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The impact of Zambia's unconditional child grant on schooling and work: results from a large-scale social experiment

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Abstract

This paper reports the impact on child schooling and work of the Government of Zambia's Child Grant Program (CGP), an unconditional cash transfer program targeted to households with children under age 3 years in three districts of the country. Although the CGP's focus is on very young children, we look to see if the program has impacts on older children who are not the explicit target group. We use data from a large-scale social experiment involving 2,519 households, half of whom were randomized out to a delayed-entry control group, that was implemented to assess the impact of the program. We find that the CGP has no discernable impact on school enrollment of children age 7–14. However when we break the sample by older (11–14) and younger (7–10) children - based on the grade structure of the Zambian schooling system - we find a significant impact among children age 11–14 which coincided with the exact age range where sharp drop-out begins to occur in Zambia with point estimates in the range of 7–8 percentage points. Finally, we provide evidence on the potential pathways through which the unconditional cash transfer impacts on enrollment. Households in the CGP spend more on education, and in particular on uniforms and shoes, two items cited as key barriers to school enrollment in study areas.

Keywords

child labour; schooling; Zambia; unconditional cash transfer; social experiment

1 Introduction

This paper reports the impact on child schooling and work of the Government of Zambia's Child Grant Program (CGP), an unconditional cash transfer program targeted to households

with children under age three years in three districts of the country. Since the mid 1990s, and following the successful implementation of large scale programs in Brazil, Mexico and South Africa, cash transfers have become an important part of the poverty alleviation toolkit in developing countries, even among the poorest where for many, such programs seemed both administratively complex or simply unaffordable. In Africa alone there are around 30 programs with long term development objectives across 20 countries (Garcia & Moore, 2012).

The ‘African model’ of cash transfers, particularly among the newer programs which began since 2006, has several distinguishing features which separate it from those in Latin America. First and foremost, programs tend to be unconditional, and even in the few places where conditions are formally part of the program rules, these are rarely monitored and never punitively sanctioned.ⁱ The second feature is the distinct eligibility criteria and targeting method. Eligibility criteria are typically extreme poverty *and* demographic features such as labor-constraints (e.g. Zimbabwe, Malawi, Liberia) or presence of orphans or other vulnerable children (Ghana, Kenya, Lesotho). Targeting itself tends to have an important role for the community, either in compiling initial eligibility lists (Kenya, Ghana) or vetting final lists which have been compiled by those outside the community (Zimbabwe, Lesotho).

As in Latin America, the new African programs have been accompanied by a rigorous culture of evaluation and estimates by the Transfer Project suggest that there now exist 19 large-scale rigorous (i.e. with some sort of control or comparison group) evaluations of national programs across 13 countries in sub-Saharan Africa alone, indicating that the evidence base from SSA will soon far exceed that from any other region of the world in terms of both quantity of evaluations and range of topics covered.ⁱⁱ This paper contributes to the small but growing evidence on whether unconditional cash transfers can work in SSA, focusing specifically on schooling and child work; indeed there is only one other published piece that provides evidence on the schooling impacts of a national cash transfer program in sub-Saharan Africa (SSA) (Kenya CT-OVC Study Team 2012). In contrast there is a substantial evidence base on the impacts on schooling of *conditional* cash transfers (CCTs). A review by Fiszbein & Schady (2009) suggests that CCTs for schooling are effective in raising school enrolment and attendance, and in middle income countries where primary school enrolment rates are already high CCT impacts have been more significant at the secondary school level.

The intriguing and even exciting feature of UCTs is that, since money is not tied to behavior, impacts may be found in any sphere, depending on what the major constraints are facing households, and how the household itself believes the cash can best address its needs. Indeed this attractive feature of UCTs has often been used against them to justify the imposition of conditions, for example by arguing that households do not internalize the full social benefits of investments in areas such as health and education and so must be compelled to do so.

ⁱIn Lesotho and Ghana programs are nominally conditional but monitoring is not routinely performed. Tanzania is the only new program with conditions that come with punitive sanctions.

ⁱⁱTransfer Project presentation made at African Union Expert Consultation on Children and Social Protection, Cape Town, South Africa April 2014, available at http://www.cpc.unc.edu/projects/transfer/events/expert-consultation-meeting-on-children-and-social-protection-systems-in-africa/UNICEF_Handa_AU_v3.pdf.

Where one stands in the UCT versus CCT debate often depends on how one views a cash transfer. In Africa, the social protection movement has been primarily driven by poverty alleviation within a human rights framework, which, along with the relatively low supply of services such as health and schooling, has led to UCTs as the standard approach in this sector. There have been several randomized studies compare schooling CCTs and Unconditional Cash Transfers (UCTs). Baird et al. (2011) use data from an experiment in Malawi that compares a CCT to a UCT between 2007 and 2009 and find that the UCT has a lower impact on school enrollment but a larger impact on non-conditioned outcomes such as reducing marriage and teen pregnancy. Benhassine et al. (2013) run a large randomized experiment in Morocco to evaluate the impact of an unconditional cash transferⁱⁱⁱ paid to fathers of school-aged children in poor rural communities and find no difference on the impact on school participation. Akresh et al. (2013) find no significant difference between a UCT and CCT conditional on enrollment in Burkina Faso but larger impacts in the CCT on 'marginal' children, those who were most at risk of not going to school. A Campbell Systematic Review by Baird et al. (2013) found no difference in schooling impacts between CCTs and UCTs and a recent review of evidence from the Transfer Project that focuses on secondary school-age children concludes that national UCT programs in Africa tend to have schooling impact that are at least as large as those reported from evaluations of national CCTs.^{iv} However CCTs have traditionally been implemented in Latin America while most programs in sub-Saharan Africa (SSA) are SCTs, so cross-country comparisons of UCTs versus CCTs impacts are likely to confound overall levels of development, differences in supply side constraints, differences in out-of-pocket costs associated with schooling, and values placed on education. At the very least though, there seems to be an emerging body of evidence that the income elasticity of demand is such that conditionality of cash transfer programs tied to schooling does not add much in terms of schooling impacts. None of the studies cited above report on the additional administrative cost of conditions.

In this article we take advantage of the unconditional nature of the Zambian CGP, which targets families with very young children and whose objectives are focused on their health and development, to see if the program has an impact on the schooling and work of school-age children who in principle are not the main target population of the program. We use data from a large-scale social experiment involving 2,519 households, half of whom were randomized out to a delayed-entry control group, that was implemented to assess the impact of the program. As mentioned earlier, there have been several published studies on the impact of CCTs on schooling (Schultz, 2004; Attanasio et al., 2010; Sadoulet et al., 2004; Dammert, 2009; Paxson & Schady, 2007), all showing different magnitudes of positive impacts though in each of these cases of course program participation is conditional on schooling related behavior and so the results are somewhat straightforward provided the programs are implemented well and benefit levels are set high enough. Published evidence from UCTs is less abundant, with the one published paper from the Kenyan program cited above and a paper on the impact of an unconditional cash transfer - the BDH - in Ecuador (Schady & Araujo, 2006), though a substantial grey literature does exist.^v We characterize

ⁱⁱⁱEven though the cash transfer was not conditional on schooling it was labeled as an education support program.

