HHS Public Access

Author manuscript

*Health Econ*. Author manuscript; available in PMC 2017 June 28.

Published in final edited form as:

*Health Econ*. 2016 February ; 25(2): 225–236. doi:10.1002/hec.3136.

# Income transfers and maternal health: Evidence from a national randomized social cash transfer program in Zambia

**Sudhanshu Handa**,

Professor in the Department of Public Policy at the University of North Carolina at Chapel Hill and Chief of Child Poverty and Social and Economic Policy Responses at the UNICEF Office of Research, Florence, Italy

**Amber Peterman**,

Assistant Research Professor in the Department of Public Policy at the University of North Carolina at Chapel Hill

**David Seidenfeld**, and

Senior Researcher at American Institutes for Research, Washington, DC

**Gelson Tembo**

Senior Lecturer in the Department of Agricultural Economics at the University of Zambia, Lusaka

## Abstract

There is promising recent evidence that poverty-targeted social cash transfers have potential to improve maternal health outcomes, however questions remain surrounding design features responsible for impacts. In addition, virtually no evidence exists from the African region. This study explores the impact of Zambia's Child Grant Program on a range of maternal health utilization outcomes using a randomized design and difference-in-differences multivariate regression from data collected over 24 months from 2010 to 2012. Results indicate that while there are no measurable program impacts among the main sample, there are heterogeneous impacts on skilled attendance at birth among a sample of women residing in households having better access to maternal health services. The latter result is particularly interesting because of the overall low level of healthcare availability in program areas suggesting dedicated program design or matching supply-side interventions may be necessary to leverage unconditional cash transfers in similar settings to impact maternal health.

### Keywords

Cash transfers; maternal health; Zambia

Corresponding author: Amber Peterman, Department: Public Policy, University of North Carolina at Chapel Hill, Campus Box #3435, Chapel Hill, NC, 27599 USA; Telephone: 360-472-1349; Fax: 919-962-5824; amberpeterman@gmail.com; apeterma@email.unc.edu.

**Conflict of Interest**: There are no conflicts of interest to disclose.

**Ethics Review:** The study has undergone ethnics review at the American Institutes for Research and through the University of Zambia, Lusaka.

## 1. INTRODUCTION

Nearly two decades have passed since the explosion of cash transfer programs made famous by the evaluation of the Government of Mexico's *Oportunidades* (formerly *Progresa*) program, showing large impacts on a range of human capital and development outcomes. These programs, primarily in Latin America and Caribbean (LAC) countries, were typically targeted toward households with under five and school age children, and payments made conditional on completing certain co-responsibilities, such as child health check-ups or maintaining levels of school attendance. Now a mainstay of many government welfare portfolios, a 'new generation' of social cash transfer (SCT) schemes have emerged and are characterized by several distinct features. First, compared with LAC countries, the expansion has been largely into regions with weaker institutional structures, more widespread poverty and where government and institutional-run social safety nets have been historically absent and unreliable. For example, in the last five years, impact evaluations of approximately 14 large-scale government programs have been commissioned in a range of African countries including South Africa, Kenya, Ghana, Malawi, Zimbabwe and Lesotho (Davis et al. 2012). Second, unlike LAC programs, the majority of SCTs in Africa are unconditional and often linked to geographic or vulnerability targeting (e.g. orphans and other vulnerable children, the disabled or the elderly). Third, both objective and impact evaluations have expanded to include diverse targets such as adolescent risk behaviors, reproductive and maternal health, productive activities and local economy 'spillover' effects. There is great interest by policymakers and practitioners on both the efficacy and efficiency of SCTs in targeting a broad range of development outcomes in these settings. The idea of just "giving money to the poor" so beneficiaries can make use of funds according to their own needs, has also gained political popularity (Hanlon 2004).

One area garnering the attention of researchers and policymakers is the potential of poverty-targeted SCTs and related programs to affect maternal and neonatal health. Despite global improvements in maternal mortality, 20 developing countries show either increases or no progress in maternal mortality and only 22 of 137 developing countries are likely to achieve Millennium Goal Target 5 by 2015 (Lozano et al. 2011). Annual estimates report approximately 60 million women give birth outside health facilities and 52 million give birth without a skilled attendant (UNICEF 2008). There are multiple layers of demand and supply-side barriers to receiving adequate care, however program implementers and policymakers are increasingly seeking alternate and innovative methods of overcoming barriers. Although the evidence is promising from LAC evaluations that SCTs have the ability to improve maternal health, particularly healthcare utilization, there is a lack of consensus on which program design components may be responsible for impacts (Glassman et al. 2013). However, for maternal health indicators, there is no evidence of improvement particularly from the African continent using unconditional programming (Lagarde et al. 2007). Part of the challenge in providing evidence is due to the fact that the majority of poverty targeted SCTs are not designed to impact maternal health specifically. Therefore, these indicators may not be collected, or when they are, sample sizes may be small, resulting in underpowered estimates or the inability to conduct rigorous analysis.

This study uses evaluation data from the Zambian Child Grant Program (CGP), a large-scale government-run unconditional SCT given to women to evaluate the impact of income transfers on four maternal healthcare utilization outcomes: 1) antenatal care (from a doctor or nurse); 2) at least four antenatal visits; 3) quality of antenatal care (defined as receiving Voluntary Counseling and Testing (VCT) for HIV, tetanus vaccination and malaria treatment during antenatal care); and 4) skilled attendance at birth (from a doctor or nurse). Although the CGP was not designed specifically to impact maternal health outcomes, there are several pathways through which a structural intervention could improve care. The analysis utilizes the randomized control trial (RCT) design and difference-in-difference (DID) multivariate regression analyses to identify causal impacts of income transfers over a 24-month period from 2010 to 2012. Results indicate no measurable program impacts for the full sample on any of the four outcome indicators. However, we found heterogeneous impacts on skilled attendance at birth among a sample of women residing in households with better access to maternal health services. Although there are a number of explanations for lack of robust significant impact, taken together, results indicate tailoring complementary services or matching supply-side interventions may be necessary to enhance the impact of unconditional poverty-targeted SCTs for maternal health. Policy and research implications are discussed in the context of the growing number of SCT programming and impact evaluations both globally and in Africa specifically.

