Pellerano, Luca; Porreca, Eleonora; Rosati, Furio C.

Working Paper

The Income Elasticity of Child Labour: Do Cash Transfers Have an Impact on the Poorest Children?

IZA Discussion Papers, No. 11983

Provided in Cooperation with:
IZA – Institute of Labor Economics

Suggested Citation: Pellerano, Luca; Porreca, Eleonora; Rosati, Furio C. (2018) : The Income Elasticity of Child Labour: Do Cash Transfers Have an Impact on the Poorest Children?, IZA Discussion Papers, No. 11983, Institute of Labor Economics (IZA), Bonn

This Version is available at:
https://hdl.handle.net/10419/193277

I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 11983

The Income Elasticity of Child Labour: Do Cash Transfers Have an Impact on the Poorest Children?

Luca Pellerano
Eleonora Porreca
Furio C. Rosati

NOVEMBER 2018

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 11983

The Income Elasticity of Child Labour: Do Cash Transfers Have an Impact on the Poorest Children?

Luca Pellerano
International Labour Organization and Institute for Fiscal Studies

Eleonora Porreca
Bank of Italy, DG Economics, Statistics and Research and Statistical Analysis Directorate

Furio C. Rosati
University of Rome Tor Vergata and IZA

NOVEMBER 2018

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Income Elasticity of Child Labour: Do Cash Transfers Have an Impact on the Poorest Children?

The possible non linearity of the income elasticity of child labour has been at the centre of the debate regarding both its causes and the policy instruments to address it. We contribute to this debate providing theoretical and empirical novel results. From a theoretical point of view, for any given transfer size, there is a critical level of household income below which an increase in income has no impact on child labour and education. We estimate the causal impact of an increase in income on child labour and education exploiting the random allocation of the Child Grant Programme, an unconditional cash transfer, in Lesotho. We show that the poorest households do not increase investment in children's human capital, while relatively less poor households reduce child labour and increase education. In policy terms, the results indicate that cash transfers might not be always effective to support the investment in children's human capital of the poorest households. Beside the integration with other measures, making the amount of transfer depends of the level of deprivation of the household might improve cash transfer effectiveness.

JEL Classification: H, C93, I28, J1, J24

Keywords: child labour, education, cash transfer, randomized experiment, Lesotho

Corresponding author:
Furio C. Rosati
Department of Economics and Finance
University of Rome Tor Vergata
via Columbia n. 2
00133 Roma
Italy

E-mail: f.rosati@economia.uniroma2.it

1. Introduction

According to the International Labour Organization (ILO 2017), 152 million children aged 5-17 were involved in child labour in 2016, about 10% of the children in this group. Most of working children live in low and middle-income countries. Sub-Saharan Africa shows the highest incidence of child labour, with one in five children involved in it.

Living standards of the household, market imperfections, availability of schools and relative returns to education and work play a crucial role on the allocation of children's time[1].

A recurring debate on the causes of child labour concerns the role of poverty as one of its main determinants. Understanding whether household income affects child labour is essential for policy design and in particular for cash transfers (CTs), one of the main social protection instruments advocated (and implemented) also to combat child labour. Both targeting and transfer size are essential elements of CTs (conditional or not) and, of course, consideration of heterogeneity of response depending on the household income would be an important element for their efficient design.

Cross sectional and cross-countries studies, however, do not indicate the presence of a substantial child labour income elasticity. As pointed out in Edmonds (2005), this can be due to endogeneity problems, but also to the inherent non-linearity in the relationship between child labour and income. In fact, assuming that the income elasticity of leisure is close to zero for poor households, the Basu and Van (1998) subsistence hypothesis implies that an increase in income affects child labour supply so far as it allows households to raise above subsistence. The empirical evidence on the non-linearity of income elasticity is not very large. Edmonds (2005) shows that the effect of the income increase in Vietnam, between 1993 and 1998, was present mainly for the households that moved above the poverty line. A few impact evaluation

[1] For a review see Edmonds (2005) and Cigno – Rosati (2005).

studies, mainly focused on conditional cash transfers (CCTs) in Latin America, address the issue of heterogeneity by income. They tend to find a larger reduction of child labour and increase in school enrolment among the children belonging to the relatively poorer household (Galiani and McEwan 2013 and Glewwe and Olinto 2004 for the Honduran PRAF-II, Edmonds and Schady 2012 for the Bono de Desarollo Humano in Ecuador, Sparrow 2007 for the Jaringam Pengaman Social in Indonesia, Dammert 2009 for the Social Safety Net in Nicaragua).

In this study we provide novel evidence on the non-linear relationship between child labour and income. In a simple theoretical model presented here, built in the Basu and Van (1998) spirit, we show that the income elasticity of child labour is null for extremely poor households, it becomes negative (and decreasing with income) for relatively less poor households and finally becomes zero as households become more affluent.

We make use of an experiment relative to an unconditional cash transfer (UCT) in Lesotho and offer new evidence on the income elasticity of child labour and on the effectiveness of cash transfer for the poorest households. Our results indicate that, at least in poor rural communities, UCT programs lead to an impact that is consistent with the theoretical predictions outlined and contradict some of earlier findings: extremely poor households do not change children's time allocation, while relatively less poor households reduce child labour and increase school attendance.

We estimate the impact of Phase 1-Round 2 of the Child Grant Programme (CGP), randomly assigned to poor households in Lesotho from 2011 to 2013. Using survey data from an experimental evaluation, we find that it generates an increase in consumption expenditure for children (uniforms and shoes) in extreme poor households. In less poor households children reduce their participation in economic activities by 17 percentage points and work on average 3 hours less per week and almost 1 day less per week as a consequence of the CGP.

Our results contribute to the limited existing literature by offering causal evidence relative to the non-linearity of the relationship between child labour and income and by extending the evidence on the heterogeneous effects of cash transfers. In particular we show that the effectiveness of cash transfers does not necessarily increases with the level of deprivation of the household. Moreover, to the best of our knowledge, this is the first attempt to analyse heterogeneous impacts by income on child labour of UCT in Sub-Saharan Africa. The literature on heterogeneous effects is mainly focused on conditional transfers in middle-income countries characterized by higher urbanization and higher child employment in paid activities outside the household. Instead, the CGP is an unconditional transfer and has been implemented in rural areas of Lesotho, where children are mainly involved in farming and livestock activities inside the household. These structural differences may also contribute to explaining results obtained in our study.