^{iv}<http://www.cpc.unc.edu/projects/transfer/publications/briefs/TPEducationBrief2014Sept28.pdf>

^vSee evaluation reports from Ghana, Zambia and Lesotho at www.cpc.unc.edu/projects/transfer.

our contribution to the literature therefore as being only the second article that studies the impact of a national unconditional program on schooling. However, a key difference between this program and the Kenya CT-OVC is that the CGP's objectives are focused on very young children, and so schooling effects, particularly among older children, would be considered somewhat secondary level or 'spillover' effects facilitated in part by the unconditional nature of the program; the potential for such wider impacts in programs that are not tightly conditioned may not be fully appreciated in this literature.

2 The Zambian Child Grant Program

The Zambian Government's Ministry of Community Development and Social Services (MCDSS)^{vi} initiated the Child Grant Program (CGP) in 2010 as part of their piloting of a series of alternative cash transfer designs in the country. This categorical model targets any household with a child under 3 years old in three districts with high poverty and rates of infant mortality and under-nutrition: Kalabo and Shangombo in the Western Province and Kaputa in Northern Province. All three districts are near the Zambian border with either the Democratic Republic of Congo (Kaputa) or Angola (Shongombo and Kalabo) and require a minimum of two days of travel by car to reach from Lusaka. In addition, Shongombo and Kalabo are cut off from Lusaka by a flood plain that turns into a river in the rainy season. Hence these three districts represent some of the most remote locations in Zambia, and indeed part of the learning around the CGP is to test delivery and monitoring mechanisms for remote communities in anticipation of an eventual scaled up program. Recipient households receive a flat (i.e. irrespective of household size) 70 kwacha (KW) a month (equivalent to U.S. \$12) transfer, an amount deemed sufficient to purchase one meal a day for everyone in the household for one month. Payments are made every other month through a local pay-point manager, and there are no conditions to receive the money. The primary objectives of the program are to address young child health and nutrition and household food security, secondary outcomes relate to asset building and economic strengthening.

3 Conceptual framework.

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

^{vi}The Ministry changed name to Ministry of Community Development, Mother and Child Health (MCDMCH) in 2013.

4 Evaluation Design

The CGP impact evaluation is a multisite cluster randomized controlled trial (RCT). Within each of the three geographically targeted districts, selected based on their high poverty and child malnutrition rates (Kaputa in Northern Province, and Kalabo and Shangombo in Western Province), communities designated by Community Welfare Assistance Committees (CWACs) were randomly assigned to either the treatment condition to start the program in December 2010 or to the delayed control condition to start the program at the end of 2013. The MCDSS decided to implement a randomly assigned delayed control group because it did not have sufficient resources or capacity to deliver the program to all eligible households immediately. Thus, the Ministry instituted a policy of randomly assigning communities to current or delayed treatment, deeming it to be the most ethical and fair way to select the order in which communities receive the resources as they became available.

The Ministry conducted the first step of the randomization process by selecting and ordering 30 CWACs within each district (out of roughly 100 CWACs in each district) through a lottery held at the Ministry headquarters in June 2010 with Ministry staff from the three districts participating. This process created transparency and understanding about how the communities were selected for everyone involved in implementing the program. After the 90 CWACs were randomly selected (30 from each district) for the study, CWAC members and Ministry staff identified all eligible households, namely households with at least one child under 3 years old, in the study communities. Among all eligible households in each CWAC, 28 households were randomly sampled from for inclusion in the study.^{vii} Baseline data were collected for the 28 randomly sampled households in each randomly selected CWAC in each district (30 CWACs per district) and located in one of the three geographically targeted districts. The final study sample size was just over 2,500 households.

The baseline data collection began before CWACs were randomly assigned to treatment or control conditions, thus neither the households nor the enumerators knew who would enter immediately and who would enter later. Random assignment occurred after the baseline data collection was complete, with the Ministry's Permanent Secretary flipping a coin to determine whether the first half of the list of randomly selected CWACs would be in the treatment or the delayed control condition. This process was conducted in public with local officials, Ministry staff, and community members present as witnesses; given the public nature of the randomization and the fact that this was a government program study participants could not be blinded at follow-up. Note that this is a multisite social experiment because random assignment of CWACs occurs within each of the three districts (See Fig. A1 - Flowchart of study design in Annex A). Table 1 shows the timing of data collection. Wave 3, administered during the harvest season, entailed a shorter survey focusing on consumption in order to assess the impact of the CGP on consumption smoothing by comparing impacts across the agricultural cycle. Since the 30- and the 36-month observations are gathered during the same (academic) year we do not use the 30-month survey in this paper.

^{vii}The sample size was determined through a power analysis to ensure that the study was able to detect meaningful effects. The 28 households per CWAC and 30 CWACs per district result from this power analysis.

The core survey instrument used in each round is a household questionnaire, though a community questionnaire was also administered in each wave to gather information on local prices, shocks, and other contextual characteristics; a health facility questionnaire was also administered at baseline.. The household survey is multi-topic and very similar, both in layout and coverage, to two national surveys (the Living Conditions and Monitoring Survey – LCMS; and the Zambia Demographic and Health Survey - ZDHS). The questionnaire contained detailed modules on consumption, household assets and conditions, health, education, productive activities, early child development, subjective poverty and food security, and anthropometric measurements among others. The main respondent to the household questionnaire is typically the intended CGP recipient, the primary care-giver of young children, virtually all of whom are women.

The study underwent ethical review at the American Institutes for Research (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>).

Loss to follow-up (attrition) in the study is quite low, with the highest rates (9 per cent) reported at 24-months, driven by the drying of the Cheshi Lake in Kaputa District. However many of these households were recovered in subsequent rounds so that overall attrition by wave 4 was only 2 per cent (see Table A1 in the Annex). Both overall attrition and selective attrition are presented in official evaluation reports and neither is significant^{viii}. Nevertheless, specially constructed household weights (inverse probability weights) are used to ensure external validity, that is, that the impact estimates are representative of all eligible households.

We also check for overall and selective attrition at the individual level. In particular, we investigated differential attrition by testing for similarities at baseline between treatment and control groups for balanced panel children; the only significant differences are the proportion of children living in households where the intended respondent is divorced and household size (see Table A2 in the Annex). We then checked for overall attrition by testing whether the full sample of children 7–14 differ from the panel sample of children 7–14 at baseline (see Table A3 in the Annex). Balanced panel children are slightly younger, more likely to live in Shongombo and less likely to live in Kaputa, with slightly less members age 19–35 and slightly more aged 36–55 in the household. In the Annex we also report the same analysis for the unbalanced panel and these show only two differences between the two samples.