## 2. BACKGROUND

### 2.1. Transfers and maternal health

There is growing interest in the ability of cash and other transfers to improve maternal health, as indicated by the increase in recent research and ongoing evaluations focused on these linkages around the world. In a recent systematic review of the impacts of conditional cash transfer (CCT) programs on maternal and newborn health, Glassman and colleagues (2013) categorize evidence into “broad” and “narrow” programming. Broad programs include the typical large-scale poverty-targeted LAC CCTs with general poverty objectives, while narrow programming is designed to impact maternal health specifically, typically through voucher schemes or payments linked to behavior around the time of birth.

While it is not immediately obvious that “broad” CCTs would deliver impacts on maternal health, they include a set of design features allowing for the potential improvements in outcomes. The first and most obvious pathway is through an income effect. Households receiving transfers will on average have more disposable income, which can be used to purchase healthcare, including buying medicine, paying health facility fees or purchasing transportation to distant health facilities. If transfers are given to women specifically, the program increases bargaining power or otherwise empowers women and a larger share of the disposable income may be spent on women-specific goods. If transfers result in greater food security, dietary diversity and better health or nutrition of the households in general, maternal-specific health may also improve. Programs with conditions or monthly trainings for participants may include antenatal or postnatal visits as part of the household co-responsibilities, or stimulate demand for healthcare through health or nutrition trainings (de Brauw and Peterman 2011). In addition, many programs include supply-side incentives such

as the removal of user fees for health services or the increase in the supply of health services through investment in infrastructure.

Despite these promising features, traditional CCTs have not always been successful in inducing a measurable impact on maternal health or service utilization. For example, evaluations of *Oportunidades* using a RCT design and study sample size of 840 to 892 births, found increases in quality of antenatal visits, however no impact on use of antenatal care, number of visits or type of facility visited (Barber 2009; Barber and Gertler 2008). Further, quasi-experimental evidence from El Salvador's *Communidades Solidarias Rurales* similarly showed no impact on antenatal care service utilization using samples of approximately 494 births (de Brauw and Peterman 2011). However, evidence from Uruguay and Guatemala are slightly more promising. Using program administrative micro-data matched to longitudinal vital statistics, Amarante and colleagues (2012) found that participation in the unconditional cash transfer *Plan de Atencion Nacional a la Emergencia Social (PANES*) resulted in significantly more total antenatal visits (albeit a small effect size, equal to approximately two percent of the baseline average), however no impact on the week of the first visit (N = 67,863). Finally, a quasi-experimental evaluation of Guatemala's *Mi Familia Progresa* found that among 1,163 women, there were significant increases in the total number of antenatal visits (only among the non-indigenous sample, five to eight percentage points), as well as antenatal attendance at a health facility (11 percentage point increase) for their last birth (Gutierrez et al. 2011).

Evidence on outcomes more proximate to birth are also mixed. One analysis of the *Oportunidades* RCT found little or no impact on skilled attendance at birth among rural women (Urquieta-Salomon et al. 2009), however results are in contrast to those using more recent data, where younger cohorts of Mexican women (aged 15 to 19 and 20 to 24 years) had 88 percent and 41 percent greater likelihood of choosing a physician or nurse as compared to traditional midwife for childbirth (Sosa-Rubi et al. 2011). Results from El Salvador show significant impacts on rates of births in hospitals and birth attendance by doctors and nurses (de Brauw and Peterman 2011), however there are no significant impacts on these outcomes in Uruguay (Amarante et al. 2012). In Guatemala, impacts on skilled attendance at birth are mixed, depending on what subgroup of women were being examined (Gutierrez et al. 2011).

The second strand of literature, referred to as the "narrow" programs, examines dedicated maternal health voucher schemes and other maternal-health specific supply or demand side incentives, primarily in Asia. In India, Lim and colleagues (2010) evaluate the *Janani Suraksha Yojana* program, a one-time cash incentive to induce women to give birth in facilities using a quasi-experimental design. Utilizing nationwide district-level household survey data, results indicate the program had a positive impact on completion of three antenatal care visits, in-facility births or out-of-facility births with a skilled attendant, and in some models, a reduction of neonatal and perinatal deaths, but no effect on maternal mortality. However, the authors note targeting challenges and quality of healthcare as restricting factors in both program implementation and evaluation.[1] Small but significant positive impacts were also found in a quasi-experimental evaluation from the Safe Delivery Incentive Program in Nepal, a nation-wide program designed specifically to increase skilled

attendance at birth through a variety of incentives to women (cash transfer to compensate for travel to clinics at the time of birth and free delivery care) and to providers (cash transfer for every delivery attended). However, similarly, authors note key constraints in program delivery, including institutional capacity and weak health services (Powell-Jackson et al. 2009a; Powell-Jackson et al. 2009b). Finally, increases in use of antenatal care (22 percentage point increase), postnatal care (35 percentage point increase) and institutional delivery (22 percentage point increase) were found to be associated with purchase of a voucher booklet ($50 equivalent) in an evaluation of the *Dera Ghazi Khan* maternal health voucher scheme in Pakistan (Agha, 2011). However, the study was not designed as experimental and thus there are some limits both to methodology and to geographical representation necessitating further validation.

