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Wealth differentials in the impact of conditional and unconditional cash transfers on education: findings from a community-randomised controlled trial in Zimbabwe

Rory Fenton[a,b], Constance Nyamukapa[b,c], Simon Gregson[b,c], Laura Robertson[b], Phyllis Mushati[c], Ranjeeta Thomas[b] and Jeffrey W. Eaton[b]

[a]Agricultural Learning and Impacts Network (ALINe), Firetail Ltd., London, UK; [b]Department of Infectious Disease Epidemiology, School of Public Health, Imperial College London, London, UK; [c]Manicaland Centre for Public Health Research, Biomedical Research and Training Institute, Harare, Zimbabwe

ABSTRACT

We investigated (1) how household wealth affected the relationship between conditional cash transfers (CCT) and unconditional cash transfers (UCT) and school attendance, (2) whether CCT and UCT affected educational outcomes (repeating a year of school), (3) if baseline school attendance and transfer conditions affected how much of the transfers participants spent on education and (4) if CCT or UCT reduced child labour in recipient households. Data were analysed from a cluster-randomized controlled trial of CCT and UCT in 4043 households from 2009 to 2010. Recipient households received $18 per month plus $4 per child. CCT were conditioned on above 80% school attendance, a full vaccination record and a birth certificate. In the poorest quintile, the odds ratio of above 80% school attendance at follow-up for those with below 80% school attendance at baseline was 1.06 ($p = .67$) for UCT vs. CCT. UCT recipients reported spending slightly more (46.1% (45.4–46.7)) of the transfer on school expenses than did CCT recipients (44.8% (44.1–45.5)). Amongst those with baseline school attendance of below 80%, there was no statistically significant difference between CCT and UCT participants in the proportion of the transfer spent on school expenses ($p = .63$). Amongst those with above 80% baseline school attendance, CCT participants spent 3.5% less ($p = .001$) on school expenses than UCT participants. UCT participants were no less likely than those in the control group to repeat a grade of school. CCT participants had .69 (.60–.79) lower odds vs. control of repeating the previous school grade. Children in CCT recipient households spent an average of .31 fewer hours in paid work than those in the control group ($p < .001$) and children in the UCT arm spent an average of .15 fewer hours in paid work each week than those in the control arm ($p = .06$).

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CONTACT Rory Fenton rorytfenton@gmail.com

Introduction

Cash transfers (CT) have grown in popularity over the past 15 years as anti-poverty tools in developing countries. In particular, their use in boosting school attendance has been promoted in Latin America and, increasingly, in sub-Saharan Africa. There is strong evidence from Latin America that CT can increase school attendance. One of the developing world's largest conditional cash transfer programmes (CCT), Nicaragua's Red de Protección Social, increased school enrolment by 13 percentage points (Maluccio et al., 2005). A similar conditional programme in Honduras, PRAF, saw a more modest attendance increase of 3% points, from a higher baseline level (Glewwe et al., 2004). Unconditional cash transfers (UCT) are also becoming more popular and have achieved similarly positive developmental impacts. A study of a UCT in Zambia found school attendance increased by 3% points and the percentage of hungry households dropped from 56 to 35% (MCDSS/GTZ, 2006).

While CT have been demonstrated to boost school attendance amongst recipients, it is important to understand how these benefits are distributed between recipients and whether wealth inequality affects this. There is some evidence for an impact of CT in reducing wider inequality in society. The first two years of Mexico's Progresa, targeted at low-income households, saw the number of people in poverty decline nationally by 17% and the poverty gap reduced by 36% (Skoufias et al., 2005). A study attributed 21% of the drop in income inequality between the mid-1990's and mid-2000's in Brazil and Mexico, and 15% of that drop in Chile, to the impact of a CCT (Soares et al., 2007). However, evidence is lacking about how inequality between recipients affects the impact of the transfer on children's education and broader wellbeing and whether this impact varies between UCT and CCT.

There are competing theories (Hanlon et al., 2010) about whether the poorest recipients of CT will benefit more or less than the wealthiest recipients. If structural issues such as racial or sectarian discrimination underpin the poorest group's low school attendance, CT will do little to address these underlying issues, thereby not benefitting these groups. Poorer, less-educated households may also struggle with the paperwork involved in proving they have met conditions. Wealthier households may be closer to meeting conditions to begin with, making it less challenging for them to comply. Conversely, the poorest households are likely to have more "room for improvement", resulting in greater relative gains, and CT will account for a larger proportion of a poorer household's income, increasing the power of conditions as an incentive.

