforts. If beneficiaries know the objectives and rules around programme qualification—particularly if these are fixed at enrolment without the possibility to increase benefits by subsequently increasing number of children in the household, programmes may be more able to protect against unintended incentives. Future research should examine longer-term impacts in various settings, including potential protection effects of the programme among adolescent first pregnancy and transition to adulthood in beneficiary households.

Acknowledgments:

We thank Molly Rosenberg, Guy Stecklov, and three anonymous reviewers for their help and guidance in reviewing this manuscript, as well as our funders. The Child Grant impact evaluation was commissioned by the Government of Zambia (GRZ) through the Ministry of Community Development, Mother and Child Health to the American Institutes for Research and the University of North Carolina at Chapel Hill and funded by a consortium of donors including DFID, UNICEF, Irish Aid, and the Government of Finland. Palermo, Handa, and Peterman received additional funding from the Swedish International Development Cooperation to the UNICEF Office of Research - Innocenti for analysis of the data and drafting of the manuscript.

The Child Grant impact evaluation was commissioned by the Ministry of Community Development, Mother and Child Health, Government of Zambia (GRZ) through the Ministry of Community Development, Mother and Child Health to the American Institutes for Research and the University of North Carolina at Chapel Hill, and funded by a consortium of donors including DFID, Irish Aid, and the Government of Finland. The results that appear in this article are the culmination of over four 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.

Appendix

Appendix 1.

Individual-level Attrition Analysis

Panel A. Differences in Overall Attrition by Treatment Status at 24 Month Follow-up								
	Control			Treatment			Difference	
	Attrited	Panel	P-value	Attrited	Panel	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age in years	24.33	26.20	0.01	24.46	26.09	0.05	-0.13	0.90
Highest grade attained	2.99	3.33	0.23	3.28	3.81	0.05	-0.29	0.45
Divorced/separated/ widowed	0.15	0.15	0.77	0.17	0.11	0.03	-0.02	0.69
Never married	0.45	0.27	0.00	0.50	0.28	0.00	-0.05	0.45
Log (household size)	1.83	1.72	0.01	1.93	1.74	0.00	-0.10	0.16
Shangombo District	0.23	0.36	0.00	0.26	0.35	0.05	-0.03	0.75
Kaputa District	0.55	0.28	0.00	0.50	0.28	0.00	0.05	0.71
Log (distance to food market)	2.45	2.42	0.82	1.90	2.11	0.29	0.56	0.11

Panel B. Differences in Overall Attrition by Treatment Status at 36 Month Follow-up								
	Control			Treatment			Difference	
	Attrited	Panel	P-value	Attrited	Panel	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age in years	24.26	26.11	0.02	25.21	25.89	0.39	-0.95	0.36
Highest grade attained	2.62	3.36	0.00	3.40	3.76	0.14	-0.78	0.05
Divorced/separated/ widowed	0.15	0.15	0.75	0.18	0.11	0.02	-0.02	0.53
Never married	0.55	0.26	0.00	0.48	0.29	0.00	0.07	0.15
Log (household size)	1.89	1.72	0.00	1.92	1.75	0.00	-0.03	0.68
Shangombo District	0.27	0.35	0.03	0.26	0.35	0.03	0.01	0.90
Kaputa District	0.44	0.30	0.00	0.39	0.31	0.15	0.05	0.69
Log (distance to food market)	2.28	2.44	0.29	1.88	2.10	0.11	0.40	0.23

Panel C. Differences in Overall Attrition by Treatment Status at 48 Month Follow-up								
	Control			Treatment			Difference	
	Attrited	Panel	P-value	Attrited	Panel	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age in years	25.11	26.06	0.24	25.22	25.93	0.28	-0.11	0.91
Highest grade attained	2.64	3.40	0.00	3.40	3.79	0.11	-0.76	0.04
Divorced/separated/ widowed	0.15	0.15	0.97	0.16	0.11	0.11	-0.01	0.80
Never married	0.53	0.25	0.00	0.52	0.27	0.00	0.01	0.88
Log (household size)	1.92	1.70	0.00	1.93	1.74	0.00	-0.01	0.86
Shangombo District	0.36	0.34	0.58	0.32	0.34	0.64	0.04	0.73

Panel A. Differences in Overall Attrition by Treatment Status at 24 Month Follow-up								
	Control			Treatment			Difference	
	Attrited (1)	Panel (2)	P-value (3)	Attrited (4)	Panel (5)	P-value (6)	Col(1)-Col(4) (7)	P-value (8)
Kaputa District	0.36	0.31	0.34	0.39	0.30	0.16	-0.04	0.77
Log (distance to food market)	2.19	2.47	0.02	1.82	2.13	0.06	0.36	0.25

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.