From a policy point of view, our results confirm that targeting is very important for insuring the effectiveness of CTs. However, in order to obtain the desirable effects in terms of human capital, transfers should be large enough to modify household behaviour in the desired direction. Some forms, even simplified, of means testing would be necessary to improve the efficiency of the transfer without increasing substantially the overall budget. Our findings also point to the need to give more attention to the size of the transfer in assessing their impact. A somehow obvious point but often neglected in the literature[2].

Finally, in this paper we test for the presence of spill-over effects, by assessing the impact of the CGP on non-eligible children. Results indicate the absence of spill-over effects, as neither child labour nor child education in non-eligible households were influenced by the programme.

The paper is organized as follows. The next section presents the theoretical framework. Section 3 presents the program, the experimental design and the implementation of the CGP.

[2] See De Hoop and Rosati (2014).

The data, the descriptive statistics and the balance analysis are discussed in Section 4. Section 5 illustrates the estimation approach and section 6 presents the results and robustness checks. Concluding remarks are presented in section 7.

2. Theoretical framework

We develop a simple overlapping generation model in the spirit of Basu and Van (1998), but without assuming exogenous level of subsistence consumption. In particular, we consider a two-periods overlapping generation model in which adult household members value current household consumption and children's future consumption. The latter is assumed to be a function of the investment in education in the first period. Adult labour supply is assumed to be inelastically fixed. Parents decide about children's time allocation between work and education[3].

To keep the exposition simple, we make several additional assumptions. Fertility is assumed to be exogenous. More importantly, we assume that households cannot save or borrow. To allow for savings will not alter the results, while in absence of credit constraints, decisions relative to consumption and investment in education would be separable[4] and the allocation of children's time between education and work would not depend on income, but only on relative returns.

As mentioned, the unitary household derives utility from current consumption, C_1, which includes parents' and children's consumption, and from children's future consumption, C_2. Children have 1 unit of time that can be allocated either to work, H, remunerated with a wage w, or to schooling, S. Beside its opportunity cost, education also has a direct cost of e. Children's future consumption is a concave function of the human capital accumulated through education, $g(S)$. Parents inelastically supply 1 time unit of work in period 1. Labour income

[3] For simplicity of exposition we do not consider that time can also be allocated to leisure. This assumption will not change our results in a substantial way and the implication of relaxing it will be discussed later.
[4] See, inter alia, Cigno – Rosati (2005).

plus any additional non-labour income constitute the resources available to the household in the first period, Y_1. The households' maximization problem can hence be written as follows:

$$Max\ U(C_1, C_2)$$

$$s.t. \quad C_1 = wH + Y_1 + \tau - eS \qquad (1)$$

$$C_2 = g(S) + \varepsilon$$

$$1 = H + S$$

Where $U(.)$ is a concave utility function with $U(.)' > 0$ and $U''(.) < 0$ and τ is an unconditional cash transfer. Expressing child labour supply in terms of schooling and allowing for corner solutions, the Lagrangian function, the FOC and the complementary slackness conditions are:

$$L = U\big(w(1-S) + Y_1 + \tau - eS, g(S)\big) + \lambda_1(1-S) + \lambda_2(S)$$

$$\frac{\partial U}{\partial S} = -wU'_{C_1} - eU'_{C_1} + g'\ U'_{C_2} - \lambda_1 + \lambda_2 = 0 \qquad (2)$$

$$\lambda_1(1-S) = 0$$

$$\lambda_2 S = 0$$

The maximization problem has three possible solutions: one interior solution and two corner solutions. Taking as given all the other parameters of the model, it is easy to see that the solution depends on the level of Y_1. There is a level of Y_1, Y^*, such that for $Y_1 < Y^*$, we have $\lambda_2 > 0$, S=0, $g'\ U'_{C_2} < U'_{C_1}(e+w)$ and $\frac{\partial S}{\partial \tau} = 0$. The level of resources of the household is so low, given the other parameters of the model, that the household allocates the time of their children only to work and a marginal change in income does not affect such allocation. For $Y^{**} > Y_1 > Y^*$, we have an interior solution and children's time is allocated according to:

$$\frac{U'_{C_1}}{U'_{C_2}} = \frac{g'}{e+w} \qquad (3)$$

with $H > 0,\ S > 0,\ \lambda_1 = 0,\ \lambda_2 = 0$. The amount of time dedicated to each activity is determined to equate the marginal rate of substitution between current and future consumption

to the relative price of future consumption. In this case it is easy to see that $\frac{\partial S}{\partial \tau} > 0$ and $\frac{\partial S}{\partial \tau \partial \tau} < 0$.

As Y_1 grows above Y^{**}, we have the other corner solution with $\lambda_1 > 0$, S=1, $g' U'_{C_2} > U'_{C_1}(e+w)$ and, obviously, $\frac{\partial S}{\partial \tau} = 0$. Households with relatively higher income, send their children only to school in order to transfer as many resources to the future as the time constraint allows.

In conclusion, this simple model indicates that very poor households do not send their child to school at all, moreover a marginal increase in current income does not change their behaviour (unless the increase is such that $Y_1 + d\tau > Y^*$). For relatively less poor households an increase in income reduces child labour and increases schooling, but at a decreasing rate up to a point where children completely stop working.

In other words, a UCT causes heterogeneous effects on work and education according to the level of income of the household, with null effects for very poor households and negative (for work) but decreasing effects for relatively less poor households. Following the introduction of a UCT we can, therefore, expect to observe children from very poor households (those with income below Y^*) to continue working, unless the transfer is such to bring them above the income threshold, in which case some of the children will begin to attend school without necessarily stop working. For children from less poor households (for those with $Y^* < Y_1 < Y^{**}$) we should observe an increase in school attendance and a reduction in work, with some children stopping work altogether if the transfer is such that Y_1 becomes greater than Y^{**}.

If transfers were conditional on school attendance, the results just discussed would be partially different. In what follows we present a heuristic discussion of such differences. A CCT offers a transfer τ conditional on a minimum investment in education S^*. For households with income $Y_1 > Y^*$, a CCT will have qualitatively the same impact of a UCT, unless S^* is greater than the optimal S the household would have chosen with a transfer τ. In this latter

case, the household might not find optimal to accept the transfer. If the household has an income $Y_1 < Y^*$, the effect will depend also on the amount of the transfer. If $\tau > w(1 - S^*) + e\,S^*$, i.e. if the transfer covers both the opportunity and the direct cost of sending a child to school, the household will accept the offer, send the child to school and reduce child labour, as $U(Y_1 + w(1 - S^*) + \tau - e\,S^*, g(S^*) + \varepsilon) > U(Y_1 + w, \varepsilon)$ i.e. as the lifetime utility sending the children to school for the required time and accepting the transfer is higher with respect to the baseline one.