In summary, although promising evidence exists for CCTs (and SCTs in general) in affecting maternal health, particularly around healthcare utilization at the time of birth, questions remain surrounding design features responsible for impacts (Glassman et al. 2013). In addition, although dedicated voucher schemes have demonstrated positive impacts, evaluation and institutional limitations have been cited across studies and no cost-effectiveness assessments exist. Many of the current evaluations suffer from small sample sizes and the inability to control for important factors such as service facility data, thus are not able to clearly articulate which casual pathway is leading to, or lacking in determining impacts. None of the seven studies reviewed by Glassman and colleagues (2013) with antenatal indicators, nor the six studies reviewed with skilled attendance at birth come from Africa. Therefore, there is interest not only in how program dynamics play out in a very different context such as Zambia, but also how large-scale unconditional programs may perform in affecting similar indicators to those studied in more traditional CCT programs.

### 2.2 The Zambian Child Grant Program

The CGP is a large-scale social safety net owned by the Zambian Government and operated by the Zambian Ministry of Community Development, Mother and Child Health (MCDMCH). The CGP was initiated in 2010 with the overall goal of reducing extreme poverty and curbing the intergenerational transfer of poverty. The CGP currently operates in Kalabo, Shangombo and Kaputa districts which represent the districts with the highest rates of mortality, morbidity, stunting and wasting among children aged zero to five years. In eligible areas, the program transfers a fixed sum of 60 Zambian kwacha (ZMW) rebased (approximately US $12) on a bi-monthly basis to the primary female adult in households with a child under the age of three at program initiation. Recipients are universally targeted within the geographical areas selected for the program. There is a continuous enrollment system, in which households were immediately enrolled after having a newborn baby. The transfer amount is expected to be sufficient to purchase food equivalent to one meal per day for all household members and is distributed through a local pay-point manager. The specific objectives of the program are to: 1) supplement, not replace, household income; 2) increase the number of children enrolled in and attending primary school; 3) reduce the rate of

[1]In addition, other scholars have called for need of further research to confirm these findings, both generally, as well as to specifically examine contextual factors contributing to program impact (Das and Roa 2011; Khan et al. 2010).

mortality and morbidity among children under 5 years old; 4) reduce stunting and wasting among children under 5 years old; 5) increase the number of households owning assets such as livestock; and 6) increase the number of households that have a second meal a day. Results of the 24 month follow-up evaluation report show that the CGP was largely successful at meeting poverty-related targets, with significant impacts on reducing extreme poverty and increasing food security, consumption, dietary diversity, asset ownership and productive activities, material well-being of older age-group children, as well as stimulating the local economy (AIR, 2013). As previously noted, the CGP was not designed to specifically impact maternal health outcomes, although there are several pathways through which impacts could occur, particularly through an income effect and through empowerment of women who receive cash.

## 3. DATA AND METHODOLOGY

### 3.1 Data

The CGP evaluation was commissioned by the Government of Zambia and is part of the Transfer Project, a consortium of international research partners and civil society partnering with national governments to support improved knowledge and practice on SCT in Africa.[2] A baseline survey among a random sample of 2,515 beneficiary and non-beneficiary households was carried out in October to November of 2010 and a longitudinal follow-up survey was conducted 24 months later in October to November of 2012. The baseline sample includes 14,565 individuals, of whom 4,793 are under the age of five, split evenly among 90 randomly assigned treatment and comparison clusters (45 in each arm). The sample size was powered to detect effects for child anthropometry using the smallest analysis subgroup (children under age five) and accounting for attrition and nonresponse rates. The survey contained detailed modules on socioeconomic status, wealth and production activities, early childhood development, women's empowerment and collected anthropometry for all children under five. A module was administered collecting birth-specific information on and matching to mothers or primary caregivers for all children born up to five years prior to the baseline or during the program period. In addition, community-level and health-facility level surveys were collected to account for access to services and infrastructure. Questionnaires, reports and further information are publicly available on the Transfer Project website. The study underwent ethnics review at AIR in Washington, D.C. and through the University of Zambia in Lusaka.

### 3.2 Methodology

The CGP evaluation is designed as an RCT, taking advantage of the government's planned rollout of the program, with the unit of randomization at the Community Welfare Assistance Committee (CWAC), the lowest level of program administration. CWACs were randomly assigned to either treatment (starting the program in December 2010) or delayed-entry (starting the program at the end of 2013). The latter group serves as the comparison for the purpose of evaluation. The randomization was conducted by the Ministry in collaboration with the research team after the baseline survey had been administered, both to ensure

[2]The Transfer Project Website: http://www.cpc.unc.edu/projects/transfer [7 November 2013].

transparency and unbiased implementation of data collection. The randomized, longitudinal design of the evaluation allows estimation of the program impact as the difference in the change in health utilization outcomes from baseline to follow-up of treatment women in comparison to the change in health utilization outcomes of similar control women. The specific notation for the DID estimator is the following:

$$DID = [E[I_1] - E[I_0]] - [E[C_1] - E[C_0]], \quad (1)$$

where $E[I_1] - E[I_0]$ is the change in health care utilization outcome of interest, for example skilled attendance at birth, for the treatment group from baseline to follow-up, and $E[C_1] - E[C_0]$ is the change in skilled attendance at birth for the control group from baseline to follow-up. The DID estimator provides an unbiased estimate of the treatment effect under the assumption that in the absence of the treatment, outcomes in the two groups would have followed similar trends. Further, an analysis of community prices and shocks found no significant differences across treatment and control CWACs during the study period (AIR, 2013). Since the program is designed as an unconditional transfer, the DID can be taken as measuring a pure income effect of the program, rather than capturing additional supply-side or learning effects from health information trainings.