Evidence is also sparse about whether CCT and UCT recipients spend their transfers differently. It might be expected that CCT recipients will spend more of the transfer on desired outcomes like education, as they are incentivised to do so by the conditions. This suggests a market failure whereby households fail to value education as highly as they should, which the incentive of the CCT can then correct, encouraging higher spending on education than UCT do (Baird et al., 2013). Conversely, recipients may have previously spent little on education because they simply didn't have enough money, in which case one would expect similar spending patterns in CCT and UCT recipient households.

Finally, whether the improvements in attendance incentivised by CT programmes actually result in better education outcomes remains an open question. Brazil's Bolsa Familia, a CT conditional on school attendance, indeed increased attendance but did not increase children progressing to higher grades at the end of the school year (Campello et al., 2014). This is consistent with other programmes that have been successful in increasing school

attendance (e.g. deworming schoolchildren (Miguel et al., 2004) and providing additional teachers (Banerjee et al., 2007)) but have also failed to have an impact on learning outcomes. These studies did not compare the effects of concurrent CCT and UCT programmes on education outcomes.

One barrier to school attendance that has been cited is the need for children to provide labour, which CT might be expected to reduce by providing an alternative source of household income. There is evidence from a school fees reduction programme that children replace labour with school, although there have not been any previous studies on the effects of CT on child labour (Ravallion et al., 2000). CCT may be especially effective at reducing child labour if they encourage children to replace labour with school. However, they could be less effective if conditions are harder for the poorest households (where child labour may be most common) to meet.

From 2009 to 2010, a community-randomized controlled trial (C-RCT) was conducted in an HIV-endemic region of Zimbabwe to investigate the comparative effects of CCT and UCT programmes on school attendance, among other outcomes. The results showed that, after one year, the proportion of children with over 80% school attendance at the end of the trial was 7.2% (.8–13.7) higher in the UCT and 7.6% (1.2–14.1) higher in the CCT groups than in the control arm (Robertson et al., 2013). The present analysis uses data from the same C-RCT in Zimbabwe to investigate whether there were differences between the benefits of CCTs and UCTs: (1) in improving school attendance amongst children from the poorest households; (2) in the proportions of CTs spent on school education by the poorest households; (3) in improving educational quality; and (4) in reducing child labour.

Methods

Setting and study design

The data in this study are taken from the baseline and follow-up surveys conducted in 2009 and 2010 in a C-RCT comparing CCT and UCT in Manicaland, an area in the east of Zimbabwe (Robertson et al., 2013). This study took place in the same communities as a larger cohort study, on-going since 1998, which investigates HIV in the area (Gregson et al., 2006). Between 2006 and 2008, HIV prevalence in Manicaland was 17% among 15–49 year olds, with some 21% of children in the area having lost one or both parents (Robertson et al., 2008).

With so many orphaned and vulnerable children (OVC) in Manicaland, a CT programme was proposed to address their welfare, targeted at households containing OVC. In July 2009, all households were enumerated in ten geographically distinct sites (of the 12 in the Manicaland cohort study (Robertson et al., 2013); on average, around 1200 households per site). After baseline enumeration, each site was divided into three similarly sized matched clusters, and each of these 3 clusters was randomly assigned to one of the CCT, UCT and control arms.

Households were eligible to participate in the CT programme if they (i) contained children, (ii) were not in the highest wealth quintile, and (iii) satisfied at least one of the following conditions: the head of the household was a child; the household cared for at least one orphan (a child with at least one deceased parent), a disabled person, or an individual who was chronically ill; or the household was in the poorest wealth quintile. Wealth quintiles

were created by surveying household assets and using a simple summed score (Lopman et al., 2007; Robertson et al., 2012). Local communities were asked to split households in their areas into quintiles ranked from poorest to least poor (Robertson et al., 2014). To be considered as being in the poorest quintile for the CT intervention, a household had to have been deemed to be so by both the wealth index and community ranking. 1199 households were assigned to the control group, 1525 to the UCT arm and 1319 to the CCT arm.

