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Income Elasticity of Child Labor: Do Cash Transfers have an Impact on the Poorest Children?

Abstract

The possible nonlinearity of the income elasticity of child labor has been at the center 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 labor and education. We estimate the causal impact of an increase in income on child labor and education exploiting the random allocation of the Child Grant Programme, an unconditional cash transfer (CT), in Lesotho. We show that the poorest households do not increase investment in children's human capital, while relatively less poor households reduce child labor and increase education. In policy terms, the results indicate that CTs 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 CT effectiveness.

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1 Introduction

According to the International Labour Organization (ILO, 2017), 152 million children aged 5–17 were involved in child labor in 2016: 10% of children in this group. Most of the working children live in low- and middle-income countries. Sub-Saharan Africa shows the highest incidence of child labor, 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 labor concerns the role of poverty as one of its main determinants. Understanding whether household income affects child labor 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 labor. Both targeting and transfer size are essential elements of CTs (conditional or not) and, of course, considering the heterogeneity of response at different levels of household income is an important element for their efficient design.

Cross-sectional and cross-countries studies, however, do not indicate the presence of a substantial child labor income elasticity. As pointed out in the study by Edmonds (2005), this can be due to endogeneity problems and also the inherent nonlinearity in the relationship between child labor and income. In fact, assuming that the income elasticity of leisure is close to zero for poor households, the Basu and Van's (1998) subsistence hypothesis implied that an increase in income affects child labor supply so far as it allows households to raise above subsistence. The empirical evidence on the nonlinearity of income elasticity is not very large. Edmonds (2005) showed that the effect of the increase in income in Vietnam, between 1993 and 1998, was present mainly for the households that moved above the poverty line, and hence it was limited for households who experienced an increase in income not sufficient to bring them above the poverty line. A few impact evaluation studies, mainly focused on conditional CTs (CCTs) in Latin America, study heterogeneity by income, but the issue remains unsettled. They found different results: Galiani and McEwan (2013) for Honduras and Sparrow (2007) for the Jaringam Pengaman Social in Indonesia found larger reduction of child labor and increase in school enrolment among the children belonging to the relatively poorer household. Glewwe and Olinto (2004) for the Honduran PRAF-II and Dammert (2009) for the Social Safety Net in Nicaragua did not find any heterogeneous impact, while Ranzani and Rosati (2004) found that the impact of Oportunitades in Mexico on child labor increases slightly with the level of household income.

In this study, we provide novel evidence on the nonlinear relationship between child labor and income and, to our knowledge, the first causal study on the effects on child labor of unconditional CT (UCT). Assessing the impact of a UCT provides solid evidence about the nonlinearity of the income effect as it generates a pure income effect, without additional changes in the budget sets (as, e.g., those induced by a CCT).

We show in a simple theoretical model, built in the Basu and Van's (1998) spirit, that the income elasticity of child labor is null for extremely poor households, and it becomes negative (and decreasing with income) for relatively less poor households and finally becomes zero as households become more affluent. We will also discuss how CCTs are more likely with respect

1 For a review, see Edmonds (2005) and Cigno and Rosati (2005).

to UCTs to affect also the poorest households, thus identifying a possible reason for the differences between our results and some of those in the literature.

We make use of an experiment relative to an UCT in Lesotho and offer new evidence on the income elasticity of child labor and on the effectiveness of CT 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: extremely poor households do not change children's time allocation, while relatively less poor households reduce child labor 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% and work on average 3 hours less per day 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 nonlinearity of the relationship between child labor and income and by extending the evidence on the heterogeneous effects of CTs. In particular, we show that the effectiveness of CTs does not necessarily increase with the level of deprivation of the household. Moreover, to the best of our knowledge, this is the first attempt to analyze heterogeneous impacts by income on child labor of UCT in sub-Saharan Africa. As discussed, the literature on heterogeneous effects by income 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.

From a policy point of view, our results confirm that targeting is very important for insuring the effectiveness of CTs. However, to obtain the desired effects in terms of child labor reduction, the transfers should be large enough to modify household behavior. Some forms, even simplified, of means testing of the amount of benefits would be necessary to improve the efficiency of the transfer. Our findings also point to the need to pay more attention to the size of the transfer in assessing its impact, a somehow obvious point but often neglected in the literature.[2]

We also test for the presence of spillover effects, by assessing the impact of the CGP on non-eligible children. The results indicate the absence of spillover effects, as neither child labor nor child education in non-eligible households were influenced by the program.