In a multivariate regression framework, the DID estimator can be calculated with the following equation:

$$Y = \beta_0 + \beta_1 T + \beta_2 CGP + \beta_3 (T * CGP) + \beta_x X + \varepsilon, \quad (2)$$

where $Y$ is the outcome of interest at baseline or follow-up, $T$ is the time trend equal to 1 if the observation is in the follow-up, and $CGP$ is an indicator for whether the household is in the treatment group, and $\beta_3$ is coefficient for the interaction term between the two, or the DID intent to treat estimator. In other words, $\beta_3$ represents the amount of change in outcome, Y, which is attributable to the intervention. In equation 2, $X$ is a vector of baseline covariates and $\beta_x$ is a vector of coefficients that correspond to each covariate. Theoretically, with random assignment, the inclusion of baseline controls is not necessary to obtain unbiased estimates of $\beta_3$ [3] However, to increase the precision of the estimates and control for any minor differences between treatment and comparison arms at baseline, models including covariates will be presented in the Appendix. Control variables included in equation 2 are at the individual level (age and age squared in years of mother/primary caregiver, highest grade of education of mother/primary caregiver, indicators for mother/primary caregivers marital status), household level (household size, distance to nearest food market) and the health facility level (distance to nearest health facility, index of service availability of maternal health care) and a vector of community level prices of staple foods in ZMW (maize grain, rice, beans, dried fish, sugar, salt, soap and panadol, a common form of pain medication). The index of service availability is constructed through factor analysis

[3] In particular the success of randomization of the CGP evaluation sample has been demonstrated through balancing of general household socio-economic indicators (Seidenfeld and Handa, 2011; AIR, 2013).

containing indicators that the cluster health facility offers: 1) obstetric care, 2) antenatal care, 3) tetanus vaccination, 4) VCT, 5) malaria tests, 6) pregnancy tests, 7) family planning and 8) mobile clinics. The scale reliability between these indicators is high (Cronbach's alpha = 0.91). Descriptive statistics for all control variables including those contributing to the health service availability index are included stratified by treatment and control in Appendix Table A1. Equation 2 is estimated using linear probability models (LMPs)[4] and in all regressions, district-level dummy variables are included and standard errors are clustered at the level of randomization (CWAC-level).[5]

### 3.3 Sample and key indicators

The information for the maternal health care utilization indicators comes from a module collecting data on every child living in the household aged zero to 60 months and links to the biological mother or primary caregiver from the household roster. The sample for analysis includes information on pregnancies for children born at least 15 months prior to the baseline or follow-up survey. This cut point ensures that pregnancies are only included in the sample if by the follow-up women were participating in the program for the entire nine months of the pregnancy.[6] The resulting sample is 1,155 births in the baseline (580 control and 575 treatment) and 559 births in the follow-up (288 control and 271 treatment). The reduced sample in the follow-up is reflective of the initial program enrollment requirement of a child under the age of five for households, which by the follow-up would have aged out of the birth cohort examined. We examine four outcomes: 1) antenatal care from qualified health practitioner (doctor or nurse), 2) at least four antenatal visits during pregnancy, 3) quality of antenatal care (defined as receiving VCT for HIV, tetanus vaccination and malaria treatment during antenatal care), and 4) skilled attendance at birth (doctor or nurse).[7] Outcome 2 follows the recommended number of visits as outlined by the WHO and all measures are self-reported. Births are included in the sample regardless of whether the woman appears in both the baseline and follow-up surveys.[8]

### 3.4 Attrition analysis

In virtually all analysis of longitudinal data, we may be concerned about attrition over the panel period and how this may bias results. Attrition analysis in the 24-month impact report for the CGP shows that attrition is approximately nine percent, however finds no differential attrition between treatment and control with respect to basic demographic and socio-

[4]We also estimate probit models with the same specifications, however find qualitatively identical results, and therefore prefer LMP specifications for simplicity of interpretation.

[5]Contamination between treatment and control clusters is one threat to internal validity. This could occur if a control cluster household mistakenly ended up on a beneficiary list for a neighboring treatment cluster, or if a treatment cluster household subsequently moved into a control cluster and was misclassified as a non-beneficiary. Tracking protocol specified that if any household moved to another study cluster, they would be tracked and retained in the evaluation sample as their original cluster treatment status. To examine potential contamination, we identify the number of households in control areas who have ever received the transfer at follow-up and find that 35 households out of 1,145 (~3%) control cluster households report having ever received the transfer, and 32 households report current receipt of the transfer. This indicates that there is likely little contamination between treatment and control clusters.

[6]Results are robust to cut points at 14 months and 16 months.

[7]We do not analyze any antenatal care utilization (whether or not from a skilled practitioner), as virtually the entire sample reports at least one visit. We conduct robustness checks by analyzing each quality of care indicator separately for the main analysis, and similar to the combined indicator, find no significant impacts by individual indicator.

[8]We also run parallel analysis at the household level, using household-level averages for outcomes and characteristics of the CGP transfer recipient for socio-demographic indicators specific to the mother/caregiver, however find qualitatively similar results and thus present only individual level analysis here.

economic indicators (AIR, 2013). In the current analysis, we may be concerned about multiple types of attrition. First, we may be concerned about household level attrition, which may be primarily poverty or mobility related. Second, we may be concerned about potential bias where certain households are more likely to have births due to demographics or other unobserved factors. To examine these differences, we first look at mean differences in the household level averages between cross-sectional and panel households, and conduct t-tests to determine if there are significant differences (Appendix Table AI). Results show that although outcome variables are on average equal between panel and cross-sectional households, mothers or caregivers in cross-sectional households are more likely to be younger, widowed and live in larger households. These attrition differences are a threat to the internal validity of the estimates only if attrition is differential by treatment status. AIR (2013) shows that attrition is balanced between treatment and control arms, which is similarly the case for the pregnancy subsample (p-value = 0.927).