On the other hand, if $\tau < w(1 - S^*) + e\,S^*$, the effect is ambiguous, as in this case $U(Y_1 + w(1 - S^*) + \tau - e\,S^*, g(S^*) + \varepsilon)$ can be higher or lower than $U(Y_1 + w, \varepsilon)$.

Therefore a CCT might reduce child labour and increase school attendance also for children belonging to households below subsistence if the transfer is large enough to cover direct and opportunity cost of education (but not necessarily large enough to move them above subsistence [5].

3. The Child Grant Programme

3.1. Background

Lesotho is one of the poorest countries in the world, with a GDP per capita at US$1,067 (in 2015), 60% of population living on less than US$1.90 a day (at international prices in 2011) and a GINI index equal to 54% in 2010[6]. Lesotho registered the third highest HIV rate in the world, with a prevalence of 22.7% in 2015. Poverty, food insecurity and HIV/AIDS are the main threats to development and care of children, increasing the number of orphans and vulnerable children.

During the last decade, several policies have been implemented in Lesotho to increase access to and quality of education (Education Acts), regulate children's rights (Children's

[5] For a discussion of the impact of a partial CCT subsidy see De Hoop et al. (2018)
[6] Most recent data available from The World Bank Development Indicators.

Protection and Welfare Act), protect and support vulnerable children (the National OVC Strategic Plan, the new National HIV and AIDS Strategic Plan, the National Strategy to Eliminate Mother-to-Child Transmission of HIV). The Child Grant Programme (CGP) was developed within this policy framework.

The CGP was introduced in 2009 by the Ministry of Social Development of the Government of Lesotho, funded by the European Commission with technical support from UNICEF-Lesotho. It consists of an unconditional cash transfer targeted to poor and vulnerable households with children. The main goal of the CGP "is to improve the living standards of Orphans and other Vulnerable Children (OVC) so as to reduce malnutrition, improve health status and increase school enrolment among OVCs"[7]. Even though the CGP is an unconditional transfer, it includes a form of "nudging" or "soft" conditionality. Beneficiaries received at each payment round a clear message that the transfer should be spent on the interest and needs of children. All recipients report having received instructions at the pay point to spend money on children, with a strong emphasis on education and school uniforms (Pellerano et al. 2014). [8]The first phase of the programme was planned in three rounds. Phase 1 - Round 2 was the object of an impact evaluation. The baseline survey for the impact evaluation was conducted in September 2011 and a follow-up survey took place in September 2013 in 48 Electoral Divisions (EDs) and 10 Community Councils (CCs) spread across 5 Districts[9], as illustrated in Figure 1. The communities covered by the CGP are exclusively in rural areas.

The impact evaluation was commissioned by the Government and UNICEF to Oxford Policy Management (Pellerano et al. 2012, Pellerano et al 2014). We make use of the impact evaluation data to assess the impact of the programme on child labour and education.

[7] Manual of operation in use for round 1A of the CGP pilot. November 2008.

[8] Pace et al. (2016) show that "soft conditionality" attached to the CGP plays a crucial role in increasing child related expenditure.

[9] Kanana and Tebe-Tebe Councils in Berea District; Litjojela and Malaoaneng Councils in Leribe District; Metsi-Maholo and Malakeng Councils in Mafateng District; Qiloane and Makheka Councils in Maseru District; Mosenekeng and White Hills councils in Qacha's Nek District.

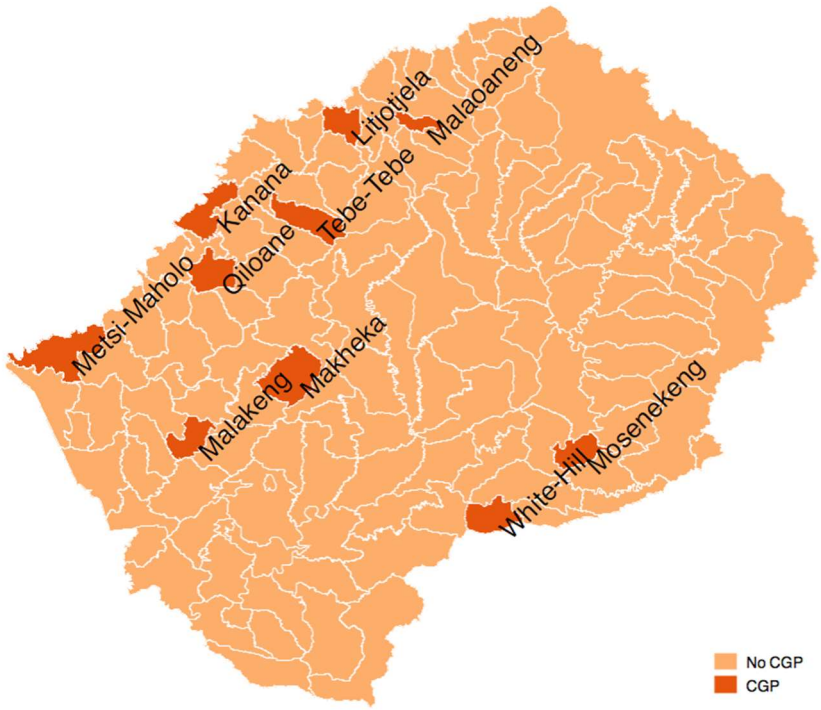

Figure 1: Community Councils covered by CGP in Phase 1 - Round 2

During the first year and half of Phase 1 - Round 2 (from September 2011 to March 2013), the transfer was set at M360 (US$ 50) every quarter for all beneficiaries, independently of the size of the household and of the number of children. From April 2013, the amount of the transfer has been indexed to the number of children (aged 0-17) in the household[10]. The new mechanism only marginally affected the design of the evaluation, since it was implemented only for the last payment before the follow-up survey.

An additional transfer, the Food Emergency Grant (FEG), was provided only to the beneficiary households of the CGP in treated EDs since the autumn of 2012. The FEG was intended to respond to the poor harvest during the 2010/2011 farming season, and provided the CGP beneficiaries with an additional bimonthly transfer of M400. Even if the FEG had a different primary goal from the CGP, namely to purchase seeds and other agricultural inputs, we

[10] M360 (US$ 50) to households with 1-2 children; M600 (US$ 84) to households with 3-4 children; M750 (US$ 105) to households with 5 and more children.

cannot separately ascribe changes in outcome variables to each of them and therefore our results reflect the impact of both transfers.