^{viii}On one hand, no differential attrition between the treatment and the control group is found and therefore internal validity is preserved. On the other hand, testing for overall attrition indicates that the overall baseline sample does not differ, on average, from those households that remain in the sample (after two and/or three years). Still, the highest attrition rate (9 per cent) reported at 24-months is driven by migration out of Kaputa District due to the drying of the Cheshi Lake. However, many of these households were recovered by the third year follow up (i.e. overall attrition at 36 month is only 2 per cent). details of this analyses are available in the evaluation reports on the Transfer Project website or by request from the correspondign authors.

5 Zambian Educational System

Zambia's school system consists of a primary (7 years) and a secondary (5 years) level. Children are expected to enter primary school at age 7 and complete lower basic (grades 1–4) at age 11 and middle basic (grades 5–7) by age 13. Students take common examinations at the end of the primary cycle and successful pupils are awarded a Certificate of Primary Education and allowed to continue onto secondary education. Secondary education is divided into junior secondary (or upper basic) – which comprises grades 8 and 9 – and senior secondary (or high school) – which corresponds to grades 10 to 12. Again, there are common examinations at the end of grade 9 (Junior Secondary School Certificate) and successful students are allowed to continue onto senior secondary. Successful completion of high school requires passing the School Certificate Examination by the end of grade 12. This examination is also used for selection into tertiary education programs at universities, colleges and technical institutes. Due to the common examinations at grades 7 and 9, these are threshold levels at which significant drop-out occurs.

Enrolment in primary education^{ix} has been steadily increasing over the last 10 years: it went from 70.5 per cent in 2001 to 85.9 per cent in 2004 and 93.7 per cent in 2012^x. Primary completion rates have increased from 63.3 per cent in 2000 to 74.1 per cent in 2004 and 91.3 per cent in 2012. However, it is important to note that children are entering school at later ages. During 2004 to 2012, less than 47 per cent of children entered school at age 7^{xi} while the rest waited to enroll later^{xii}. The reasons for late enrolment range from inadequate classroom space, long distances to schools, socio-economic and cultural beliefs^{xiii}.

Although primary completion rates are above 90 per cent, transition to lower secondary education has not improved much. According to the latest data, of those students who were enrolled in primary education in 2010, only 56 per cent continued to secondary education – a percentage similar to the one recorded in 2001. Moreover, because of inefficiencies in the transition from primary to secondary education and within the lower secondary cycle, of those who should be graduating from upper basic (junior secondary, grades 8 to 9) only about 40 per cent actually do^{xiv}.

There are a number of barriers that prevent child schooling. There are economic constraints; school is free up to grade 7, however school fees were not abolished for higher grades. Moreover, even though no formal fee is charged at primary level, there are still some informal fees that households often incur such as PTA or other types of contributions. Other indirect costs encompass, among others, uniforms and shoes; these are often compulsory at secondary level but there is social stigma attached with not owning these items even in

^{ix}Net School Enrollment measured as the total is the ratio of children of the official primary school age who are enrolled in primary school to the total population of the official primary school age.

^xWorld Development Indicators, World Bank

^{xi}The average net intake ratio in grade 1 for the period 2004–2012 was 47.6 per cent. The average net intake ratio corresponds to the number of new entrants in the first grade of primary education who are of the official primary school-entrance age, expressed as a percentage of the population of the same age.

^{xii}As suggested by the average gross intake ratio in grade 1 for the period 2004–2012, 118 per cent. The average gross intake ratio is the number of new entrants in the first grade of primary education regardless of age, expressed as a percentage of the population of the official primary entrance age.

^{xiii}http://www.sacmeq.org/sites/default/files/sacmeq-reports/sacmeq-iii/national-reports/s3_zambia_final.pdf, p 16.

^{xiv}World Development Indicators, World Bank.

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

6 Data and sample

As mentioned earlier the CGP is targeted to families with very young children and its primary objectives relate to young child health and nutrition. There are 2,519 households and 14,565 people in the evaluation study, including 4,793 children ages 5 and under, with the largest number under 1 year old (1,427). Figure 1 below capture the age distribution of children age 0–17 at baseline and then after 2 and 3 years. These density graphs show indeed that the majority of children in these beneficiary households are very young at baseline and even by wave 3 (three years after program initiation) the modal age is less than 5. In contrast there are very few children over age 13 and two-thirds of households actually have no members age 14–17 in the household.

Given that the compulsory school age is 7 and households targeted for the program do not have many children over age 14, we focus our analysis on children age 7–14 in this analysis. Table 2 reports demographic and household characteristics of this group. The sample is predominantly rural and mainly engaged in agriculture. The mean household size is 7 and the sample as a whole is very young which is in line with the eligibility requirement of the program. On average two household members are children under 5 and two are between 6 and 12. There are very few elderly in the household and the majority of adults (1.2 on average) is in the age range 19–35 while the mean age of children (7–14) is 10. Virtually all of the transfer recipients – those who receive the grant – are female with a mean age of 35, 80 percent are married and 30 percent never attended school recipients never attended school. Finally, the sample is poor^{xv} with mean monthly per capita expenditure around 34 Kwacha, or less than 30 U.S. cents per person per day. Around 74 percent of children 7–14 are enrolled at baseline whereas around 6 out of ten children are involved in some form of work, either paid or unpaid. Note that paid work is quite low among this age group—only 2 percent in this sample.

The assignment into the program is random for households with children under age 3 at baseline while our analysis sample are 'non-target' children, those age 7–14. The last column in Table 2 provides the test for equality of means at baseline to assess balance among this sample of children. There is no statistically significant difference between treatment and control children in that age range in our outcome variables (schooling and

^{xv}In 2010 units, the extreme poverty line is roughly Kwacha 90.5 and the moderate poverty line is roughly Kwacha 155.

child work). All household level characteristics are also balanced except that recipients in control communities are around 4 percentage points more likely to be divorced at baseline and there are slightly more children in the age range 13–18 in treatment households. We control for both these variables in our econometric specifications (see below). Figure 2 presents density graphs for school enrollment and work by age to understand the pattern of schooling in Zambia. School enrollment rates increase steeply starting from early ages but are still only at 50 per cent by age 7 which is the compulsory age of enrollment, and continue to increase through age 12/13 at which point drop-outs begin. This is the age that coincides with the rather sharp increase in paid work displayed in the right hand panel of Figure 2. While overall rates of paid work are low, the age at which school drop-out begins also coincides with the age when unpaid work is displaced by paid work. These trends suggest that the CGP might encourage the on-time enrollment into school of younger kids who are entering the system for the first time, but may also delay drop-outs around ages 12–13. For work on the other hand, the space to have impacts is above age 13/14 and there are very few children in that age group in the CGP—we thus do not expect to see much impact, if any, on work given the nature of the target population.

Finally, Table 3 below provides some indication of the complementarity and/or substitution between child labour and schooling. Whereas almost half of the children 7–14 combine schooling with some form of work (paid or unpaid) almost a third only go to school. If we consider paid work then almost three quarters of children only go to school whereas the rest are mainly idle. This seems to support some complementarity between schooling and unpaid work and some substitution between schooling and paid work.