## 4. RESULTS

### 4.1 Descriptive statistics

Table I shows descriptive results by treatment status for the baseline (top panel) and the follow-up (bottom panel) across the four outcome indicators. The last column reports mean differences and significance levels between treatment and control. In the baseline, overall approximately 73 percent of births report antenatal care from a doctor or nurse and 63 percent of births have received at least the four recommended visits. In addition, 77 percent of the sample has met all the quality of care indicators, however, only 36 percent of births in the baseline are attended by a doctor or nurse. Of the four outcomes, skilled attendance at delivery is the only indicator with statistically significant differences between the treatment and control groups in the baseline, where the treatment group reports a lower percentage with skilled attendance in comparison to the control group (31 percent versus 41 percent). The bottom panel shows there have been few trends in the receipt of skilled antenatal care or completing four visits. Quality of antenatal care has improved among the sample as a whole (91 percent), with slightly larger increases in the treatment group. Finally, skilled attendance at birth increases slightly in the overall sample (from 36 percent to 38 percent), driven by increases in the treatment group.

The baseline statistics across all indicators are very similar in magnitude to the most recent Zambian Demographic and Health Survey (ZDHS) in 2007, which collected information on births over the five years prior to the survey. According to the ZDHS, antenatal care is nearly universal (97 percent), 59 percent of the births in the sample complete at least the recommended four visits and 42 percent of births were attended by a doctor or nurse (CSO et al. 2009).

Appendix AII presents descriptive indicators for control variables utilized in regression analysis as well as differences between treatment and control samples. On average mothers or primary caregivers are 28 years old and have completed four years of education. The majority of mothers or primary caregivers are in a union or marriage, while approximately four percent are widows, eight percent are divorced and 13 percent have never been married. The average number of household members is just over six people and the sample is split

equally between the three pilot districts. There are no significant differences in any of the control variables across treatment status.

### 4.2 Impact results

Table II presents the unadjusted impact estimates across all four outcomes with sample sizes and R squared values reported at the bottom of the table. Results show that there are no significant program impacts across all indicators.[9] Robustness checks including covariates show virtually no changes from the unadjusted results, although program impact coefficient for skilled attendance approaches weak positive significance. The adjusted results are reported in Table AIII.

### 4.3 Heterogeneous impacts

Following previous literature finds program impacts of transfers among subgroups, we conduct sensitivity analysis to see if there are potential heterogeneous impacts of the CGP transfer. There are a number of reasons we may expect heterogeneous impacts of the transfer. If the transfer works to empower women or give them greater bargaining power, there is a possibility a greater proportion of the income could be allocated to women-specific goods and services. We test this pathway through interacting the program treatment effect with mother or primary caregivers education and mother's age, both proxies for bargaining power. In addition, to test if supply-side considerations are a constraint in healthcare utilization outcomes, we interact the program treatment effect with the index of health services as well as the distance to the nearest health facility. The remaining specifications of the regression analysis, including covariates are unchanged from the main equation 2. The heterogeneous results are reported in Table III, displayed similarly to results from the main analysis. Findings indicate that across all antenatal outcomes there is no evidence of significant heterogeneous impacts. However, for skilled attendance at birth, women who have access to better health services in their community are more likely to give birth with a doctor or nurse (both significant at the 1% level). In addition, when interaction terms are added, the program impact coefficient for both receipt of any skilled antenatal care (coeff: 0.383, t-stat: 2.40) and quality of antenatal care (coeff: 0.238, t-stat: 1.87) approach or become significant. This appears to be driven by interactions with mother or caregiver's age, which are weakly significant and negative. This indicates the program may have a larger impact on younger mothers or caregivers in the sample as compared to older mothers or caregivers, however these results should be taken as suggestive, as the interaction terms are significant only at the $p<0.10$ level.

## 5. DISCUSSION AND CONCLUSION

Using a large-sample RCT, we evaluate the impact of the CGP, an unconditional SCT program on a range of antenatal care indicators and skilled attendance at birth. We find no measurable program impacts for the full sample on any outcome indicators. We believe this is the first rigorous large-scale evaluation of an unconditional SCT on maternal healthcare

[9]The 95% Confidence Intervals (CI) for the coefficients reported in Table II are as follows: 1) Skilled antenatal care (CI: −0.117 −0.164), 2) Four antenatal care visits (CI: −0.226 – 0.0290, 3) Quality of antenatal care (CI: −0.092 – 0.074), Skilled attendance at birth (CI: −0.036 – 0.180).

indicators and the first of any type of cash transfer program in Africa. However, since the CGP was not designed with the objective of affecting maternal health, this lack of program impact cannot be seen as a failure of the program to meet its stated objectives. This lack of measurable effect may also be an artifact of the program location, as the three districts are in rural areas with high levels of poverty and food insecurity and with low access to and with poor quality health facilities. Thus if supply-side constraints are binding, even if increased income may have stimulated demand for healthcare, because there were no increases in supply of services, there may be a limit on the observable program impact. Indeed, consistent with this hypothesis we find heterogeneous impacts for skilled attendance at birth among a sample of women residing in households having better access to maternal health services. We find no evidence that women who have greater bargaining power as proxied by age and higher education are better able to leverage resources for use for their health care. This mirrors findings from the main evaluation indicating no measurable effects of the CGP were found on women's household decision-making power (AIR 2013). Thus, at least in the case of skilled attendance at delivery, access to healthcare services appears to be key to leveraging additional cash inflows to achieve better service utilization, suggesting that tailoring complementary services or matching supply-side interventions can enhance the impact of unconditional poverty-targeted SCTs for maternal health in similar settings.

There are a number of important policy lessons learned from these results. First, our findings underscore the importance of quality service provision in improving utilization of antenatal care and skilled delivery at birth, either implemented jointly or independently of transfer programs. The Government of Zambia has made efforts to improve maternal healthcare in the last decade through dedicated programming, including the removal of user fees and addressing supply-side bottlenecks. However, according to the DHS, using direct sisterhood method estimates, Zambia's maternal mortality ratio (MMR) increased from 646 to 729 deaths per 100,000 live births between 1996 and 2002, and showed only modest decreases in 2007 to 591 deaths per 100,000 live births, making it one of the highest in the world (Zambian Central Statistics Office et al. 2003, 2009).[10] Thus, there is need for more dedicated funding or innovative programming in order to increase service quality and coverage for maternal health.