The monthly transfer of the CGP represents about 16% of monthly consumption of eligible households at baseline. The CGP monthly transfer combined with the FEG represents about 42% of monthly consumption expenditure of eligible households before the transfer.

3.2. Experimental design

Phase 1 - Round 2 of the programme was originally intended to provide cash transfer to all eligible households in the 10 selected Community Councils (CCs). However, as financial resources were not sufficient to cover the whole population in the CCs, it was decided to randomly choose beneficiary households among the eligible ones.

Since in the 10 CCs there were a total of 96 EDs, 48 EDs were randomly assigned to the treatment group and 48 EDs to the control group in public lottery events. After the randomization, all eligible households in treatment EDs received the first payment in September 2011. Eligibility criteria were based on a combination of Proxy Means Testing (PMT) and community validation. Socio-economic information on all households living in the 10 CCs was collected through a community census in early 2011 and used to set up the National Information System for Social Assistance (NISSA) and to obtain wealth indicators. According to this method, five categories of households were identified: ultra poor (NISSA 1), very poor (NISSA 2), poor (NISSA 3), less poor (NISSA 4) and better off (NISSA 5). In order to be eligible for the program households needed to have at least one child below 18 years old, to belong to the NISSA 1 or NISSA 2 groups, and also to be selected by the members of their community as being the ‘poorest of the poor’.

Eligible households were selected in both treatment and control EDs, but the transfer was provided only to households living in treatment EDs, thus leading to a valid control group.

The household surveys for the CGP impact evaluation were administered to a random sample of eligible households in treatment and control EDs. In order to assess possible spill-over effects of the programme, the surveys were administered also to a random sample of non-eligible households in treatment and control EDs. Information for all the four groups was collected before the implementation of the CGP (baseline in June/August 2011) and after the delivery (follow-up in June/August 2013).

The sample of the evaluation survey was drawn from the NISSA population[11], through a multi-stage stratified random cluster design[12]. The evaluation sample was drawn before and independently from the random assignment of the treatment, thus ruling out possible anticipation effects. The household survey covers several topics at household, adult and child level: demographic and socio-economic information, adult and child education and work, children's health and time use, household economic activities. A community evaluation survey was administered to a representative member of each village in combination with the household survey both at baseline and at follow-up. It provides information on community's services, local labour market, local prices and other community characteristics.

To make the sequence of the events clearer, Figure 2 presents the timing of the surveys and of the program implementation.

[11] The NISSA census covered 20,605 households, whereas, according to Ayala Consulting (2011), in the latest census provided by the Lesotho Bureau of Statistics, the expected population living in the 10 CCs was 30,603. Differences were attributed as related to different approaches to collection procedures and changes in administrative boundaries.

[12] In the first stage, among the 98 EDs (Primary Sampling Unit - PSU), 48 pairs of EDs were identified on the basis of similar characteristics in order to ensure balance in covariates between treated EDs and control EDs; in the second stage 40 pairs were randomly selected among the 48 to be covered by the evaluation survey; in stage three 2 villages (or clusters) (Secondary Sampling Unit - SSU) were drawn in each selected ED; in the fourth stage 20 households (10 eligible and 10 non-eligible) were randomly selected and surveyed in each cluster.

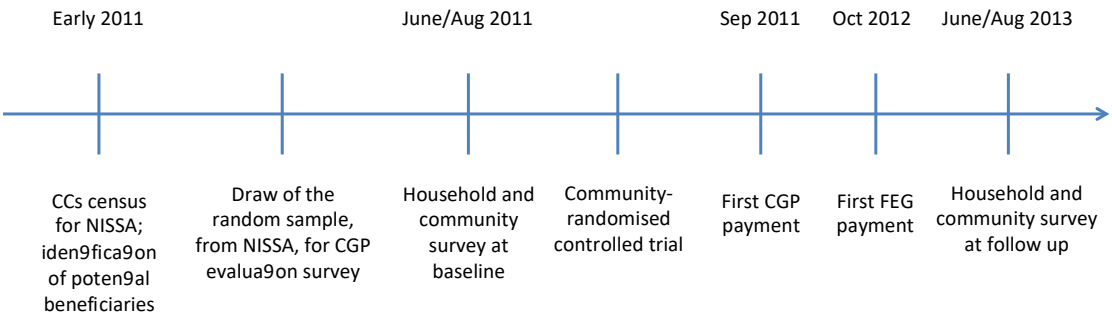

Figure 2: Timeline of CGP (Phase 1 - Round 2) evolution

3.3. Implementation of the CGP

Even if the CGP was designed to be provided on a regular base, payments have not always been on a quarterly basis, but they have been delayed at times. After the first payment in September 2011, when households received the equivalent of three monthly outlays (M1080), only three payments out of five were made on a regular quarterly basis. The intended number of quarterly payments should have been 10, whereas actual disbursements were between six and seven. However, beneficiary households received the total intended amount of the transfer (Pellerano et al. 2014). The relative unpredictability of the payment schedule could have affected household behaviour, but the experiment design does not allow testing for such effects.

Moreover, the conjunction of the CGP and the FEG might have confused households on how to properly allocate the two transfers. In fact, from the follow-up survey it emerges that only 20% of CGP beneficiary households were aware of having received the FEG. It is an open question the extent to which the impact of the program might have been affected by such irregularities in the payment and by the juxtaposition of the two transfers.

In the follow-up survey, a specific section was administered to the beneficiary households that received the transfer and to representative members of treated communities asking information on understanding and perceptions about the CGP, on who was responsible for

spending the transfer, on how it has been used and on the occurrence of problems with the payment mechanism.

In Table 1 we report what beneficiaries think about the program in terms of goals, selection criteria, payment mechanism and related problems. According to 64% of beneficiaries, the primary selection criterion is poverty, whereas 16% and 15% think respectively that it is based on the presence of children and orphans in the household. Despite the heterogeneity of considerations about the selection criteria, all households (99%) have recognized the primary goal of the programme, i.e. "the money is for the children". Decision on the spending of the transfer is, for 69% of households, undertaken by one person only: the household head in the majority of the cases. 55% of beneficiaries declare having spent the transfer for the children, in particular on food, education, shoes and clothing. While, 44% of beneficiaries have spent money primarily on food for the household.