Appendix 2:

Impact of CGP on women's total fertility and related outcomes, Other Women in Household

Panel A: 24 months				
	Total fertility (1)	Currently pregnant (2)	Ever been pregnant (3)	Ever had miscarriage stillbirth or abortion (4)
Treatment	-0.223 (1.04)	-0.001 (0.10)	-0.038 (1.06)	-0.025 (2.26)**
R^2	-	0.03	0.31	0.06
N	568	568	568	568
Panel B: 36 months				
Treatment	-0.196 (1.26)	-0.014 (1.19)	-0.054 (1.91)*	-0.003 (0.29)
R^2	-	0.03	0.3	0.04
N	773	770	773	772
Panel C: 48 months				
Treatment	0.178 (0.85)	-0.009 (0.75)	0.023 (0.78)	-0.012 (1.53)
R^2	-	0.02	0.23	0.05
N	780	776	780	779

Notes: Robust t-statistics in parentheses.

* $p < 0.1$

** $p < 0.05$;

*** $p < 0.01$.

All models control for baseline values of log (household size), log (distance to food market), Total fertility models run with Poisson models; all other outcomes use linear probability models.

highest grade attained, marital status, basket of prices, and current age.

Appendix 3.

Household-level Attrition Analysis

Panel A. Differences in Overall Attrition by Treatment Status at 24 Month Follow-up

	Control			Treatment			Difference	
	Attrited	Panel	P-value	Attrited	Panel	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Maize grain price	21.66	22.03	0.81	24.31	25.57	0.35	-2.65	0.27
Rice price	3.75	3.89	0.42	2.89	3.35	0.06	0.87	0.01
Bean price	3.30	3.97	0.03	3.49	4.13	0.08	-0.19	0.73
Cooking oil price	10.89	10.82	0.73	10.86	11.03	0.41	0.03	0.93
Sugar price	7.61	7.56	0.68	8.01	7.85	0.49	-0.40	0.35
Table salt price	4.22	3.89	0.68	4.09	3.93	0.84	0.13	0.93
Toilet soap price	5.01	5.26	0.07	5.03	5.13	0.57	-0.03	0.92
Log household size	1.67	1.66	0.85	1.65	1.68	0.59	0.03	0.73
Shangombo	0.12	0.35	0.00	0.15	0.35	0.00	-0.03	0.73
Kaputa	0.73	0.29	0.00	0.71	0.30	0.00	0.02	0.88
Log distance to food market	2.59	2.43	0.53	1.91	2.14	0.41	0.68	0.12
Recipient highest grade attained	3.96	3.77	0.52	4.23	4.33	0.79	-0.26	0.60
Recipient widowed	0.07	0.06	0.80	0.12	0.06	0.04	-0.05	0.22
Recipient widowed/divorced	0.13	0.15	0.58	0.21	0.12	0.03	-0.08	0.14

Panel B. Differences in Overall Attrition by Treatment Status at 36 Month Follow-up

	Control			Treatment			Difference	
	Attrited	Panel	P-value	Attrited	Panel	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Maize grain price	22.01	22.00	0.99	25.46	25.46	1.00	-3.45	0.31
Rice price	3.85	3.88	0.90	3.19	3.32	0.62	0.66	0.17
Bean price	3.48	3.91	0.26	3.61	4.09	0.14	-0.13	0.83
Cooking oil price	10.29	10.83	0.05	10.86	11.02	0.48	-0.58	0.20
Sugar price	7.74	7.56	0.56	7.77	7.86	0.82	-0.03	0.96
Table salt price	3.27	3.93	0.22	3.89	3.94	0.91	-0.62	0.46
Toilet soap price	5.17	5.23	0.81	5.02	5.12	0.38	0.15	0.66
Log household size	1.70	1.66	0.67	1.62	1.68	0.48	0.09	0.50
Shangombo	0.19	0.34	0.15	0.20	0.34	0.13	0.00	0.98
Kaputa	0.33	0.33	1.00	0.54	0.33	0.03	-0.20	0.27
Log distance to food market	1.86	2.46	0.09	2.20	2.12	0.75	-0.34	0.51
Recipient highest grade attained	5.05	3.77	0.05	4.27	4.32	0.93	0.78	0.38
Recipient widowed	0.10	0.06	0.59	0.12	0.06	0.22	-0.03	0.74