If SCTs are specifically aimed at maternal or neonatal health, considerations could also be given to how programs might seek to actively empower or increase bargaining power of women, or increase spending on gender-friendly goods. For example, several CCTs in Africa (Eritrea, Mozambique and Senegal) have built in maternal health related conditions, aimed at directly incentivizing and prioritizing use of the transfer on maternal health (Glassman et al. 2013). However, such initiatives must carefully consider the context of the intervention, in particular the availability of health services and ensure that the transfer is large enough to offset the additional private cost of conditions (Jones et al. 2011). For example, in extreme resource-poor settings, transfers could be offered in conjunction with demand-side

[10]Although Zambia is rated by the World Health Organization as making "insufficient progress" in reductions, the small numbers of maternal deaths, and hence the large confidence intervals associated with these estimates make it difficult to assess trends. For example, Zambia's 2007 MMR 95 percent confidence interval is 450–752 (Zambian Central Statistics Office et al. 2009, pp. 324). Confidence intervals for the previous estimates were not calculated.

financing, health insurance schemes focused on obstetric care, or in areas where supply-side programming (results-based financing and other health system initiatives) are also operating (Jones et al. 2011; Sharan et al. 2010).

There are important limitations to the analysis. First, all outcome and control indicators are self-reported, and thus subject to recall and other biases. In addition, global positioning surveillance (GPS) information for the health facility data was not gathered, thus for those clusters without a facility in the community, no linkage can be made to a more complex measure which would take into account the relative distance to the next closest service point. Finally, we acknowledge that the evaluation was not powered on maternal health indicators, and was instead based on child anthropometry indicators. However, we can provide some robustness checks on the expected size of average change in our outcomes based on simulated increases in expenditure. To do this, we calculate the relationship between per capita expenditure and our maternal health outcomes using the baseline data. We simulate the effect of a universal increase of a 0.33 standard deviations in average expenditure, or the approximate amount of the transfer, similar to the methodology used in Seidenfeld and Handa (2011) to predict program impacts. We find virtually no increases in our outcome indicators, and in fact, the only indicator which significantly improves with the expenditure quintile is skilled attendance at birth. This evidence suggests wealthier households in our sample are not using additional cash to buy more maternal health, which is consistent with our findings and suggestion more than just an income effect is needed to induce greater maternal healthcare utilization.

In conclusion, it is important to highlight research lessons to apply to future similar evaluations for SCTs and dedicated maternal health voucher or incentive schemes such as those currently underway in Afghanistan, Bolivia, Malawi and the Philippines (Chavez 2010; Fernandez and Olfindo 2011; Glassman et al. 2013). The first lesson, particularly for SCTs without a maternal health focus is to plan for sample sizes sufficient to analyze maternal health outcomes and conduct robust analyses, which will vary by setting depending on research design and underlying prevalence of indicators in the study population. If attention is not paid to this in the early design stages, lack of impact may be attributed to small sample sizes. Since publication bias will favor significant impacts, lack of publication when no impacts are found may lead to a false sense of overall effectiveness of transfers in improving health. Second, there is need for standardization of measures, moving beyond service utilization to healthcare outcomes (birth weight, neonatal mortality, maternal mortality and morbidities) as well as quality of services received (Glassman et al. 2013). It is also crucial to measures supply-side and access issues, to understand how they link to care utilization and to demonstrate casual pathways through which outcomes are realized. These may be collected in dedicated facility surveys, or as perceptions/knowledge of services and distance to access points in household survey questionnaires. In some cases, if government monitoring and facility data is of high quality and at a low level of aggregation, these data can be matched to evaluation data using GPS data or other methods. This is particularly important in the case of dedicated schemes where there may be supply-side improvements to health infrastructure as part of the program. Finally, there is a lack of cost-effectiveness analysis on measured impacts to assess trade-offs and potential synergies in delivering benefits to mothers, children and households in resource-poor settings. Given the volume of

forthcoming research on this topic, future publications should prioritize comparisons and policy recommendations by typology of program, recognizing that incentives, conditionalities and program objectives are likely to play a large role in the potential of programs to improve maternal health and healthcare utilization.

## Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

## Acknowledgments

The Child Grant impact evaluation was commissioned by the Government of Zambia (GRZ) through the Ministry of Community Development, Mother and Child Health and funded by a consortium of donors including DFID, UNICEF, Irish Aid, and the Government of Finland. Peterman received additional funding from the William and Flora Hewlett Foundation for analysis of data. We are grateful to participants at the Global Health Metrics and Evaluation Conference 2013 and the Transfer Project Annual Workshop 2013 for helpful comments; to Palm Associates in Lusaka for excellent data collection, to Stanfeld Michelo and Manzunzo Zulu (Government of Zambia), Paul van Ufford and Charlotte Harland (UNICEF) and Kelly Toole (DFID) for their support in the evaluation and helpful comments on the paper, to Cassandra Jessee and Leah Prencipe of AIR for excellent program management and research assistance and to Aimee Urata for editorial assistance.

## 7. WORKS CITED

Agha S. Impact of a maternal health voucher scheme on institutional delivery among low income women in Pakistan. Reproductive Health. 2011; 8:10. [PubMed: 21539744]

Amarante, V., Manacorda, M., Miguel, E., Vigorito, A. Working paper. University of California; Berkeley: 2012. Do cash transfers improve birth outcomes? Evidence from matched vital statistics, program and social security data.

American Institutes for Research (AIR). Zambia's Child Grant Program: 24-month impact report. Washington, DC: Author; 2013.