Table 1: Beneficiaries' perceptions and experience with the CGP

	Percentage of household
Selection criteria for CGP	
Household poverty	64.32
Children in household	15.5
Orphans in household	14.77
Person responsible for CGP spending	
Household head	75.5
Other household member	20.9
Children	1.08
Primary use of last CGP payment	
Food for household	43.96
Food for children	17.84
Shoes and clothing for children	19.64
Education expenditures	17.12

Source: Authors' calculation from the follow-up survey

4. Data, descriptive statistics and balance analysis

Table 2 presents the characteristics of the data, including attrition rates[13] of eligible and non-eligible households. Due to resource constraints, the number of non-eligible households surveyed at follow-up was substantially reduced. Attrition rate among the eligible households is higher for control households (12%) than for treated households (5%). A test (available on request) shows that the non-response rate is not random between treatment and control group, as shown also by Pellerano et al. (2014).

We use sampling weights in order to make inference on the entire "study population", i.e. the NISSA population. Moreover, in order to address the potential attrition bias, sampling weights are multiplied by the inverse of the probability to remain in the sample at follow-up. Following Pellerano et al. (2014), sampling weights adjusted for attrition bias are constructed as follows: $w_{ij} = \left(\left(\frac{A_i}{m_i a_{ij}}\right)\left(\frac{N_{ijk}}{n_{ijk}}\right)p\right)$, where A_i is the total number of households in the sample frame of villages in the $i-th$ ED, m_i is the number of villages sampled in the $i-th$ ED, a_{ij} is the number of households interviewed in village j in the $i-th$ ED, N_{ijk} is the total number of households of type k in village j in the $i-th$ ED, n_{ijk} is the number of households interviewed of type k in village j in the $i-th$ ED and p is the inverse of the probability of a households to remain in the sample at follow-up. Sampling weights adjusted for attrition bias are used throughout the study.

[13] The number of households with completed interviews in both surveys is 2,151, of which 1,354 eligible and 797 non-eligible. The mismatch is due both to the loss of observations from baseline and to the addition of new households at follow-up. The new observations are a consequence of changes in demographic structure of households. Some children moved out from the original households at baseline and the new households where they live at follow up constitute new observations. The new households reported in Table 2 are not eligible for the programme and, for sake of the study, not included in the final sample. Whereas, for split households still eligible for the programme, the one with the higher probability of receiving the transfer is matched with the corresponding original household in the baseline (Pellerano et al. 2014). These households are grouped with all the other households not split.

Table 2: Sample and sample attrition

				Matched	Non-matched		
	Status	**Baseline**	**Follow-up**	**Final sample**	**Baseline only**	**Follow-up only**	**Attrition rate**
Eligible	**T**	747	732	706	41	26	5%
	C	739	674	648	91	26	12%
Total		1,486	1,406	1,354	132	52	9%
Non-eligible	**T**	779	401	396	383	5	49%
	C	789	405	401	388	4	49%
Total		1,568	806	797	771	4	49%
Total		3,054	2,212	2,151	903	61	30%

Source: Authors' calculation, baseline and follow-up survey

The main goal of this study is to estimate the impact of the CGP on children's work and education. Our final sample, therefore, is constituted of all matched children aged 6-15 at baseline and 8-17 at follow-up. We end up with a sample constituted of 2,928 children and 1,603 households, of which 2,098 eligible children and 1,107 eligible households.

The outcome variables considered refer to children's education and work. Work outcomes include both the intensive and the extensive margins for all economic activities (household business, farming/livestock and paid activities outside the household) and for farming/livestock activities only, the sector where most children are employed. The household questionnaire provides information on the participation into economic activities during the last 12 months prior the interview and on the number of hours and days worked during the last 7 days prior the interview. For education, we focus on school enrolment and on information about time devoted to study outside the school in a typical school day. Finally, we look at school expenditures for each child since the beginning of the academic year and to their disaggregation in school fees and expenditures for uniforms and shoes.

Table 3 presents the descriptive statistics and balance tests at baseline for the outcome variables (Panel A) and for the covariates (Panel B) used in the estimates. There are not systematic

differences between treatment and control groups, as we do not reject the null hypothesis of the t-test on the equality of means at 1% significance level. Treated and control households significantly differ only in some demographic characteristics: treated households have a somehow larger household size than control households.

Overall, 30% of eligible children worked in the 12 months preceding the survey. Almost the totality of working children was employed in farming and livestock activities within the household (98%).[14] Descriptive statistics by gender and age, not shown in the table, indicate higher participation rates for males (40%) than for females (20%) and for older children (43% of children aged 13-15) than for younger children (25% of children aged 6-12). Conditional on participation, hours and days worked per week are respectively 20 and 3. 92% of the children were enrolled in school at the time of the baseline survey and they spent about 45 minutes per day doing homework. Expenditure on school fees and on uniforms and shoes per child amounted to M35 and M3 respectively.

[14] Given the small sample size of children working in the household business and in paid activities outside the household, extensive and intensive margins of labour in these two groups are not reported in Table 3.

Table 3: Balance analysis on eligible children at baseline

Panel A: outcome variables	**Treatment**	**Control**	**Difference**
Child level			
Work (all activities)	0.32	0.29	0.03
Hours worked (all activities)	5.99	5.26	0.73
Days worked (all activities)	1.04	0.92	0.11
Farm work	0.34	0.33	0.01
Hours worked (farm activities)	6.78	6.04	0.74
Days worked (farm activities)	1.17	1.08	0.09
Enrolled in school	0.93	0.93	0.00
Homework/study outside school	45.23	45.53	-0.30
Total school expenditures	103.41	127.93	-24.52
School exp. on fees	30.46	40.68	-10.22
School exp. on uniforms/shoes	1.99	3.59	-1.6
Panel B: covariates	**Treatment**	**Control**	**Difference**
Child sex	0.49	0.50	-0.02
Child age	10.35	10.49	-0.14
Orphan child	0.13	0.13	0.00
Household size	6.06	5.70	0.37**
0-5 children present in the hh	0.56	0.52	0.04
Female hh head	0.51	0.55	-0.04
Age of hh head	52.39	52.81	-0.42
Highest edu among adults in the hh	7.73	7.49	0.24
Economic shock	0.62	0.58	0.04
Expenditure per capita	117.94	125.78	-7.84
Asset index	-0.45	-0.50	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%; sampling weights adjusted for attrition bias; SE clustered at ED

5. Estimation approach

Given the satisfactory results from the balance analysis presented in Table 3, we exploit the randomized treatment assignment to estimate the impact of the CGP on children's labour and education by comparing outcome variables of treatment and control children at follow up. We estimate the intention-to-treat (ITT), by considering all eligible households in treated and control EDs in the sample. The compliance rate is high, with 94% of eligible households in treated EDs actually receiving the transfer and very little spill-over[15]. Our estimates are, therefore, very close to the average treatment effect (ATE).