7 Empirical Strategy

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

$$Y(i, t) = \alpha + \beta_T T(i) + \beta_{R2} (R2) + \beta_{R3} (R3) + \beta_{TR2} (T(i) * R2) + \beta_{TR3} (T(i) * R3) \quad (1) \\ + \sum_{j=1}^J \theta_j X_j(i, t) + \varepsilon(i, t)$$

In this framework $Y(i, t)$ is the the outcome indicator (either schooling or child labour) for the individual child i at time t . Our main schooling variable captures whether the child is currently attending school, as reported by the main respondent to the household questionnaire (the intended CGP recipient): it is a dummy equal to 1 if the child is currently attending school, 0 otherwise; a child is defined as working if s/he normally does any work, either paid or unpaid, including unpaid domestic work/chores; a child is considered to be engaged in paid work if s/he spent any time (at least an hour) in paid work in the last two weeks; all child work information is collected from the main respondent – the intended CGP recipient; the paid work variable is based on a two week recall. $T(i)$ is a dummy variable indicating treatment status; R2 and R3 are dummy variables for the two time periods

considered where $R2$ refers to the 24-month follow up and $R3$ to the 36-month follow-up; β_{TR2} and β_{TR3} capture the treatment effect at time 2 and 3; X is a set of control variables; ϵ is the error term. Robust standard errors are adjusted for clustering at the CWAC level. All models are estimated using linear probability models for ease of interpretation; probit models produced similar results and are available upon request.

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

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

We estimate Eq. (1) for children 7–14 and then separately for younger and older children given the sharp drop-out rate that begins in the older age group. We provide estimates over two different samples, the unbalanced panel and the (balanced) panel. The unbalanced panel consists of children 7–14 at baseline who appear in the survey at least twice; it thus tracks outcomes among the same age cohort of children as they move through the study. For those age 7–14 years at baseline, the unbalanced panel analysis compares them to children 9–16 and 10–17 at 24- and 36-months respectively. The balanced panel consists of children who appear in all three waves of the study; while this sample is the strongest in terms of internal validity it may not be representative of the typical child in beneficiary households though the attrition analysis suggests that there should not be much difference between the two sets of estimates and this is in fact confirmed below. The benefit of the unbalanced panel is that it includes the households living in Kaputa who were briefly lost due to the drying of the lake at 24-months, but were then interviewed at 36-months.^{xvi}

^{xvi}The main evaluation reports for this study use pooled cross sectional DD impact estimates, which compare children age 7–14 with children of the same age at follow-up. This approach does not take advantage of the longitudinal nature of the study. The evaluation reports containing these estimates are publically available at <http://www.cpc.unc.edu/projects/transfer/countries/zambia>.

8 Impact estimates

We begin by showing graphically the pattern in school enrollment and work by study arm at baseline and 36-months to get a sense of the underlying patterns in the data and the likely impact of the program. Figures 3 and 4 show baseline and 36m follow-up outcomes respectively for the unbalanced panel of children age 7–14 at baseline. For schooling we see a clear separation in the two lines by around age 11 which is exactly the age that the curve begins to flatten, suggesting that the CGP might be delaying drop-out at this critical threshold. For work on the other hand, suggestion of a program effect is for paid work, where it appears like the CGP might be reducing the increase in paid work among older children. Tables for enrollment rates and work by treatment status and wave are also available in the Annex (Tables A4, A5 and A6).

Table 4 presents impact estimates on enrollment for children age 7–14 for both analytical sample, first without any covariates and then with the set of covariates described above. There are no statistically significant impact of the CGP in any wave for either sample though the 36-month impacts are larger and approach significance at 10 percent. The results on child work are reported in Table 5 and these also show no program impacts. As a robustness check we also estimated the child labor and schooling models using seemingly unrelated equations estimation. These results are reported in Table A7 and also show no significant impacts of the program on either outcome for this age group.

The slope of the enrollment-age profile is very steep below 10 years of age while drop-outs start occurring between ages 11–13 so it makes sense that the program may have different impacts for children of different ages. At older ages is also when work for pay starts becoming more of an issue, so we break the full sample into two age groups, 7–10 and 11–14 to see if perhaps the program operates differently depending on age.

Tables 8 and 9 report results for schooling and work for children age 7–10 and again, for neither specification or sample is there a program effect on either outcome. For older age children on the other hand we now see some program effects, the point estimates in *Table 10* indicating an increase in school enrollment of between 6–8 percentage points depending on the specification (with or without control variables) and sample (unbalanced or balanced panel). Table 11 indicates that there is no program effect on work (unpaid or paid) among this age group, but this is exactly the age where paid work begins to increase noticeably so we provide separate estimates for the likelihood of engaging in any paid work in Table 12. These estimates show a significant impact of the CGP in reducing the probability of paid work by 4–5 percentage points with strongest impacts in the unbalanced panel.^{xvii}

The positive impacts of the CGP on older, secondary age children are consistent with previous literature on *conditional* cash transfers that finds larger impacts at older age groups. For instance, Sadoulet et al. (2004) found that Progres/Oportunidades's impact on schooling among children aged 8 to 17 was greater at secondary than primary school level (Mexico) while Skoufias and Parker (2001) found the largest impacts on use of time for

^{xvii}The 'treated' variable in each of these tables is not statistically significant indicating no difference at baseline between the treatment and control group.

children among children age 12 and older, i.e. those of secondary school age. Both those studies also found larger impacts on girls; we also tested for differential impacts by sex of the child in the CGP data and did not find any^{xviii}.

9 The impact of CGP on school inputs

As discussed in Section 4 financial barriers remain important in Zambia, even at primary levels despite the elimination of formal school fees. Financial costs include formal or informal school fees or other costs such as transportation, uniforms and shoes, or school equipment (stationary) and these costs tend to be higher for secondary school age children. For example, teachers indicated that while students are not required to wear shoes or uniforms at primary schools these requirements are enforced at secondary schools; physical distances are also greater for secondary schools, and barriers to school access tend to affect girls disproportionately. The provision of an unconditional cash transfer has the potential to alleviate some of these barriers that inhibit educational access and in this section we investigate the potential pathways through which the CGP generates the impacts on school enrolment reported above.

For each child currently enrolled in school we collected detailed data on expenditure made by the household on each individual child on education in the current school year. Education expenditures include fees, uniform, transport, stationary and books, PTA levy and any other as reported by respondent. Of particular interest is spending on uniforms as according to our informal discussions with teachers and school administrators during field work children who cannot afford uniforms often suffer from peer pressure and stigma which can increase both absenteeism and eventual drop-out. Aside from school expenditure, we also collected information for every child aged 5 to 18, enrolled or not, on whether they have a pair of shoes. Shoes ownership can enable children to walk far distances on rough terrains protecting feet from cuts and infections and preventing diseases such as worms. Using these data we estimate DiD models using the unbalanced panel sample^{xix} to see whether education and uniform expenditures, or the probability of owning a pair of shoes is higher for children in the treatment group with respect to those in control areas. This would help us understand how the CGP has led to an increase in the school enrollment rates reported earlier.