This article 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. 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 an exogenous level of subsistence consumption. In particular, we consider

2 See De Hoop and Rosati (2014).

a two-period 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 labor 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, $g(S)$. We assume that individuals have an innate amount of human capital, so that $g(0) = k > 0$. Parents inelastically supply 1 time unit of work in period 1. Labor income plus any additional nonlabor income constitute the resources available to the household in the first period, Y_1. The households' maximization problem can hence be written as follows:

$$\begin{aligned} &\text{Max}\, U(C_1, C_2) \\ &s.t. \quad C_1 = wH + Y_1 + \tau - eS \\ &C_2 = g(S) \\ &1 = H + S \end{aligned} \tag{1}$$

where $U(.)$ is a concave utility function with $U(.)' > 0$ and $U''(.) < 0$ and τ is an UCT. Expressing child labor supply in terms of schooling and allowing for corner solutions, the Lagrangian function, the First Order Conditions (FOC) and the complementary slackness conditions are:

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

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

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

$$\lambda_2 S = 0$$

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 $\lambda_2 > 0$, $S = 0$, $g'U'_{C2} < U'_{C1}\,(e + w)$, and $\frac{\partial S}{\partial \tau} = 0$. The level of resources of the household is very low, given the other parameters of the model, that the household allocates the time of their children only

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 and Rosati (2005).

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:

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

with $H > 0$, $S > 0$, $\lambda_1 = 0$, $\lambda_2 = 0$. The amount of time dedicated to each activity is determined as 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 $\lambda_1 > 0$, $S = 1$, $g'U'_{C2} > U'_{C1}(e + w)$ and, obviously, $\frac{\partial S}{\partial \tau} = 0$. The households with relatively higher income send their children only to school 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 behavior (unless the increase is such that $Y_1 + d\tau > Y^*$). For relatively less poor households an increase in income reduces child labor 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 extremely 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 simply discussed would be partially different. In the following text, 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 also depend on the amount of the transfer. If $\tau > w(1 - S^*) + eS^*$, 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 labor, as $U(Y_1 + w(1 - S^*) + \tau - eS^*, g(S^*)) > U(Y_1 + w)$, 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 $\tau < w(1 - S^*) + eS^*$, the effect is ambiguous, as in this case $U(Y_1 + w(1- S^*) + \tau - eS^*, g(S^*))$ can be higher or lower than $U(Y_1 + w)$.

Therefore, a CCT might reduce child labor 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] This is a sufficient, but not necessary condition: depending on the shape of the utility

5 For a discussion of the impact of a partial CCT subsidy, see De Hoop et al. (2019).

function and on the other parameters of the model, a CCT might reduce child labor in households below subsistence even if it does not fully cover direct and opportunity costs.

3 The Child Grant Programme

3.1 Background

Lesotho is one of the poorest countries in the world, with a GDP per capita of 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 Protection and Welfare Act), and 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 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 UCT 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 the 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 program 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, 2014). We make use of the impact evaluation data to assess the impact of the program on child labor and education.

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

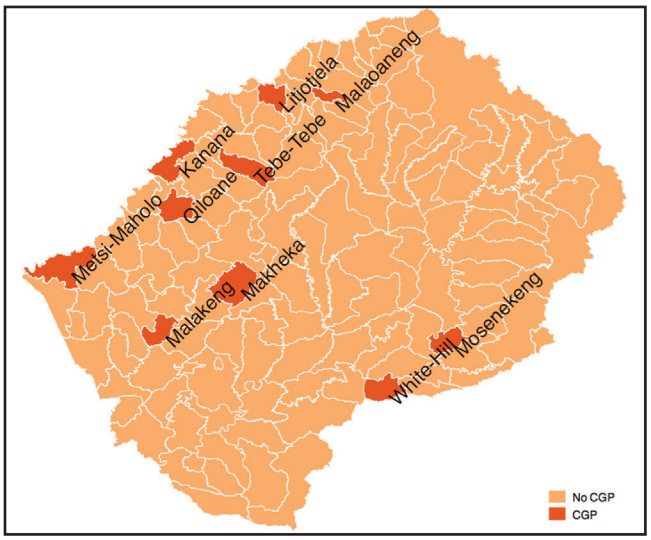
6 Most recent data available from The World Bank Development Indicators.