In particular we estimate with OLS the following equation:

$$Y_{iv} = \beta_0 + \beta_1 X_{iv} + \beta_2 T_v + \varepsilon_{iv} \quad (10)$$

where Y_{iv} indicates the outcome for child i in ED v at follow-up. We include a set of relevant observable characteristics at child and household level at baseline, X_{iv}, to increase precision of the estimates. The impact of the transfer is given by the coefficient β_2, relative to the dummy variable T_v equal to 1 for treatment EDs and equal to 0 for control EDs.

The set of control variables includes: sex and age of the child; a dummy equal to 1 if child is orphan (0 otherwise); household size; a dummy equal to 1 if children aged 0-5 are present in the household (0 otherwise); gender and age of the household head; the highest level of education reached by any adult member of the household; a dummy equal to 1 if the household has been affected by a serious economic shock during the 12 months prior the interview (0 otherwise); fixed effects for the 10 Community Councils (CCs). Robust and clustered standard errors at ED level and sampling weights adjusted for attrition bias are used throughout the analysis.

To analyse the heterogeneity of the impact of the transfer according to the level of resources of the household (as well as to other characteristics), we estimate the following OLS regression:

$$Y_{iv} = \beta_0 + \beta_1^{'} X_{iv} + \beta_2 T_v + \beta_3 C_{iv} + \beta_4 C_{iv} T_v + \varepsilon_{iv} \quad (11)$$

[15] A negligible number of eligible households in control EDs (1%) and non-eligible households in treated EDs (5%) received the treatment; no non-eligible households in control EDs have managed to receive the transfer.

where C_{iv} is the characteristic of interest. In particular, we interact the treatment variable, firstly, with four dummy variables for each quartiles of monthly expenditure per capita (at baseline) and, secondly, with demographic characteristics of children to assess heterogeneity by age and sex. Estimation results of equations (10) and (11) are shown in section 6.1.

As robustness check for heterogeneity by monthly expenditure per capita, we estimate equation (11) interacting the treatment variable with dummy variables of alternative measures of deprivation, such as the quartiles of the asset index, the mean and the median expenditures at baseline. Results of the robustness checks are discussed in section 6.2 and presented in the Appendix.

Finally, we test for the existence of spill-over effects comparing non-eligible children in treatment and control EDs. Estimation results are presented in section 6.3.

6. Results

6.1. Impact of the CGP

In Panel A of Table 4 we report results of the overall impact estimated through equation (10). The programme increased school enrolment, but had no effects on child labour. Participation in economic activities did not change, and there is only a marginally significant decrease in days and hours worked in agricultural activities within the household (column 5 and 6 of Table 4).

Treated children are 5 percentage points more likely to be enrolled in school and spend 6 minutes more doing homework on a typical school day. These impacts represent an increase of 4% in enrolment rate and of 13% in the time spent studying with respect to baseline values. Involvement in economic activities did not decrease: participation rates of children are not affected and the decrease in hours and days worked in farm and livestock activities is statistically significant at 10% level only. The transfer not only increased school attendance, but also expenditures on uniforms and shoes by M42, a substantial amount considering that this expenditure amounted to only M2 at baseline. Expenditures on fees do not appear to be affected by the transfer.

The results look rather different once we allow for heterogeneous effects by level of income. The transfer appears to affect children's time allocation only for the relatively less poor household. The reduction on the extensive and the intensive margin of child labour in agricultural household production is significant only for relatively less poor households (17 percentage points on participation, 3 hours and almost one day in the fourth quartile) and smaller and statistically insignificant in the first three quartiles. Also the increase in school enrolment, time spent on studying and school expenditure on fees is larger for children belonging to the highest quartile (14 percentage points on school enrolment, 9 minutes on studying and about M150 on expenditure on school fees) and statistically insignificant for children belonging to the first three quartiles. However, the lower the expenditure per capita the higher the increase in school expenditure on uniform and shoes, as shown in column (11). The magnitude and the statistically significance of the interaction term decreases moving from the first quartile to the fourth quartile.

Heterogeneous impacts by income are in line with the theoretical framework discussed in section 2. From the theoretical model and estimation results, it emerges that three factors play a crucial role on the impact of the CGP: the unconditionality of the transfer, the initial economic condition of beneficiary households and the transfer size. The unconditionality makes the transfer a pure income effect, which, depending on the initial economic condition of households and the transfer size, results in different changes in the outcome variables as discussed in Section 2. For extreme poor households the very low level of initial income and the transfer size are such that the marginal utility of current consumption remains higher than the marginal utility of education. Hence, the allocation of children's time does not change, but children's consumption (of uniforms and shoes) increases. By contrast, for less poor households the sufficiently "high" initial level of income and the transfer size are such that the marginal utility of education increases and becomes higher than the marginal utility of current consumption. Time allocation of children tends to changes into more education and less work.