Barber SL. Mexico's conditional cash transfer programme increases cesarean section rates among the rural poor. European Journal of Public Health. 2009; 20(4):383–388. [PubMed: 19933778]

Barber SL, Gertler PJ. Empowering women to obtain high quality care: Evidence from an evaluation of Mexico's conditional cash transfer programme. Health Policy and Planning. 2008; 24(1):18–25. [PubMed: 19022854]

Central Statistics Office (CSO) [Zambia], Central Board of Health [Zambia] and ORC Macro. Zambia Demographic and Health Survey 2001–2002. Calverton, Maryland, USA: Central Statistics Office, Central Board of Health, and ORC Macro; 2003.

Central Statistical Office (CSO), Ministry of Health (MOH), Tropical Diseases Research Centre (TDRC), University of Zambia, and Macro International Inc. Zambia Demographic and Health Survey 2007. Calverton, Maryland, USA: CSO and Macro International Inc; 2009.

Chavez, F. [7 November 2013] Bolivia: Cash for Checkups to Slash Maternal Deaths. 2010. http://www.ipsnews.net/2010/03/bolivia-cash-for-checkups-to-slash-maternal-deaths/

Das A, Rao D. India's Janani Suraksha Yojana: further review needed. The Lancet. 2011; 377(9762): 295–6.

Davis B, et al. Evaluating the Impact of Cash Transfer Programmes in sub-Saharan Africa: an Introduction to the Journal of Development Effectiveness Special Issue. Journal of Development Effectiveness. 2012; 4(1):1–8.

de Brauw, A., Peterman, A. Comunidades Solidarias Rurales. Washington, D.C: 2011. Can Conditional Cash Transfer Programs Improve Maternal Health Outcomes? Evidence from El Salvador's. IFPRI Discussion Paper 1080

Fernandez, L., Olfindo, R. Overview of the Philippines Conditional Cash Transfer Program: The Pantawid Pamilyang Pilipino Program. World Bank, Philippine Social Protection Note; 2011.

Glassman, A., Duran, D., Koblinsky, M. CGD Policy Paper 019. Washington DC: Center for Global Development; 2013. Impact of Conditional Cash Transfers on Maternal and Newborn Health.

Gutiérrez, JP. study team. Evaluación Externa de Impacto del Programa de Transferencias Monetarias Condicionadas. Banco Interamericano de Desarrollo; 2011.

Hanlon J. It is Possible to Just Give Money to the Poor. Development and Change. 2004; 25(2):375–383.

Jones, N., Samuels, F., Gisby, L., Presler-Marshall, E. Overseas Development Institute Background Note. ODI; London: 2011. Rethinking cash transfers to promote maternal health: good practice from developing countries.

Khan ME, Hazra A, Bhatnagar I. Impact of *Janani Suraksha Yojana* on Selected Family Health Behaviors in Rural Uttar Pradesh. Journal of Family Welfare. 2010; 56:9–22.

Lagarde M, Haines A, Palmer N. Conditional Cash Transfers for Improving Uptake of Health Interventions in Low- and Middle-Income Countries: A Systematic Review. JAMA. 2007; 298(16):1900–1910. [PubMed: 17954541]

Lim S, Dandona SL, Hoisington JA, James SL, Hogan MC, Gakidou E. India's *Janani Suraksha Yojana*, a conditional cash transfer programme to increase births in health facilities: An impact evaluation. The Lancet. 2010; 375(9730):2009–2023.

Lozano R, Wang H, Foreman KJ, et al. Progress towards Millennium Development Goals 4 and 5 on maternal and child mortality: an updated systematic analysis. The Lancet. 2011; 378(9797):1139–1165.

Powell-Jackson T, Morrison J, Tiwari S, Neupane BD, Costello AM. The experiences of districts in implementing a national incentive programme to promote safe delivery in Nepal. BMC Health Services Research. 2009a; 9:97. [PubMed: 19508710]

Powell-Jackson, T., Neupane, BD., Tiwari, S., Tumbahanghe, D., Manandhar, Costello, AM. The impact of Nepal's national incentive programme to promote safe delivery in the district of Makwanpur in Dov Chemichovsky. In: Hanson, Kara, editor. Innovations in Health System Finance in Developing and Transitional Economies (Advances in Health Economics and Health Services Research 21). Bingley, West Yorkshire: Emerald Group Publishing Limited; 2009b. p. 221-249.

Seidenfeld, D., Handa, S. Zambia's Child Grant Program: Baseline Report. American Institutes for Research (AIR); Washington DC: 2011.

Sharan, M., Ahmed, S., Naimoli, JF., Ghebrehiwet, M., Rogo, K. Working paper. World Bank; Washington DC: 2010. Health System Readiness to Meet Demand for Obstetric Care in Eritrea: Implications for Results-Based Financing (RBF).

Sosa-Rubi SG, Walker D, Servan E, Bautista-Arredondo S. Learning effect of a conditional cash transfer programme on poor rural women's selection of delivery care in Mexico. Health Policy and Planning. 2011; 26(6):496–507. [PubMed: 21278371]

UNICEF. The state of the world's children. UNICEF; New York: 2008.

Urquieta-Salomon J, Angeles G, Mroz T, Lamadrid-Figueroa H, Hernandez B. Impact of *Oportunidades* on skilled attendance at delivery in rural areas. Economic Development and Cultural Change. 2009; 57(3):539–558.