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

8 Pace et al. (2016) showed 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 Child Grant Programme (CGP) in Phase 1—Round 2.



transfer has been indexed to the number of children (aged 0–17) in the household.[10] The new mechanism marginally affected only 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 cannot ascribe changes in outcome variables to each of them separately and therefore our results reflect the impact of both transfers.

The monthly transfer of the CGP represents about 16% of the 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.

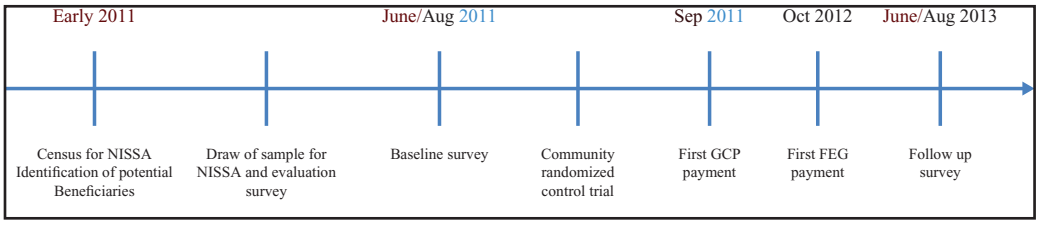
3.2 Experimental design

Phase 1—Round 2 of the program was originally intended to provide CT to all eligible households in the 10 selected 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 Electoral Division (ED), 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. Socioeconomic information on all households living in the 10 CCs was collected through a community census in early 2011 and used to set up the

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.

Figure 2 Timeline of Child Grant Programme (CGP) (Phase 1—Round 2) evolution.



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). 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 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.

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

The household surveys for the CGP impact evaluation were administered to a random sample of eligible households in treatment and control EDs. To assess possible spillover effects of the program, the surveys were also administered to a random sample of non-eligible households in treatment and control EDs. Information of 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 multistage 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 socioeconomic information, adult and child education and work, children's health and time use, and 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 labor market, local prices, and other community characteristics.

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),

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 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 the third stage, 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.

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 6 and 7. 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 behavior, 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%, respectively, think 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 program, i.e., "the money is for the children." For 69% of households, decision on the spending of the transfer is undertaken by only one person: the household head in the majority of the cases. Of note, 55% of beneficiaries declare spending the money 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 Child Grant Programme (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[14] (available on request) shows that the nonresponse rate is not random between treatment and control group, as shown also in the study by Pellerano et al. (2014).

We use sampling weights to make inference on the entire "study population," i.e., the NISSA population. Moreover, 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 households to remain in the sample at follow-up. Sampling weights adjusted for attrition bias are used throughout this study.

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

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.

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 to both the loss of observations from baseline and 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 program and, for sake of this study, not included in the final sample. Whereas, for split households still eligible for the program, 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.

14 The regression of the probability of remaining in the sample indicated that it was not independent from some observable characteristics of the households. As discussed in the remaining of the section, we corrected for this by using inverse probability weights.

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 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

Table 3. Balance analysis on eligible children at baseline

	Treatment	Control	Difference
Panel A: outcome variables			
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 expenditure on fees	30.46	40.68	−10.22
School expenditure on uniforms/shoes	1.99	3.59	−1.6
Panel B: covariates			
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 household	0.56	0.52	0.04
Female household head	0.51	0.55	−0.04
Age of household head	52.39	52.81	−0.42
Highest education among adults in the household	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

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 one demographic characteristic: treated households have a somehow larger household size than control households.

Overall, 30% of eligible children worked in 12 months preceding the survey. Almost the totality of working children was employed in farming and livestock activities within the household (98%).[15] 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 20 and 3, respectively. Of note, 92% of the children were enrolled in school at the time of the baseline survey and they spent about 45 min/day doing homework. Expenditure on school fees and on uniforms and shoes per child amounted to M35 and M3, respectively.

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 labor 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 spillover.[16] Our estimates are, therefore, very close to the average treatment effect (ATE).

In particular, we estimate with Ordinary Least Squares (OLS) the following equation:

$$Y_{iv} = \beta_0 + \beta_1 X_{iv} + \beta_2 T_v + \varepsilon_{iv} \tag{4}$$

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 CCs. Robust and clustered standard errors at ED level and sampling weights adjusted for attrition bias are used throughout the analysis.