Table 4: Impact of the CGP - OLS

	(1) Work	(2) Hours	(3) Days	(4) Farm work	(5) Hours Farm	(6) Days Farm	(7) Enrolled at school	(8) Time to study	(9) School expenditures	(10) School expenditures: fees	(11) School expenditures: uniforms/shoes
Panel A											
CGP	-0.02	-1.57	-0.33*	-0.02	-1.80*	-0.37*	0.05***	6.20**	39.60	-2.03	41.71***
	(0.035)	(0.979)	(0.172)	(0.035)	(0.949)	(0.193)	(0.018)	(2.424)	(35.671)	(28.360)	(6.845)
Panel B											
CGP*1Q	0.04	0.43	0.04	0.05	-0.51	0.07	0.05	4.19	-44.83	-95.75	52.61***
	(0.058)	(1.714)	(0.345)	(0.061)	(1.712)	(0.389)	(0.030)	(5.365)	(79.592)	(63.817)	(15.085)
CGP*2Q	-0.01	-2.49	-0.30	-0.02	-2.55	-0.43	0.02	2.84	3.59	-18.85	34.44**
	(0.052)	(1.910)	(0.311)	(0.055)	(2.327)	(0.369)	(0.033)	(4.583)	(53.716)	(35.842)	(16.921)
CGP*3Q	0.01	-1.19	-0.30	0.01	-0.80	-0.29	-0.00	12.17**	73.77	43.95	40.25**
	(0.065)	(1.829)	(0.336)	(0.078)	(1.538)	(0.361)	(0.028)	(5.691)	(66.357)	(56.732)	(15.706)
CGP*4Q	-0.15**	-3.67**	-0.91***	-0.18**	-3.83*	-0.98***	0.15***	8.10*	210.89***	129.45**	39.68**
	(0.067)	(1.766)	(0.249)	(0.068)	(2.047)	(0.280)	(0.053)	(4.638)	(74.884)	(51.977)	(18.537)
F-test	1.741	1.730	3.791	2.234	1.444	3.674	2.817	2.334	2.195	2.188	9.770
Prob > F	0.149	0.152	0.007	0.072	0.227	0.009	0.031	0.063	0.077	0.078	0.000
Panel C											
CGP*Age	-0.00	-0.16	-0.01	-0.00	-0.09	-0.01	0.02**	0.28	-5.42	-2.43	-1.60
	(0.009)	(0.309)	(0.054)	(0.010)	(0.330)	(0.061)	(0.007)	(1.194)	(19.183)	(14.379)	(3.451)
CGP*Female	-0.01	2.43	0.43	0.02	3.26*	0.62*	0.01	-5.32	21.01	36.01	-19.34
	(0.047)	(1.903)	(0.344)	(0.047)	(1.759)	(0.341)	(0.033)	(4.429)	(85.712)	(62.900)	(14.015)
Obs.	1,938	1,938	1,938	1,734	1,716	1,717	1,938	1,707	1,749	1,747	1,740

***p<0.01, **p<0.05, *p<0.1; sampling weights adjusted for attrition bias; SE clustered at ED level

We do not observe significant heterogeneous impacts of the CGP on child labour according to age (Panel C of Table 4). However, the impact of the programme on education seems to be higher on the school enrolment of older children. This result is quite reasonable in the context of Lesotho, where only primary school is free and compulsory and most of younger children (aged

6-11) are enrolled. For younger children, therefore, the impact of the transfer was marginal with respect to school enrolment. Finally, there is not significant heterogeneity by gender. There is weak evidence of a higher reduction on time spent working for boys with respect to girls.

6.2. Robustness check

As robustness check for heterogeneous impacts by poverty, we consider, as mentioned, the four quartiles of the asset index as possible indicators of socio-economic conditions of households. The differentiation of households' choices on the allocation of the CGP according to the poverty status, presented in Table 4, is corroborated using this different measure of deprivation, as shown in Tables A.1 in the Appendix.

We also carried out the estimates using the median and the mean expenditures at baseline as well as a dummy indicating whether the household was below the national poverty line at baseline. The results (not shown) are consistent with those reported in the paper.

6.3. Spill over effects

Policy interventions can affect also local non-target population. Spill-over effects can take place thanks to different transmission mechanisms (Angelucci and De Giorgi 2009). We analyse if indirect effects took place by exploiting the random assignment and the availability of data of non-eligible households in treated and control EDs at baseline and at follow-up. We compare outcome variables of non-eligible households in treated EDs with outcome variables of non-eligible households in control EDs at follow-up controlling for background characteristics considered at baseline[16]. Table 5 indicates that the CGP did not affect non-eligible children neither in terms of labour nor in terms of education.

[16] We adopt our preferred estimation specification used in the previous sections.

Table 5: Impact on non-eligible children - OLS

	(1) Work	(2) Hours	(3) Days	(4) Farm work	(5) Hours Farm	(6) Days Farm	(7) Enrolled at school	(8) Time to study	(9) School expenditures	(10) School expenditures: fees	(11) School expenditures: uniforms/shoes
CGP	-0.01	-0.09	-0.12	-0.01	-0.47	-0.12	-0.01	-2.05	-40.06	-55.09	8.98
	(0.05)	(1.90)	(0.24)	(0.05)	(1.81)	(0.24)	(0.02)	(4.22)	(80.05)	(57.32)	(21.52)
Obs.	799	799	799	741	724	728	798	707	742	741	739

***p<0.01, **p<0.05, *p<0.1; sampling weights adjusted for attrition bias; SE clustered at ED level

7. **Conclusion**

The possible non linearity of the income elasticity of child labour is an essential element for understanding its causes and for designing effective policies. Especially as one of the main interventions advocated and implemented to address it relies on various forms of income transfers.

Causal evidence of the (non linear) impact of income on child labour is scarce and (with the exception described in the introduction) mainly based on experimental evidence deriving from CCT implemented in middle-income countries.

As we have seen, theory indicates that below a critical ("subsistence") income level, changes in income do not affect child labour and school attendance. Only above this critical level, changes in income affect (at a decreasing rate) household decisions concerning child labour.

The existing evidence seems to indicate that the income elasticity of child labour decreases with household income. In this paper we show that is not always the case and that there are situations in which the observed behaviour is consistent with the theory outlined: transfers affect only the behaviour of the relative less poor households.

We have analysed the possible non linear response to income changes by evaluating the impact of the CGP (Phase 1- Round 2) on two specific dimensions of child wellbeing: children's work and education. The CGP is an unconditional cash transfer randomly assigned to poor households of Lesotho providing a regular transfer every quarter.

Looking at the aggregate effects on all the beneficiaries, we find that the CGP generated an increase of the enrolment rates by about 4%, of the time spent on studying by 13% and of the expenditures on uniforms and shoes. No significant effect on child labour was identified.

However, we find substantial heterogeneous treatment effects by household income. Significant reduction in both extensive and intensive margins of children's work and increase in enrolment rates and expenditure on school fees can be identified only for children belonging to relatively less poor households. The poorest households apparently used the transfer only to increase expenditures on school uniforms and shoes, without changing children's time allocation.

These findings are consistent with the theoretical framework developed and appear to support the hypothesis that the effectiveness of cash transfer can also increase, at least within a given range, with the level of income of the beneficiary households.

It is of course not straightforward to reconcile our results with those in the literature. We can only speculate, that one reason is due to the fact that the existing literature refers to CCT transfers and we have seen that, if large enough CCT transfers might have a larger impact than UCT on child labour at low levels of income. Moreover, as mentioned, the existing literature is relative to middle income countries where levels of deprivation are obviously lower.