Results for education and uniforms expenditures (log of) are reported in Table 13 while results for (ownership of) shoes are reported in Table 14. In each table we report results for all children in our analysis sample (age 7–14) and also for the older age group 11–14 since out-of-pockets are higher among this age group and impacts on schooling also appear only among the older group. Note that education (and uniform) expenditures are only estimated on the sample of children who attend school while shoes is estimated on all children, whether enrolled or not.

^{xviii}: Results are available upon request.

^{xix}We also estimated the DiD using the balanced panel sample and results are consistent—these are available upon request.

Results in Table 13 indicate that school expenditures are 48 per cent higher for treated children than for their control peers after two years among children 7 to 14 enrolled; even though lower, the difference is still large and significant (at the 10 percent level) at 36-months (29 percent). Among older children age 11–14 the differences in overall spending are also quite large at 37 and 21 percent after 2 and 3 years respectively even though statistically significant (at the 10 percent level) only at 24-month. The last two columns of Table 13, which focus only on the uniform component of overall education spending, are revealing, and show that the CGP has enabled households to increase their spending on school uniforms for children of all ages (primary and secondary level). Again the point estimates are quite impressive—between 38 and 75 per cent increase in spending on uniforms depending on the wave and age-group.

The program has also a strong and significant impact on whether children own shoes (see Table 14). Since we have information on shoes for all children, we report results for all children and then only for those currently enrolled in school. Shoe ownership increases for all school age children age 7–14 by 24–30 percentage points depending on the survey wave; treatment effects are very similar among older school age children (by 22 to 31 percentage points). Coefficients across the sample of all school age children and the sample of school age children who are actually enrolled in school are similar, indicating that shoes are not being purchased only for those attending school, but for all children in the household. This is interesting as it shows that shoes are not the only reason kids are not being sent to school.

10 Discussion and Conclusions

This paper reports the impact on child schooling and work of the Government of Zambia's Child Grant Program (CGP), an unconditional cash transfer program targeted to households with children under age 3 years in three districts of the country. Although the CGP focus is on very young children, we look to see if nevertheless the program has impacts on older children who are not the explicit target group. Indeed, since money is not tied to behavior, impacts may be found in any sphere, depending on what the major constraints are facing households, and how the household itself believes the cash can best address its needs. Although there have been several published studies on the impact of CCTs on schooling, the literature on the schooling impacts of unconditional cash transfers is scant, and evidence from national programs in Africa quite limited. We use data from a large-scale social experiment involving 2,500 households, half of whom were randomized out to a delayed-entry control group, that was implemented to assess the impact of the program in three districts in Zambia.

We find that the CGP has no discernable impact on school enrollment of children age 7–14. However when we break the sample by older and younger children – based on the grade structure of the Zambian schooling system - we find a significant impact among children age 11–14 which coincided with the exact age range where sharp drop-out begins to occur in Zambia with point estimates in the range of 7–8 percentage points. Finally, we provide evidence on the potential pathways through which the unconditional cash transfer facilitates enrollment. Results show that households in the CGP spend more on education, and in particular on uniforms and shoes, two items cited as key barriers to school enrollment in

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

Compared to CCTs, the number of published studies on the impact of UCTs on schooling is low; this is only the second study that presents evidence from a rigorous evaluation of a government UCT from Africa. How does the African evidence based on UCTs compare to the evidence from Latin America on CCTs? The Kenya CT-OVC Study Team (2012) report an impact of 8 percentage points on secondary school-age children, similar to the 7–8 point impact we report here among lower secondary school-age children. These impacts compare quite favorably with the 7 point impact of *Progresa* on enrollment reported by Brauw & Hoddinott (2008), the 8-point impact reported by Schultz (2004) for the secondary school transition in *Progresa*, or the 5–7 percentage points impact reported by Attanasio et al. (2010) in the Colombian CCT *Familias en Accion*. For reasons mentioned earlier, this is not a strict apples to apples comparison. Nevertheless such a comparison establishes that unconditional cash transfer programs in Africa have effects on secondary school enrollment that are in the same range as what has been reported for CCTs in Latin America.

Appendix

Fig. A1.
Flowchart of study design

Table A1:

Household and individual level attrition

	<u>Households</u>		<u>Children 7–14 yrs</u>	
	N	Overall attrition (%)	N	Overall attrition (%)
Baseline	2519		3080	
2 year follow-up	2298	9	2532	18
3 year follow-up	2459	2	2689	13

	<u>Households</u>		<u>Children 7–14 yrs</u>	
	N	Overall attrition (%)	N	Overall attrition (%)
Total	7276		8301	

Overall attrition is calculated as the difference between the number of children covered at baseline (3080) and the number of children covered in each of the follow up (2532 and 2689 respectively) as a percentage of the number of children covered at baseline.

Table A2 -

Differential (selective) attrition for balanced and unbalanced panel

Key variables	Balanced panel			Robust clustered
	Mean at baseline		Difference in means	
	Treated	Control		
Age	10.056	9.932	0.124	(1.419)
Female	0.495	0.502	−0.00683	(−0.272)
Enrolled	0.745	0.737	0.00829	(0.306)
Involved in any paid or unpaid work	0.584	0.597	−0.0139	(−0.361)
Involved in paid work	0.023	0.022	0.000477	(0.0502)
Involved in unpaid work	0.551	0.565	−0.0146	(−0.367)
Age of recipient	35.232	35.060	0.172	(0.505)
Recipient never married	0.056	0.070	−0.0140	(−0.632)
Recipient divorced	0.081	0.129	−0.0478***	(−2.919)
Recipient widowed	0.077	0.080	−0.00264	(−0.126)
Recipient attended school	0.714	0.680	0.0335	(0.824)
Household size	7.375	7.054	0.320**	(2.171)
N° members age 0–5	1.991	1.944	0.0476	(0.753)
N° members age 6–12	2.259	2.168	0.0916	(1.098)
N° members age 13–18	1.013	0.920	0.0933*	(1.881)
N° members age 19–35	1.169	1.088	0.0805	(1.521)
N° members age 36–55	0.843	0.843	0.000204	(0.00445)
N° members age 56–69	0.075	0.069	0.00654	(0.371)
N° members age 70+	0.024	0.023	0.000482	(0.0448)
Shangombo district	0.320	0.334	−0.0145	(−0.308)
Kaputa district	0.251	0.254	−0.00248	(−0.0571)
Expenditure per capita	34.682	32.929	1.752	(0.824)

Key variables	<u>Unbalanced panel</u>			<i>Robust clustered</i> T-statistic
	Mean at baseline		Difference in means	
	<i>Treated</i>	<i>Control</i>		
Age	10.064	9.975	0.0890	(1.147)
Female	0.495	0.505	−0.0102	(−0.455)
Enrolled	0.744	0.740	0.00399	(0.149)
Involved in any paid or unpaid work	0.569	0.605	−0.0361	(−0.920)
Involved in paid work	0.026	0.020	0.00663	(0.791)
Involved in unpaid work	0.532	0.574	−0.0421	(−1.060)