Panel A. Differences in Overall Attrition by Treatment Status at 24 Month Follow-up

	Control			Treatment			Difference	
	Attrited	Panel	P-value	Attrited	Panel	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recipient widowed/ divorced	0.24	0.15	0.23	0.22	0.12	0.10	0.02	0.85

Panel C. Differences in Overall Attrition by Treatment Status at 48 Month Follow-up

	Control			Treatment			Difference	
	Attrited	Panel	P-value	Attrited	Panel	P-value	Col(1)-Col(4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Maize grain price	22.02	22.00	0.99	24.16	25.53	0.38	-2.14	0.48
Rice price	3.95	3.88	0.74	3.24	3.32	0.62	0.71	0.06
Bean price	3.98	3.90	0.87	4.04	4.07	0.89	-0.07	0.92
Cooking oil price	10.28	10.84	0.08	11.01	11.01	0.96	-0.72	0.09
Sugar price	7.36	7.57	0.48	7.61	7.87	0.40	-0.25	0.69
Table salt price	4.04	3.91	0.62	3.96	3.94	0.96	0.09	0.90
Toilet soap price	5.39	5.23	0.39	5.13	5.12	0.88	0.25	0.41
Log household size	1.80	1.66	0.00	1.61	1.68	0.34	0.19	0.02
Shangombo	0.24	0.34	0.29	0.29	0.34	0.39	-0.04	0.75
Kaputa	0.33	0.33	1.00	0.44	0.33	0.07	-0.11	0.48
Log distance to food market	1.93	2.46	0.11	2.02	2.12	0.59	-0.09	0.83
Recipient highest grade attained	4.36	3.78	0.39	4.59	4.31	0.62	-0.23	0.81
Recipient widowed	0.06	0.06	0.94	0.13	0.06	0.05	-0.07	0.24
Recipient widowed/ divorced	0.15	0.15	0.95	0.16	0.12	0.43	-0.01	0.92

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.

Appendix 4.

Summary Statistics Total Number of Children Living in Household, by Wave and Treatment Status

Panel A: All children

	Baseline		24 months		36 months		48 months	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
0-1 years	0.76	0.79	0.43	0.40	0.45	0.46	0.34	0.36
2-4 years	0.93	0.89	1.13	1.15	0.98	1.02	0.85	0.80
Total 0-4 years	1.92	1.88	1.84	1.82	1.80	1.84	1.55	1.55

Panel B: Female Children

Panel A: All children								
	Baseline		24 months		36 months		48 months	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline		24 months		36 months		48 months	
	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
0–1 years	0.37	0.42	0.19	0.19	0.20	0.23	0.16	0.18
2–4 years	0.47	0.44	0.55	0.58	0.47	0.53	0.40	0.42
Total 0–4 years	0.94	0.97	0.89	0.92	0.85	0.93	0.74	0.80

Panel B: Male Children								
	Baseline		24 months		36 months		48 months	
	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
0–1 years	0.37	0.36	0.23	0.21	0.25	0.23	0.18	0.18
2–4 years	0.45	0.43	0.56	0.55	0.49	0.48	0.45	0.39
Total 0–4 years	0.95	0.89	0.92	0.88	0.93	0.89	0.81	0.76

Appendix 5:

Impact of CGP on Household-Level Child Counts, Ages 0–1 and 2–4 years, By Gender, Poisson Models

Panel A: Total Number of Children Aged 0–1 year									
	Males & Females, Combined			Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	24 months	36 months	48 months	24 months	36 months	48 months	24 months	36 months	48 months
Treat	0.03	0.04	0.04	0.12**	0.13**	0.15***	–0.04	–0.05	–0.06
	(0.029)	(0.029)	(0.029)	(0.051)	(0.051)	(0.051)	(0.060)	(0.061)	(0.060)
Time=24 months	–0.57***			–0.63***			–0.48***		
	(0.040)			(0.078)			(0.065)		
Treat*Time(24 months)	–0.10			–0.13			–0.07		
	(0.067)			(0.113)			(0.100)		
Time=36 months		–0.52***			–0.62***			–0.42***	
		(0.032)			(0.077)			(0.062)	
Treat*Time(36 months)		–0.01			0.01			–0.03	
		(0.045)			(0.101)			(0.081)	
Time=48 months			–0.80***			–0.84***			–0.73***
			(0.040)			(0.081)			(0.073)