Our analysis indicates that, at least in low income countries UCT might not affect the decisions of the extremely poor in terms of school attendance and child labour. From a policy point of view, our results stress the importance of targeting, transfer size and also point to some of limits of the use of cash transfer as an instrument to promote school attendance and reduce child labour in situation of extreme poverty. In fact, the results indicate that cash transfers might not be always effective to support the investment in children's human capital of the poorest households. Beside the integration with other measures, making the amount of transfer depends of the level of deprivation of the household might improve cash transfer effectiveness. Indexing the transfer to the number of children (as implemented in the reformed CGP) might represent a possible compromise. More in

general, monetary transfers appear not to be necessarily a sufficient instrument to reduce child labour and promote school attendance of children belonging to the poorest households.

References

Angelucci M., De Giorgi G., 2009. "Indirect Effects of an Aid Program: How do Cash Transfers Affect Ineligibles' Consumption?", *American Economic Review*, American Economic Association, vol. 99(1), pages 486-508, March

Attanasio O., Fitzsimons E., Gomez A., Gutiérrez M. I., Meghir C. and Mesnard A., 2010. "Children's Schooling and Work in the Presence of a Conditional Cash Transfer Program in Rural Colombia", *Economic Development and Cultural Change*, Vol. 58, No. 2 (January 2010), pp. 181-210

Ayala Consulting, 2011. "National Information System for Social Assistance Update on Coverage of Qiloane CC", May, prepared for UNICEF. Government of Lesotho

Basu, K., Van, P., 1998. "The economics of child labor", *American Economic Review*, Vol. 88, pp. 412–427

Cigno A., Rosati F. C., 2005. "The Economics of Child Labour", Oxford University Press

Covarrubias K., Davis B., and Winters P., 2012. "From Protection to Production: Productive Impacts of the Malawi Social Cash Transfer Scheme", *Journal of Development Effectiveness*, 4 (1): 50–77.

Dammert A. C., 2009. "Heterogeneous Impacts of Conditional Cash Transfers: Evidence from Nicaragua", *Economic Development and Cultural Change*, Vol. 58, No. 1 (October 2009), pp. 53-83

De Hoop J., Friedman J., Kandpal E., Rosati F. C., 2016. "Complementarities between schooling and child work in the presence of an education subsidy", Understanding Children's Work Working Paper

De Hoop J., Rosati F. C., 2014. "Cash Transfers and Child Labor", *World Bank Research Observer*, World Bank Group, vol. 29(2), p. 202-234

De Janvry A., Finan F., Sadoulet E., and Vakis R., 2006. “Can Conditional Cash Transfer Programs Serve as Safety Nets in Keeping Children at School and from Working when Exposed to Shocks?” *Journal of Development Economics*, 79 (2): 349–73.

Edmonds E. V., 2005. “Does Child Labor Decline with Improving Economic Status?”, *Journal of Human Resources*, University of Wisconsin Press, vol 40(1)

Edmonds E. V., Schady N., 2012. “Poverty Alleviation and Child Labor”, *American Economic Journal: Economic Policy*, 2012, 4(4): 100–124

Edward Taylor, Karne Thome and Mateusz Filipski, (2014). “Evaluating local general equilibrium impacts of Lesotho's child grants programme”, FAO 2014.

Galiani S., McEwan P. J., 2013. “The heterogeneous impact of conditional cash transfers”, *Journal of Public Economics*, 103 (2013) 85–96

Glewwe P., Olinto P., 2004. “Evaluating the impact of conditional cash transfers on schooling: an experimental analysis of Honduras’ PRAF program”, University of Minnesota and IFPRI-FCND

International Labour Office, 2013. Global Child Labour Trends 2008 to 2012. Geneva

Pace N., Daidone S., Davis B., Pellerano L., 2016. “Does “soft conditionality” increase the impact of cash transfers on desired outcomes? Evidence from a randomized control trial in Lesotho”, Working Paper 2016:33, Department of Economics, University of Venice “Ca’ Foscari”

Pellerano L., Hurrel A., Kardan A., Barca V., Hove F., Beazley R., Modise B., MacAuslan I., Dodd S., Crawfurd L., 2012. “Child Grant Programme Impact Evaluation - Targeting and Baseline Report”. Oxford Policy Management

Pellerano L., Moratti M., Jakobsen M., Bajgar M., Barca V., 2014. “Child Grant Programme Impact Evaluation - Follow-up Report”. Oxford Policy Management

Schultz, T. Paul. 2004. “School Subsidies for the Poor: Evaluating the Mexican Progresa Poverty Program.” *Journal of Development Economics*, 74 (1): 199–250

Sparrow, R. 2007. “Protecting Education for the Poor in Times of Crisis: An Evaluation of a Scholarship Programme in Indonesia.” *Oxford Bulletin of Economics and Statistics*, 69 (1): 99–122

Appendix

Table A.1: Heterogeneous impatcs - Quartiles of asset index (OLS)

	(1) Work	(2) Hours	(3) Days	(4) Farm work	(5) Hours Farm	(6) Days Farm	(7) Enrolled at school	(8) Time to study	(13) School expenditures	(14) School expenditures: fees	(15) School expenditures: uniforms/shoes
CGP*1Q	0.02	0.57	-0.02	0.03	-0.95	-0.06	0.03	1.39	68.78	44.43	28.90**
	(0.067)	(2.057)	(0.289)	(0.066)	(1.972)	(0.314)	(0.060)	(3.926)	(51.670)	(38.429)	(13.470)
CGP*2Q	-0.05	-1.69	-0.21	-0.07	-1.65	-0.30	0.00	13.41**	18.60	-26.26	42.65**
	(0.060)	(1.756)	(0.269)	(0.060)	(1.534)	(0.296)	(0.020)	(6.185)	(69.368)	(49.089)	(19.375)
CGP*3Q	0.10	-0.41	-0.42	0.09	-0.28	-0.43	0.07***	6.92	28.52	-0.52	35.85**
	(0.059)	(1.630)	(0.317)	(0.062)	(1.767)	(0.331)	(0.024)	(4.875)	(72.189)	(48.429)	(16.298)
CGP*4Q	-0.14**	-3.85**	-0.69*	-0.16***	-3.63**	-0.74*	0.07**	2.77	28.14	-18.28	49.40**
	(0.056)	(1.703)	(0.352)	(0.059)	(1.753)	(0.376)	(0.033)	(4.247)	(90.555)	(63.279)	(19.755)
Obs.	1,967	1,967	1,967	1,764	1,744	1,745	1,967	1,734	1,777	1,775	1,768

***p<0.01, **p<0.05, *p<0.1; sampling weights adjusted for attrition bias; SE clustered at ED level