Assignment of clusters to trial arms was determined by public drawing of lots. Eligible households in the CCT and UCT arms were to each receive $18 every 2 months plus $4 per child in the household, up to 3 children. The conditions applied to the CCT recipients were: children had to have 80% school attendance for the previous 2 months, have a full vaccination record, and have at least applied for a birth certificate, and parents had to attend parenting classes. When collecting cash, CCT participants were required to show relevant documents to prove they had met these conditions. After 6 months of not meeting the conditions, CCT households lost 10% of the transfer and eventually lost control of how the transfer was spent (Robertson et al., 2013). Participating households in all 3 arms (including control) were also given a small amount of maize seed and fertiliser.

The first transfers were delivered in January 2010 and the last in January 2011. A follow-up survey was conducted in March 2011, a repeat of the baseline survey with additional questions about whether the child had repeated the previous year of school and how much of the transfer had been spent on education. The cash transfer programme was funded by the Programme of Support for the Zimbabwe National Action Plan for Orphans and Vulnerable Children (administered by UNICEF Zimbabwe) and implemented by the Diocese of Mutare Community Care Programme (a local NGO). The research was funded by the Wellcome Trust [084401/Z/07/B].

Hypotheses and statistical methods

Using baseline and follow-up data, we set out to test the following hypotheses

(1) School attendance is increased less in the poorest quintile of households receiving CCTs than in the poorest quintile of households receiving UCTs. This investigates whether attaching conditions to CTs unduly penalizes the poorest recipients.

(2) (a) CCT recipients spend more of the transfers on school expenses than UCT recipients. This investigates whether conditions successfully incentivise spending of the CTs on school expenses. (b) CCT recipients with low baseline school attendance spend more of the transfers on school expenses than UCT recipients with low baseline attendance. This investigates whether conditions incentivize greater spending of CTs on school expenses amongst those who need it the most.

(3) (a) Children in households receiving CTs are less likely to have repeated the previous year of school than children in households in the control arm. This investigates whether improvements in attendance from CTs commensurately affect education outcomes, using grade repetition as a proxy measure. (b) Children in households receiving CTs with conditions are less likely at follow-up to have repeated the previous year of school than those in UCT households. This investigates whether conditions enhance the impact of CTs in improving educational outcomes.

(4) (a) Children in households receiving CTs work fewer hours than those in control households. This investigates whether CTs reduce child labour. (b) Reductions in child labour are smaller in CCT households than in UCT households. This investigates whether spending a larger part of the CTs on school expenses in CCT households than in UCT households (Hypothesis 2) results in a greater continuing need for child labour in CCT households.

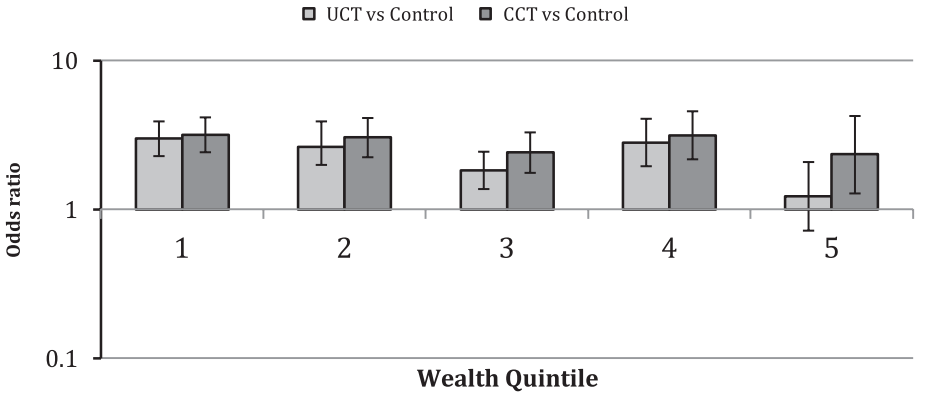

Figure 1. Odds of a child with below 80% attendance at baseline having above 80% attendance at follow-up by wealth quintile and form of CT. Wealth quintiles 1 and 5 indicate the poorest quintile and the least poor quintile, respectively.