Panel A: Total Number of Children Aged 0–1 year									
	Males & Females, Combined			Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	24 months	36 months	48 months	24 months	36 months	48 months	24 months	36 months	48 months
Treat *Time(48 months)			0.02 (0.060)			-0.00 (0.104)			0.04 (0.101)
Observations	4,815	4,976	4,942	4,815	4,976	4,942	4,815	4,976	4,942

Panel B: Total Number of Children Aged 2–4 years									
	Males & Females, Combined			Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	24 months	36 months	48 months	24 months	36 months	48 months	24 months	36 months	48 months
Treat	-0.04 (0.036)	-0.05 (0.035)	-0.04 (0.036)	-0.03 (0.057)	-0.04 (0.056)	-0.06 (0.057)	-0.05 (0.048)	-0.05 (0.048)	-0.02 (0.050)
Time=24 months	0.19 *** (0.035)			0.15 *** (0.046)			0.22 *** (0.043)		
Treat *Time(24 months)	0.07 (0.049)			0.13 * (0.072)			0.02 (0.059)		
Time=36 months		0.05 (0.038)			0.00 (0.049)			0.10 * (0.055)	
Treat *Time(36 months)		0.09 * (0.053)			0.18 ** (0.082)			0.00 (0.073)	
Time=48 months			-0.09 ** (0.042)			-0.18 *** (0.050)			0.02 (0.055)
Treat *Time(48 months)			-0.01 (0.062)			0.12 (0.087)			-0.13 (0.089)
Observations	4,815	4,976	4,942	4,815	4,976	4,942	4,815	4,976	4,942

Notes: Robust standard errors in parentheses.

* $p < 0.1$

** $p < 0.05$;

*** $p < 0.01$.

Difference-in-difference estimates run using Poisson models.

All models control for baseline values of log(household size), log(distance to food market), recipient highest grade attained, recipient marital status, basket of prices.

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Table 1.

Baseline individual- and community-level characteristics by CGP treatment

	(1)	(2)	(3)	(4)
	All	Control	Treatment	Difference
Age in years	28.2 (0.17)	28.28 (0.26)	28.11 (0.21)	-0.17 (0.33)
Highest grade attained (baseline)	3.62 (0.15)	3.4 (0.19)	3.83 (0.18)	0.42*** (0.26)
Divorced/separated/widowed (baseline)	0.13 (0.01)	0.15 (0.01)	0.12 (0.01)	-0.03** (0.02)
Never married (baseline)	0.27 (0.02)	0.27 (0.02)	0.28 (0.02)	0.02 (0.03)
Married/co-habiting (baseline)	0.59 (0.02)	0.59 (0.03)	0.6 (0.03)	0.01 (0.04)
Log (household size) at baseline	1.9 (0.01)	1.89 (0.02)	1.91 (0.02)	0.02 (0.03)
Shangombo District	0.28 (0.04)	0.29 (0.06)	0.28 (0.06)	-0.01 (0.09)
Kaputa District	0.28 (0.04)	0.28 (0.06)	0.28 (0.06)	0.00 (0.09)
log (distance to food market) at baseline	2.23 (0.13)	2.39 (0.18)	2.08 (0.18)	-0.31 (0.25)
<i>N</i>	2,675	1,326	1,349	

Standard errors in parenthesis.

* $p < 0.1$

**

*** $p < 0.05$;*** $p < 0.01$

Table 2.