To analyze 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:

$$Y_{iv} = \beta_0 + \beta_1^{'} X_{iv} + \beta_2 T_v + \beta_3 C_{iv} + \beta_4 C_{iv} T_v + \varepsilon_{iv} \tag{5}$$

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

16 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 with four dummy variables for each quartiles of monthly expenditure per capita (at baseline) and with demographic characteristics of children to assess heterogeneity by age and sex.

Estimation results of Eqs (4) and (5) are shown in Section 6.1.

As robustness check for heterogeneity by level of expenditure per capita at baseline, we estimate Eq. (5) interacting the treatment variable with dummy variables of alternative measures of deprivation, such as the quartiles of the asset index and the mean and the median of expenditures at baseline. The results of the robustness checks are discussed in Section 6.2 and presented in Appendix.

We also estimate the probability to remain in the sample for beneficiary and non-beneficiary households, including the quartiles of expenditure per capita as covariates, to reassure that the estimation results are not driven by sample attrition. As shown in Table A.1 in Appendix, the coefficients relative to the quartiles are not statistically significant, indicating that the attrition is not driven by the economic condition of the households.

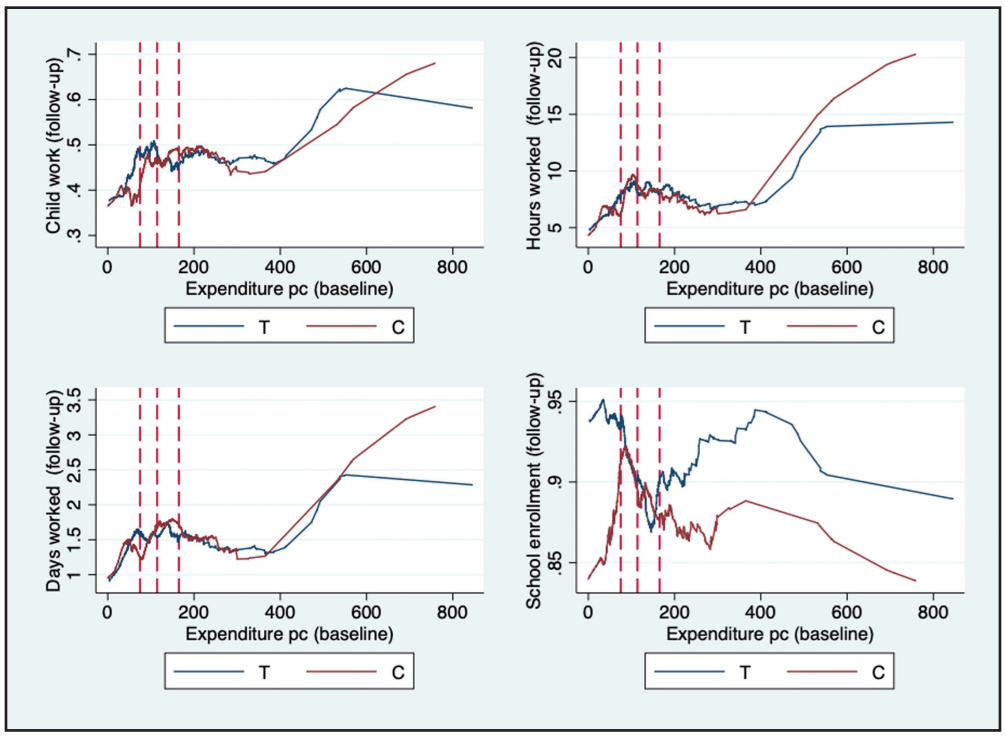
Finally, we test for the existence of spillover effects comparing non-eligible children in treatment and control EDs. The estimation results are presented in Section 6.3.

6 Results

6.1 Impact of the CGP

Preliminary visual evidence is shown in Figure 3, which presents local linear regressions (with bandwidth equal to 0.3) of child labor (participation, and hours and days work) and school enrolment on expenditure per capita for treatment and control children. The vertical lines represent the quartiles of expenditure per capita. The results indicate that large differences

Figure 3 Local regressions.



between treatment and control children in the outcome variables are observed only for relatively high expenditure per capita.

In Panel A in Table 4, we report results of the overall impact estimated through Eq. (4). The program increased school enrolment, but it had no effects on child labor. 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 (Columns 5 and 6 in Table 4).