As the size of the transfer varied by household size, we report the proportion of the total transfer spent on school expenses. At both baseline and follow-up, data on school attendance were collected. Low and high school attendance were defined as attending below and above 80% of school days in the past 2 months, respectively. To test Hypotheses 2(a) and (b), linear regression was used to assess the relationship between trial arm (CCT/UCT/control) and the proportion of a household's transfer spent on school fees. A linear regression relating trial arm and hours of paid work was used to test Hypotheses 4(a) and (b). Logistic regression models were used to test Hypotheses 1 and 3. The odds of a child with low school attendance at baseline having high school attendance at follow-up were used as the dependent variable for Hypothesis 1, with arm used as the independent variable and the model run for each wealth quintile (from the baseline household wealth index). The odds of having repeated the previous year of school at follow-up as the dependent variable to test Hypotheses 3(a) and (b), with arm used as the dependent variable. All models were run in Stata 12.

Results

Effects of CCT and UCT on school attendance in the poorest households

In the poorest wealth quintile, children in households receiving both CCT and UCT had significantly greater odds of changing from poor attendance to high attendance than those in the control group (Figure 1). The difference between CCT and UCT was not statistically significant, with an odds ratio of 1.06 (p = .67) Similar results were found for the middle three quintiles. However, different effects of the transfer were seen between arms in the least poor quintile. CCT recipients in this quintile had a similar increase in attendance to other quintiles while those in the same quintile in the UCT arm saw no significant change in attendance compared to the control arm. The difference in school attendance between UCT and CCT in the least poor quintile was statistically significant (p = .03).

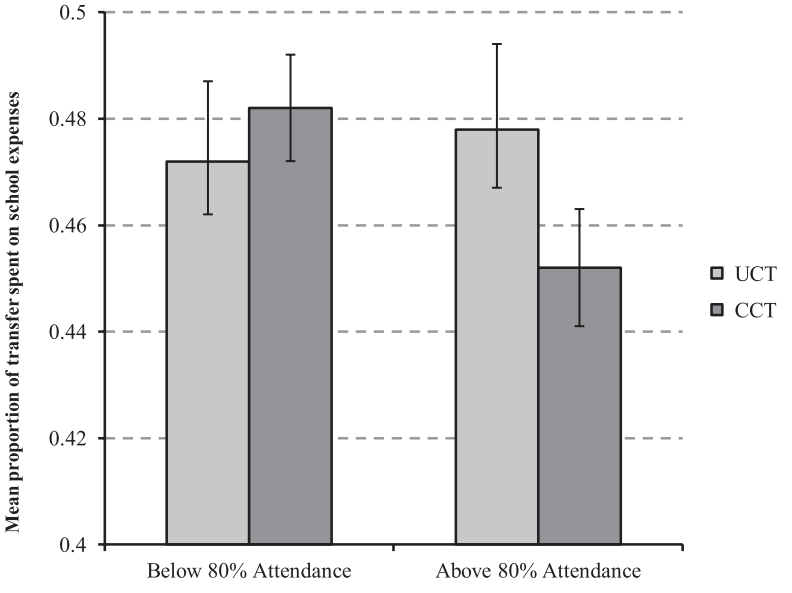

Figure 2. Mean proportion of CT spent by UCT and CCT recipient households on school expenses, for those with high and low baseline school attendance.

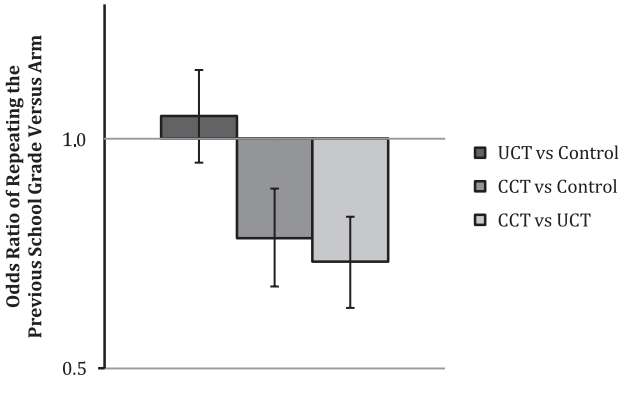

Figure 3. Odds ratio of a child having repeated the previous year of school at follow-up comparing CCT, UCT and control arms.