Means, women's fertility outcomes by CGP treatment status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 24-month	All Women							
	All	Control	Treatment	Difference	All	Control	Treatment	Difference
Currently pregnant	0.11 (0.01)	0.11 (0.01)	0.12 (0.01)	0.01 (0.02)	0.14 (0.01)	0.13 (0.01)	0.14 (0.01)	0.02 (0.01)
Ever pregnant	0.83 (0.01)	0.85 (0.01)	0.81 (0.01)	-0.04** (0.02)	0.99 (0.00)	0.99 (0.00)	0.98 (0.01)	-0.01*** (0.01)
Ever miscarried, aborted, had stillbirth	0.12 (0.01)	0.14 (0.01)	0.1 (0.01)	-0.04*** (0.02)	0.15 (0.01)	0.16 (0.02)	0.13 (0.01)	-0.04*** (0.02)
Total number of children ever born alive	3.24 (0.06)	3.32 (0.09)	3.15 (0.07)	-0.16 (0.11)	3.94 (0.07)	3.98 (0.09)	3.91 (0.10)	-0.07 (0.09)
Currently using modern contraceptive	-	-	-	-	-	-	-	-
<i>N</i>	2,669	1,324	1,345		2,096	1,054	1,042	
Panel B: 36-month	All Women							
	All	Control	Treatment	Difference	All	Control	Treatment	Difference
Currently pregnant	0.07 (0.00)	0.08 (0.01)	0.07 (0.01)	0.00 (0.01)	0.09 (0.01)	0.09 (0.01)	0.10 (0.01)	0.00 (0.01)
Ever pregnant	0.8 (0.01)	0.82 (0.01)	0.78 (0.01)	-0.03** (0.01)	1.00 (0.00)	1.00 (0.00)	1.00 (0.00)	0.00 (0.00)
Ever miscarried, aborted, had stillbirth	0.06 (0.01)	0.06 (0.01)	0.05 (0.01)	0.00 (0.01)	0.07 (0.01)	0.07 (0.01)	0.07 (0.01)	0 (0.01)
Total number of children ever born alive	3.19 (0.06)	3.24 (0.07)	3.13 (0.09)	-0.1 (0.09)	4.15 (0.07)	4.14 (0.09)	4.16 (0.10)	0.03 (0.09)
Currently using modern contraceptive	-	-	-	-	0.38 (0.02)	0.39 (0.02)	0.37 (0.03)	-0.02 (0.02)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 24-month								
All Women								
	All	Control	Treatment	Difference	All	Control	Treatment	Difference
<i>N</i>	2,993	1,491	1,502		2,171	1,103	1,068	
Panel C: 48-month								
All Women								
	All	Control	Treatment	Difference	All	Control	Treatment	Difference
Currently pregnant	0.06 (0.00)	0.06 (0.01)	0.06 (0.01)	0.00 (0.01)	0.07 (0.01)	0.07 (0.01)	0.07 (0.01)	0 (0.01)
Ever pregnant	0.78 (0.01)	0.79 (0.01)	0.77 (0.01)	-0.02 (0.01)	0.98 (0.00)	0.98 (0.00)	0.98 (0.00)	0 (0.01)
Ever miscarried, aborted, had stillbirth	0.08 (0.01)	0.09 (0.01)	0.07 (0.01)	-0.02** (0.01)	0.1 (0.01)	0.11 (0.01)	0.08 (0.01)	-0.02** (0.01)
Total number children ever born alive	3.29 (0.06)	3.31 (0.08)	3.28 (0.09)	-0.03 (0.10)	4.32 (0.07)	4.28 (0.10)	4.37 (0.11)	0.09 (0.09)
Currently using modern contraceptive	-	-	-	-	0.53 (0.02)	0.51 (0.02)	0.54 (0.02)	0.03 (0.02)
<i>N</i>	3,025	1,518	1,507		2,178	1,116	1,062	

Notes: Standard errors in parenthesis.

* $p < 0.1$

** $p < 0.05$

*** $p < 0.01$.

Contraceptive use information only available for main respondents, and not available for any women at 24-month follow-up.

Table 3:

Impact of CGP on women's total fertility and related outcomes

	All Women		Main respondents Only					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 24 months								
	Total fertility	Currently pregnant	Ever been pregnant	Ever had miscarriage stillbirth or abortion	Total fertility	Currently pregnant	Ever been pregnant	Ever had miscarriage stillbirth or abortion
Treatment	-0.02 (0.84)	0.019 (1.38)	-0.025 (1.89)*	-0.027 (1.82)*	0.008 (0.37)	0.029 (1.74)*	-0.007 (0.97)	-0.024 (1.31)
R ²	-	0.03	0.53	0.05	-	0.03	0.01	0.03
N	2,675	2,670	2,675	2,668	2,107	2,102	2,107	2,100
Panel B: 36 months								
	Total fertility	Currently pregnant	Ever been pregnant	Ever had miscarriage stillbirth or abortion	Total fertility	Currently pregnant	Ever been pregnant	Ever had miscarriage stillbirth or abortion
Treatment	-0.013 (0.60)	0.004 (0.37)	-0.016 (1.23)	0.007 (0.59)	0.012 (0.66)	0.008 (0.59)	-0.001 (0.21)	0.011 (0.71)
R ²	-	0.01	0.08	0.01	-	0.01	0.01	0.02
N	2,994	2,997	3,020	3,016	2,221	2,202	2,221	2,218
Panel C: 48 months								
	Total fertility	Currently pregnant	Ever been pregnant	Ever had miscarriage stillbirth or abortion	Total fertility	Currently pregnant	Ever been pregnant	Ever had miscarriage stillbirth or abortion
Treatment	0.005 (0.28)	0.001 (0.09)	-0.002 (0.16)	-0.021 (2.54)**	0.011 (0.66)	0.004 (0.29)	0 (0.02)	-0.028 (2.66)***
R ²	-	0.01	0.08	0.01	-	0.02	0.01	0.02
N	2,986	3,025	3,030	3,028	2,206	2,205	2,206	2,205
								Modern contraceptive use
								0.014 (0.46)
								0.03 2,173

Notes: Robust t-statistics in parentheses.

* $p < 0.1$

** $p < 0.05$

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 $p < 0.01$

Total fertility models run with Poisson models; all other outcomes use linear probability models.

All models control for baseline values of log (household size), log (distance to food market), highest grade attained, marital status, basket of prices, and current age.

Table 4: Impact of CGP on women's total fertility and related outcomes, Women Aged <25 years

Panel A: 24 months				
	(1)	(2)	(3)	(4)
	Total fertility	Currently pregnant	Ever been pregnant	Ever had miscarriage stillbirth or abortion
Treatment	-0.09 (1.31)	0.024 (1.17)	-0.02 (0.63)	-0.021 (1.38)
R ²	-	0.03	0.34	0.02
N	1,003	1,005	1,006	1,004
Panel B: 36 months				
Treatment	-0.107 (1.86)*	0.009 (0.68)	0.006 (0.22)	0.011 (1.06)
R ²	-	0.03	0.26	0.02
N	1,083	1,092	1,106	1,104
Panel C: 48 months				
Treatment	-0.049 (0.59)	-0.013 (0.96)	0.011 (0.41)	0.012 (1.19)
R ²	-	0.02	0.26	0.03
N	1,002	1,026	1,029	1,028

Notes: Robust t-statistics in parentheses.

* $p < 0.1$

** $p < 0.05$;

*** $p < 0.01$.

Total fertility models run with Poisson models; all other outcomes use linear probability models.

All models control for baseline values of log (household size), log (distance to food market), highest grade attained, marital status, basket of prices, and current age.

Table 5:

Impact of CGP on Household-Level Child Counts, Ages 0–4 years, Poisson Models

	Combined			Females			Males		
	(1) 24 months	(2) 36 months	(3) 48 months	(4) 24 months	(5) 36 months	(6) 48 months	(7) 24 months	(8) 36 months	(9) 48 months
Treat	-0.02 (0.018)	-0.02 (0.019)	-0.02 (0.020)	0.05 (0.032)	0.04 (0.032)	0.04 (0.032)	-0.08** (0.036)	-0.08** (0.034)	-0.07** (0.036)
Time=24 months	-0.05*** (0.017)			-0.06*** (0.023)			-0.03 (0.023)		
Treat *Time(24 months)	0.01 (0.023)			0.01 (0.030)			0.02 (0.032)		
Time=36 months		-0.06*** (0.018)			-0.10*** (0.025)			-0.02 (0.024)	
Treat *Time(36 months)		0.04 (0.026)			0.06* (0.036)			0.02 (0.034)	
Time=48 months			-0.21*** (0.021)			-0.24*** (0.029)			-0.15*** (0.029)
Treat *Time(48 months)			0.02 (0.034)			0.05 (0.046)			-0.01 (0.047)
Observations	4,815	4,976	4,942	4,815	4,976	4,942	4,815	4,976	4,942

Notes: Robust standard errors in parentheses.

* $p < 0.1$

** $p < 0.05$;

*** $p < 0.01$.

Difference-in-difference estimates run using Poisson models.

All models control for baseline values of log(household size), log(distance to food market), recipient highest grade attained, recipient marital status, basket of prices.