Key variables	Unbalanced panel			Robust clustered T-statistic
	Mean at baseline		Difference in means	
	Treated	Control		
Age of recipient	35.281	35.118	0.163	(0.467)
Recipient never married	0.057	0.067	−0.0101	(−0.456)
Recipient divorced	0.081	0.130	−0.0483***	(−2.900)
Recipient widowed	0.081	0.073	0.00748	(0.439)
Recipient attended school	0.717	0.687	0.0298	(0.846)
Household size	7.376	7.079	0.297**	(2.115)
N° members age 0–5	1.983	1.956	0.0271	(0.489)
N° members age 6–12	2.241	2.178	0.0636	(0.826)
N° members age 13–18	1.011	0.902	0.110**	(2.343)
N° members age 19–35	1.197	1.120	0.0772	(1.497)
N° members age 36–55	0.842	0.827	0.0150	(0.350)
N° members age 56–69	0.075	0.072	0.00326	(0.203)
N° members age 70+	0.026	0.024	0.00156	(0.141)
Shangombo district	0.305	0.318	−0.0134	(−0.326)
Kaputa district	0.279	0.287	−0.00871	(−0.201)
Expenditure per capita	34.528	32.454	2.073	(0.996)

p<0.01,
**
p<0.05,
*
p<0.1 T-test is for difference in means between the two arms.

Note: Total N=2362; N for Treated= 1169; N for Control= 1193

p<0.01,
**
p<0.05,
*
p<0.1 T-test is for difference in means between the two arms.

Note: Total N=2859; N for Treated= 1425; N for Control= 1434

Table A3 -

Testing individual overall attrition (without IPW) for balanced and unbalanced panel

	<u>Balanced panel</u>			
	Mean at baseline			<i>Robust clustered</i>
Key variables	Full sample	Balanced Panel	Difference in means	t-statistic
Age	10.053	9.994	0.0596 ^{**}	(2.626)
Female	0.501	0.499	0.00242	(0.476)
Enrolled	0.742	0.741	0.00113	(0.266)
Involved in any paid or unpaid work	0.583	0.591	−0.00752	(−1.164)
Involved in paid work	0.023	0.022	0.000472	(0.290)
Involved in unpaid work	0.549	0.558	−0.00859	(−1.223)
Age of recipient	35.238	35.145	0.0922	(0.709)
Recipient never married	0.064	0.063	0.00163	(0.447)
Recipient divorced	0.104	0.105	−0.00185	(−0.509)
Recipient widowed	0.078	0.078	−0.000726	(−0.144)
Recipient attended school	0.708	0.697	0.0107	(1.458)

Key variables	Balanced panel		Robust clustered	
	Mean at baseline			
	<i>Full sample</i>	<i>Balanced Panel</i>	Difference in means	t-statistic
Household size	7.260	7.213	0.0471 *	(1.979)
N° members age 0–5	1.969	1.967	0.00143	(0.118)
N° members age 6–12	2.222	2.213	0.00912	(0.653)
N° members age 13–18	0.967	0.966	0.00118	(0.0861)
N° members age 19–35	1.176	1.128	0.0484 ***	(3.258)
N° members age 36–55	0.820	0.843	−0.0235 **	(−2.086)
N° members age 56–69	0.080	0.072	0.00790	(1.619)
N° members age 70+	0.026	0.024	0.00259	(1.052)
Shangombo district	0.302	0.327	−0.0253 ***	(−2.937)
Kaputa district	0.297	0.253	0.0447 ***	(5.492)
Expenditure per capita	33.528	33.795	−0.267	(−0.638)

Key variables	Unbalanced panel		Robust clustered	
	Mean at baseline			
	<i>Full sample</i>	<i>Balanced Panel</i>	Difference in means	t-statistic
Age	10.053	10.019	0.0340 ***	(3.158)
Female	0.501	0.500	0.00132	(0.566)
Enrolled	0.742	0.742	0.000235	(0.106)
Involved in any paid or unpaid work	0.583	0.587	−0.00365	(−1.561)
Involved in paid work	0.023	0.023	−0.000357	(−0.499)
Involved in unpaid work	0.549	0.553	−0.00387	(−1.582)
Age of recipient	35.238	35.199	0.0381	(0.621)
Recipient never married	0.064	0.062	0.00238	(0.999)
Recipient divorced	0.104	0.106	−0.00206	(−1.110)
Recipient widowed	0.078	0.077	0.000647	(0.305)
Recipient attended school	0.708	0.702	0.00618 *	(1.877)
Household size	7.260	7.227	0.0331 **	(2.053)
N° members age 0–5	1.969	1.970	−0.000739	(−0.138)
N° members age 6–12	2.222	2.210	0.0126	(1.272)
N° members age 13–18	0.967	0.956	0.0106	(1.363)
N° members age 19–35	1.176	1.158	0.0179 **	(2.039)
N° members age 36–55	0.820	0.835	−0.0148 ***	(−2.942)
N° members age 56–69	0.080	0.073	0.00642 *	(1.812)
N° members age 70+	0.026	0.025	0.00112	(0.773)
Shangombo district	0.302	0.311	−0.00935 **	(−2.555)
Kaputa district	0.297	0.283	0.0144 ***	(4.072)
Expenditure per capita	33.528	33.486	0.0421	(0.226)

p<0.01,

**

p<0.05,

* p<0.1

Note: Total N=5442; N for Treated= 3080; N for Control= 2362

p<0.01,
**
p<0.05,
*
p<0.1

Note: Total N=5939; N for Treated= 3080; N for Control= 2859

Table A4:

Mean Enrollment (Unbalanced panel)

Treatment status	Baseline	24-month	36-month
<u>7–14 years</u>			
<i>Control</i>	74.3	86.0	83.5
N	1536	1269	1355
<i>Treated</i>	74.2	87.5	87.1
N	1531	1261	1330
<u>7–10 years</u>			
<i>Control</i>	65.3	87.7	89.0
N	941	789	843
<i>Treated</i>	66.0	88.9	92.1
N	903	755	798
<u>11–14 years</u>			
<i>Control</i>	88.6	83.1	74.4
N	595	480	512
<i>Treated</i>	86.0	85.6	79.5
N	628	506	532

Table A5:

Means for any work (Unbalanced panel)

Treatment status	Baseline	24-month	36-month
<u>7–14 years</u>			
<i>Control</i>	60.3	91.3	97.3
N	1522	1253	1347
<i>Treated</i>	56.2	92.8	97.1
N	1512	1234	1321
<u>7–10 years</u>			
<i>Control</i>	52.7	89.2	96.4
N	932	779	841
<i>Treated</i>	49.3	91.6	96.0
N	893	740	798
<u>11–14 years</u>			
<i>Control</i>	72.3	94.7	98.6
N	590	474	506
<i>Treated</i>	66.3	94.7	98.8
N	619	494	523

Table A6:

Means for paid work (Unbalanced panel)

Treatment status	Baseline	24-month	36-month
<u>7–14 years</u>			
<i>Control</i>	<i>2.0</i>	<i>4.4</i>	<i>3.2</i>
N	1522	1253	1347
<i>Treated</i>	<i>2.6</i>	<i>2.3</i>	<i>1.6</i>
N	1512	1234	1321
<u>7–10 years</u>			
<i>Control</i>	<i>0.7</i>	<i>3.0</i>	<i>1.4</i>
N	932	779	841
<i>Treated</i>	<i>0.7</i>	<i>1.1</i>	<i>1.0</i>
N	893	740	798
<u>11–14 years</u>			
<i>Control</i>	<i>3.9</i>	<i>6.7</i>	<i>6.1</i>
N	590	474	506
<i>Treated</i>	<i>5.3</i>	<i>4.0</i>	<i>2.5</i>
N	619	494	523

Table A7

Impact of CGP on schooling and child labour (either paid or unpaid); jointly modelled.