Patterns of spending of CTs in the CCT and UCT arms

CCT recipients reported spending a mean of 44.8% (95% CI: 44.1–45.5%) of the transfer on school expenses whilst UCT recipients spent 46.8% (45.4–46.7%), a *smaller* proportion of the transfer in the CCT arm ($p = .008$). Investigating Hypothesis 2(b), there was no significant difference between CCT and UCT arms for school spending amongst those with below 80% attendance at baseline. Amongst those with above 80% baseline attendance, CCT recipients spent 4.4% ($p = .03$) less on school expenses than those in the unconditional arm (Figure 2). This suggests that the conditions dis-incentivised spending of the CT on school expenses for those households that had already met the conditions.

Table 1. Mean hours worked per week at follow-up by children in each study arm.

Arm	Mean hours worked per week by children at follow-up
Control	.76
UCT	.61
CCT	.45

Educational outcomes in the CCT and UCT arms

No significant difference was observed in the odds of repeating a school grade at follow-up between the UCT and control arms (OR 1.07; .93–1.23, $p = .32$) (Figure 3). Participants receiving CCT had significantly lower odds of repeating the previous school grade than those in the control arm (OR .74; .64–.86, $p < .001$).

Reductions in child labour in the CCT and UCT arms

Children in both CT recipient arms spent fewer hours in paid employment than children in the control arm (see Table 1). Children in CCT recipient households spent an average of .31 fewer hours in paid work than those in the control group, a reduction of 41% ($p < .001$). Children in the UCT arm spent an average of .15 fewer hours in paid work each week than those in the control arm, a near-significant 18% difference ($p = .06$). The difference between the CCT and UCT arms is on the borderline of significance ($p = .055$).

Discussion

Contrary to our hypothesis, we found that conditions did not penalise the poorest recipients, with CCTs and UCTs having similar impacts amongst the poorest quintile. However, it is interesting to note that UCTs did not impact on the least poor recipients' school attendance, while CCT still did. An important and counter-intuitive finding is that CCT recipients actually spent less of the transfer on school expenses than UCT recipients, despite school attendance being an important condition for receiving the transfer. The results for Hypothesis 2(b) shed some light on this. These show that there was no difference in spending amongst those initially with low school attendance between CCT and UCT recipients, rather that the difference is explained by those in the CCT arm that already had high school attendance spending less of the transfer on school expenses than those in the UCT arm. A possible explanation is that the conditions made CCT recipients who were already meeting them feel complacent and so spend less on school expenses than they might otherwise have.

CTs were found to reduce child labour with CCTs being especially effective at this. The explanation that this greater reduction in labour is due to replacement by school attendance is insufficient as UCT and CCT households saw the same increase in school attendance. It is interesting that, while both the CCT and UCT interventions were shown by Robertson et al. 2013 to enhance school attendance, only CCT increased educational outcomes. It may be that, in conditioning transfer receipt on school attendance, children were made more aware of the importance of education and so paid better attention. This finding contrasts with results from Brazil's CCT Bolsa de Familia, which found no reduction in odds of repeating a school grade (Campello et al., 2014), perhaps due to contextual differences between sub-Saharan Africa and South America. A valuable further study would interview

schoolchildren in all arms to determine if the CCT recipients did indeed now pay better attention in school or if there was some other reason for improved rates of moving to higher school grades.

These findings add important detail and nuance to the existing literature on CT. An important caveat is the limited feasibility of comparing the CCT in this study with previously examined CCT from literature, as the nature and severity of conditions vary significantly between studies. The conditions used in the CCT arm were not very strongly enforced, with no family ever fully losing the transfer and conditions not at all enforced for the first 6 months of the 12-month program. Baird et al. (2013) have suggested that how strongly conditions are enforced plays a role in their effectiveness. It may be that more strictly enforced conditions would have better impacted on educational outcomes and yielded different results for the hypotheses tested in this paper. A further limitation is that data exist only for how much of the CT was spent on school expenses, which means we lack both data on total household expenditure and expenditure data from the control group. Baseline data for child labour repeating a year of school do not exist for this study, removing the ability to look at changes over time. Nonetheless, this paper provides novel insights into the educational impacts of CCT and UCT beyond average improved school attendance.

Disclosure statement

No potential conflict of interest was reported by the authors.

ORCID


Simon Gregson http://orcid.org/0000-0003-2707-0714

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