## Table I

Baseline and follow-up means for antenatal care and skilled attendance outcomes by Child Grant Program treatment status

| **Baseline** | **All** | **Control** | **Treatment** | **Difference** |
|---|---|---|---|---|
| Received antenatal care from doctor or nurse | 0.73 (0.01) | 0.74 (0.02) | 0.71 (0.02) | −0.04 (0.03) |
| At least four antenatal care visits | 0.63 (0.01) | 0.64 (0.02) | 0.63 (0.02) | −0.02 (0.03) |
| Quality of antenatal care | 0.77 (0.01) | 0.76 (0.02) | 0.79 (0.02) | 0.02 (0.02) |
| Birth attended by doctor or nurse | 0.36 (0.01) | 0.41 (0.02) | 0.31 (0.02) | −0.10 (0.03)** |
| *N* | 1,155 | 580 | 575 | |
| Follow-up | All | Control | Treatment | Difference |
| Received antenatal care from doctor or nurse | 0.72 (0.02) | 0.73 (0.03) | 0.72 (0.03) | −0.02 (0.04) |
| At least four antenatal care visits | 0.64 (0.02) | 0.69 (0.03) | 0.59 (0.03) | −0.11 (0.04)* |
| Quality of antenatal care | 0.91 (0.01) | 0.89 (0.02) | 0.92 (0.02) | 0.01 (0.03) |
| Birth attended by doctor or nurse | 0.38 (0.02) | 0.39 (0.03) | 0.36 (0.03) | −0.03 (0.04) |
| *N* | 559 | 288 | 271 | |

Notes: Standard errors in parenthesis.

* $p<0.05$;

** $p<0.01$.

Sample includes all births in households taking place 15 months prior to baseline and follow-up surveys. Quality of antenatal care is an indicator of having received Voluntary Counseling and Testing for HIV, tetanus vaccination and malaria prevention during antenatal care.

## Table II

Unadjusted regression results of the Child Grant Program on antenatal care and skilled attendance at birth

| | Skilled antenatal care | Four antenatal visits | Quality of antenatal care | Skilled birth attendance |
|---|---|---|---|---|
| Program impact (DID) | 0.023 (0.33) | −0.098 (1.53) | −0.009 (0.21) | 0.072 (1.32) |
| CGP participant | −0.048 (1.05) | −0.014 (0.32) | 0.022 (0.76) | −0.096 (1.75) |
| Time (24 month follow-up = 1) | −0.063 (1.46) | 0.025 (0.49) | 0.083 (2.85) ** | −0.021 (0.64) |
| Shangombo district | 0.082 (2.02) * | 0.014 (0.31) | −0.103 (3.83) ** | −0.183 (3.11) ** |
| Kaputa district | −0.150 (2.45) * | 0.100 (2.38) * | −0.140 (5.02) ** | −0.180 (2.50) * |
| Constant | 0.764 (17.54) ** | 0.591 (13.77) ** | 0.833 (37.66) ** | 0.530 (8.76) ** |
| $R^2$ | 0.05 | 0.01 | 0.03 | 0.04 |
| *N* | 1,787 | 1,787 | 1,787 | 1,714 |

Notes: Linear probability models with robust t-statistics in parentheses.

* $p<0.05$;

** $p<0.01$.

Sample includes all births in households taking place 15 months prior to baseline and follow-up surveys. Quality of antenatal care is an indicator of having received Voluntary Counseling and Testing for HIV, tetanus vaccination and malaria prevention during antenatal care.

## Table III

Adjusted regression results of the impact of Child Grant Program on antenatal care and skilled attendance at birth with interactions

| | Skilled antenatal | Four antenatal visits | Quality of antenatal care | Skilled birth attendance |
|---|---|---|---|---|
| Program impact (DID) | 0.383 (2.40) * | −0.139 (0.81) | 0.238 (1.87) | 0.047 (0.27) |
| Mother/caregiver grade completed * Program impact | −0.001 (0.07) | −0.000 (0.03) | −0.009 (1.17) | 0.008 (0.83) |
| Mother/caregiver highest grade completed | 0.003 (0.83) | 0.002 (0.68) | 0.005 (1.42) | 0.013 (3.05) ** |
| Mother/caregiver age (years) * Program impact | −0.008 (1.73) | −0.003 (0.72) | −0.007 (1.77) | −0.002 (0.29) |
| Mother/caregiver age (years) | 0.006 (0.86) | 0.013 (1.69) | 0.019 (2.72) ** | 0.000 (0.01) |
| Index of health services in cluster * Program impact | 0.035 (0.70) | 0.031 (0.60) | −0.012 (0.31) | 0.114 (3.09) ** |
| Index of health services in cluster | 0.005 (0.30) | 0.062 (4.11) ** | 0.016 (0.93) | 0.027 (1.19) |
| Log distance to nearest health facility * Program impact | −0.076 (1.78) | 0.065 (1.69) | −0.008 (0.31) | 0.029 (0.82) |
| Log of distance to nearest health facility | 0.022 (1.18) | −0.025 (1.20) | 0.007 (0.43) | −0.036 (1.83) |
| CGP participant | −0.027 (0.61) | 0.002 (0.06) | 0.019 (0.67) | −0.086 (1.76) |
| Time (24 month follow-up = 1) | −0.066 (1.57) | 0.023 (0.45) | 0.083 (2.72) ** | −0.007 (0.22) |
| Shangombo district | −0.008 (0.15) | −0.041 (0.88) | −0.127 (3.84) ** | −0.280 (4.85) ** |
| Kaputa district | −0.165 (2.88) ** | 0.086 (2.12) * | −0.130 (3.43) ** | −0.218 (3.30) ** |
| Constant | 1.012 (5.81) ** | 0.388 (2.30) * | 0.667 (4.32) ** | 0.901 (4.70) ** |
| $R^2$ | 0.09 | 0.06 | 0.05 | 0.12 |
| $N$ | 1,787 | 1,787 | 1,787 | 1,714 |

Notes: Linear probability models with robust t-statistics in parentheses.

* $p<0.05$;

** $p<0.01$.

Sample includes all births in households taking place 15 months prior to baseline and follow-up surveys. Quality of antenatal care is an indicator of having received Voluntary Counseling and Testing for HIV, tetanus vaccination and malaria prevention during antenatal care. Health services index is constructed through factor analysis containing indicators that the cluster health facility offers obstetric care, ANC, carries tetanus vaccines, offers VCT, malaria tests, pregnancy test, family planning and has a mobile clinic. All models control for additional socioeconomic characteristics found in Table AII including a vector of cluster level prices.