VARIABLES	Without controls				With controls			
	Unbalanced panel		Panel		Unbalanced panel		Panel	
	(1)		(2)		(3)		(4)	
	Schooling	Any work	Schooling	Any work	Schooling	Any work	Schooling	Any work
Treated	−0.00286 (−0.11)	−0.0399 (−0.86)	0.00670 (0.23)	−0.0133 (−0.28)	−0.00862 (−0.35)	−0.0478 (−1.09)	−0.000753 (−0.03)	−0.0209 (−0.47)
DD 24-months	0.0178 (0.69)	0.0564 (1.18)	0.0161 (0.58)	0.0359 (0.73)	0.0197 (0.80)	0.0646 (1.35)	0.0212 (0.81)	0.0437 (0.88)
DD 36-months	0.0390 (1.47)	0.0386 (0.79)	0.0351 (1.21)	0.0119 (0.24)	<i>0.0457</i> (1.86)	0.0465 (0.95)	<i>0.0452</i> (1.69)	0.0197 (0.39)
Observations	8,178	8,178	6,990	6,990	8,105	8,105	6,934	6,934

Estimations use difference-in-difference modeling. Robust t-statistics clustered at the CWAC level are in parentheses. Bold and underlined indicates that they are significant at $p < 0.01$; bold indicates that they are significant at $p < 0.05$; italics indicate that they are significant at $p < 0.1$. Estimations with controls include: child's gender and age, household size, recipient's age, education and marital status, districts and household demographic composition.

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Figure 1.
Age distribution of children in the Child Grant Program

Figure 2.
School and work by age at baseline

Figure 3.
School and work by age and study arm at baseline

Figure 4.
School and work by age and study arm at 36-months

Table 1:

Timing of data collection in CGP evaluation

Baseline		Follow-ups	
Wave 1	Wave 2 (24-month)	Wave 3 (30-month)	Wave 4 (36-month)
Oct–Nov 2010	Oct–Nov 2012	June–July 2013	Oct–Nov 2013

Table 2:

Baseline characteristics of children 7–14 and test for baseline equivalence (N=3080)

Variable	All	Treated	Control	T-test for mean difference	Robust clustered t-statistic
Child's age in years	10.053	10.109	9.998	0.111	1.478
Female	0.501	0.497	0.506	−0.00882	−0.392
Child is enrolled in school	0.742	0.742	0.743	−0.00084	−0.0333
Child is involved in paid or unpaid work	0.583	0.563	0.603	−0.0403	−0.996
Child is involved in paid work	0.023	0.026	0.02	0.00608	0.773
Child is involved in unpaid work	0.549	0.527	0.572	−0.0445	−1.066
Recipient's age	35.238	35.273	35.202	0.0702	0.172
Recipient never married	0.064	0.057	0.071	−0.0141	−0.667
Recipient divorced	0.104	0.082	0.125	−0.0432***	−2.742
Recipient widow	0.078	0.08	0.075	0.00475	0.294
Recipient attended school	0.708	0.724	0.692	0.0319	0.94
Household size	7.26	7.373	7.148	0.225	1.569
Number of members age 0–5 years	1.969	1.975	1.963	0.0116	0.213
Number of members age 6–12 years	2.222	2.237	2.208	0.0291	0.36
Number of members age 13–18 years	0.967	1.019	0.915	0.104**	2.246
Number of members age 19–35 years	1.176	1.213	1.14	0.0725	1.285
Number of members age 36–55 years	0.82	0.827	0.813	0.0145	0.311
Number of members age 56–69 years	0.08	0.079	0.081	−0.00239	−0.120
Number of members age 70+ years	0.026	0.024	0.029	−0.00448	−0.416
Shangombo district	0.302	0.293	0.311	−0.0174	−0.459
Kaputa district	0.297	0.291	0.304	−0.0122	−0.281
Per capita consumption (new Kwacha)	33.528	34.602	32.461	2.141	1.004

Note: Total N=3080; N for Treated= 1538; N for Control= 1542. Recipient indicates official program beneficiary.

p<0.01,**
p<0.05,*
p<0.1

Table 3:

Schooling and Child Work among Children 7–14 Years

	Any work (either paid or unpaid)		Paid work	
	Not working	Working	Not working	Working
Not enrolled	13.7	11.9	25.1	0.5
Enrolled	28.0	46.4	72.6	1.8

Note: N=3080

Table 4:

Impact of CGP on enrollment among children age 7–14 at baseline

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	−0.000759 (−0.03)	0.00835 (0.29)	−0.00687 (−0.28)	0.00130 (0.05)
DD 24-months	0.0165 (0.64)	0.0143 (0.52)	0.0180 (0.73)	0.0180 (0.69)
DD 36-months	0.0368 (1.38)	0.0342 (1.16)	0.0440 (1.77)	0.0441 (1.62)
Observations	8,282	7,071	8,208	7,014
R-squared	0.022	0.025	0.123	0.133

Difference-in-differences linear probability estimates with robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates significance at $p < 0.05$. Estimates with controls include child's gender and age, household size, recipient's age, education and marital status, district dummy variables and household demographic composition.

Table 5:

Impact of CGP on child work (paid or unpaid) among children age 7–14 at baseline

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	−0.0404 (−0.87)	−0.0143 (−0.30)	−0.0478 (−1.09)	−0.0210 (−0.47)
DD 24-months	0.0561 (1.18)	0.0361 (0.73)	0.0638 (1.33)	0.0431 (0.87)
DD 36-months	0.0391 (0.80)	0.0128 (0.26)	0.0465 (0.95)	0.0198 (0.40)
Observations	8,189	6,999	8,116	6,943
R-squared	0.207	0.200	0.251	0.243

See notes to Table 4.

Table 6.

Impact of CGP on enrolment among children age 7–10 year at baseline.

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	0.00775 (0.21)	0.00595 (0.15)	0.00874 (0.25)	0.00675 (0.19)
DD 24-months	0.00384 (0.11)	0.0145 (0.39)	−0.00233 (−0.07)	0.00911 (0.25)
DD 36-months	0.0239 (0.66)	0.0319 (0.83)	0.0205 (0.58)	0.0288 (0.79)
Observations	5029	4338	4964	4290
R^2	0.085	0.094	0.167	0.185

See notes to Table 4 for explanation.

Table 7.

Impact of CGP on any work among children age 7–10 years at baseline.

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	−0.0342 (−0.71)	−0.0162 (−0.33)	−0.0372 (−0.81)	−0.0172 (−0.37)
DD 24-months	0.0583 (1.19)	0.0461 (0.91)	0.0645 (1.31)	0.0508 (1.00)
DD 36-months	0.0297 (0.58)	0.00969 (0.19)	0.0350 (0.68)	0.0121 (0.23)
Observations	4983	4300	4919	4253
R^2	0.246	0.232	0.279	0.270

See notes to Table 4 for explanation.

Table 8:

Impact of CGP on enrollment among children age 7–10 year at baseline

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	0.00775 (0.21)	0.00595 (0.15)	0.00874 (0.25)	0.00675 (0.19)
DD 24-months	0.00384 (0.11)	0.0145 (0.39)	−0.00233 (−0.07)	0.00911 (0.25)
DD 36-months	0.0239 (0.66)	0.0319 (0.83)	0.0205 (0.58)	0.0288 (0.79)
Observations	5,029	4,338	4,964	4,290
R-squared	0.085	0.094	0.167	0.185

See notes to Table 4 for explanation.

Table 9:

Impact of CGP on any work among children age 7–10 years at baseline

VARIABLES	Without controls		With controls	
	(1) Unbalanced panel	(2) Panel	(3) Unbalanced panel	(4) Panel
Treated	−0.0342 (−0.71)	−0.0162 (−0.33)	−0.0372 (−0.81)	−0.0172 (−0.37)
DD 24-months	0.0583 (1.19)	0.0461 (0.91)	0.0645 (1.31)	0.0508 (1.00)
DD 36-months	0.0297 (0.58)	0.00969 (0.19)	0.0350 (0.68)	0.0121 (0.23)
Observations	4,983	4,300	4,919	4,253
R-squared	0.246	0.232	0.279	0.270

See notes to Table 4.

Table 10:

Impact of CGP on enrollment among children age 11–14 at baseline

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	−0.0261 (−1.02)	−0.00427 (−0.18)	−0.0310 (−1.30)	−0.00826 (−0.37)
DD 24-months	0.0507 (1.76)	0.0329 (1.24)	0.0512 (1.79)	0.0344 (1.30)
DD 36-months	0.0769 (2.40)	0.0628 (1.90)	0.0806 (2.51)	0.0688 (2.09)
Observations	3,253	2,733	3,244	2,724
R-squared	0.016	0.018	0.072	0.074

See notes to Table 4.

Table 11:

Impact of CGP on any work among children age 11–14 years at baseline

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	−0.0599 (−1.16)	−0.0223 (−0.40)	−0.0623 (−1.28)	−0.0245 (−0.47)
DD 24-months	0.0599 (1.09)	0.0287 (0.48)	0.0648 (1.18)	0.0316 (0.53)
DD 36-months	0.0623 (1.18)	0.0274 (0.48)	0.0631 (1.19)	0.0297 (0.52)
Observations	3,206	2,699	3,197	2,690
R-squared	0.156	0.154	0.179	0.176

See notes to Table 4.

Table 12:

Impact of CGP on paid work among children 11–14 years at baseline

VARIABLES	Without controls		With controls	
	(1)	(2)	(3)	(4)
	Unbalanced panel	Panel	Unbalanced panel	Panel
Treated	0.0141 (0.89)	0.00228 (0.12)	0.0141 (0.91)	0.00239 (0.13)
DD 24-months	<i>−0.0413</i> (−1.93)	−0.0240 (−1.08)	<i>−0.0397</i> (−1.88)	−0.0233 (−1.06)
DD 36-months	−0.0502 (−2.14)	−0.0437 (−1.74)	−0.0502 (−2.11)	−0.0433 (−1.70)
Observations	3,206	2,699	3,197	2,690
R-squared	0.004	0.004	0.020	0.022

See notes to Table 4.

Table 13:

Impact of CGP on education and uniform expenditures (log) by age group

VARIABLES	Total education expenditures		Uniform expenditures	
	(1)	(2)	(3)	(4)
	Age group 7–14	Age group 11–14	Age group 7–14	Age group 11–14
Treated	−0.190 (−1.29)	−0.178 (−1.07)	−0.151 (−1.28)	−0.127 (−0.89)
DD 24-months	0.476 (2.96)	<i>0.372</i> (1.79)	0.750 (5.14)	0.698 (3.28)
DD 36-months	<i>0.289</i> (1.83)	0.209 (0.91)	0.377 (2.06)	0.312 (1.23)
Age	−0.504 (−7.01)	−0.962 (−3.60)	−0.425 (−5.58)	−0.688 (−3.38)
Age squared	0.0251 (8.03)	0.0393 (4.29)	0.0193 (5.90)	0.0263 (3.51)
Female	−0.0150 (−0.40)	−0.0601 (−0.86)	0.00930 (0.26)	0.0221 (0.28)
Observations	6,615	1,914	6,675	1,935
R-squared	0.163	0.210	0.091	0.095

The dependent variables are schooling and uniform expenditures made by the household on each child, reported only for those children currently in school. Schooling and uniform expenditures are in log form. Estimations use difference-in-difference modeling. Robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates significance at $p < 0.05$. Estimations controls for basic demographic variables including: child's gender and age, household size, recipient's age, education and marital status, districts and household demographic composition. Estimations without controls are consistent with these results.

Table 14:

Impact of CGP on shoes ownership

VARIABLES	Age group 7–14		Age group 11–14	
	(1) All school age children	(2) School age children enrolled	(3) All school age children	(4) School age children enrolled
Treated	–0.000498 (–0.02)	–0.00869 (–0.28)	–0.0302 (–0.77)	–0.0395 (–1.10)
DD 24-months	0.296 (4.98)	0.311 (5.13)	0.247 (3.36)	0.263 (3.65)
DD 36-months	0.235 (5.03)	0.224 (4.62)	0.222 (3.74)	0.201 (3.39)
Age	–0.0470 (–3.22)	–0.0589 (–3.09)	0.0144 (0.22)	0.00790 (0.11)
Age squared	0.00264 (4.19)	0.00310 (3.73)	0.000115 (0.05)	0.000391 (0.15)
Female	–0.00176 (–0.16)	–0.00926 (–0.72)	0.00318 (0.15)	–0.000359 (–0.02)
Observations	8,140	6,652	2,354	1,915
R-squared	0.265	0.259	0.269	0.285

The dependent variable is shoes ownership (dummy 0/1): for each child age 5 to 18, the respondent reports whether the child has a pair of shoes. This question is asked whether or not the child is currently enrolled in school. Estimations use difference-in-difference modeling. Robust t-statistics clustered at the CWAC level are in parentheses. Bold indicates significance at $p < 0.05$. Estimations controls for basic demographic variables including: child's gender and age, household size, recipient's age, education and marital status, districts and household demographic composition. Estimations without controls are consistent with these results.