Five percent of treated children are more likely to be enrolled in school and spend 6 min 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 increased expenditures on uniforms and shoes by M42, a substantial amount considering that this expenditure amounted only to 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 labor in agricultural household production is significant only for relatively less poor households (17% points on participation, 3 h and almost 1 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% points on school enrolment, 9 min 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 in Table 4.

Heterogeneous impacts by income are in line with the theoretical framework discussed in Section 2. From the theoretical model and estimation results, it is estimated 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. The sample size does not allow to test the other prediction of the theoretical model that the impact of the CT on child labor decline with income for households above "subsistence."

We do not observe significant heterogeneous impacts of the CGP on child labor according to age (Panel C in Table 4). However, the impact of the program on education seems to be higher on the school enrolment of older children. This result is quite reasonable in the context

Table 4. Impact of the Child Grant Programme (CGP)—OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Work	**Hours**	**Days**	**Farm work**	**Hours: farm**	**Days: farm**	**Enrolled at school**	**Time to study**	**School expenditures**	**School expenditures: fees**	**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)
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)
Observation	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.

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 socioeconomic 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.2 in 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 (available on request) are consistent with those reported in the paper.

6.3 Spillover effects

Policy interventions can also affect local nontarget population. Spillover effects can take place due to different transmission mechanisms (Angelucci and De Giorgi, 2009). We analyze 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. As mentioned, eligible households were selected according to a well-defined set of criteria and they were identified both in EDs that were treated and in EDs that did not receive the treatment. The baseline and the follow-up surveys, moreover, also contained information on households who were not selected as beneficiaries in both treated and nontreated EDs. We exploit this information and 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 their background characteristics considered at baseline.[17] Table 5 indicates that the CGP did not affect non-eligible children neither in terms of labor nor in terms of education.

7 Conclusion

The possible nonlinearity of the income elasticity of child labor 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 transfer.

Causal evidence of the (nonlinear) impact of income on child labor is scarce, far from unambiguous and (with the exceptions discussed in the introduction) mainly based on experimental evidence deriving from CCT implemented in middle-income countries.

17 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
Child Grant Programme	−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)
Observation	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.

There is no experimental evidence based on UCT programs that mimic better than CCTs, a pure income effect and, therefore, allow to test for the nonlinearity of the impact of income changes on child labor.

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

We have analyzed the possible nonlinear response to income changes by evaluating the impact of the CGP (Phase 1—Round 2) on two specific dimensions of child well-being: children's work and education. The CGP is a UCT randomly assigned to poor households of Lesotho providing a regular money 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 labor 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 CT can increase, at least within a given range, with the level of income of the beneficiary households, thus offering solid evidence toward the hypothesis of nonlinearity of the income effect on child labor.

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 labor. From a policy point of view, our results stress the importance of insuring that the amount of the transfer is sufficient to bring the household above "subsistence" to affect the decisions of the extreme poor. In contrast to CCT, for UCT is not sufficient that the transfer covers the (direct and opportunity) cost of taking children out of work and into school,[18] but to be effective toward the poorest households transfers need to lift the household from extreme poverty. This points to some of the limits in the use of CT as an instrument to promote school attendance and reduce child labor in situation of extreme poverty. In fact, the results indicate that CTs 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 depend on the level of deprivation of the household might improve CT effectiveness. This might not be easy from an administrative point of view and might prove to be costly. Using simple proxy for the level of deprivation might be useful: for example, indexing the transfer amount to the number of children (as implemented in the reformed CGP) might represent a possible compromise.

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Appendix

Table A1 Probability to remain in the sample and quartiles of expenditure per capita

Variables	Probability to remain in the sample
1Q	0.13
	(0.090)
2Q	−0.01
	(0.083)
3Q	0.06
	(0.082)
HH size	0.05***
	(0.014)
Dependency ratio	0.00***
	(0.000)
One member household	−0.65***
	(0.236)
Double orphan in the household	−0.04
	(0.083)
Non resident head	−0.10
	(0.099)
Chronically ill member in the household	−0.05
	(0.061)
Savings	−0.01
	(0.061)
Livestock	−0.10
	(0.065)
Land	0.19**
	(0.095)
Asset index	−0.01
	(0.018)
nissa1	0.77***
	(0.087)
nissa2	0.48***
	(0.085)
nissa3	0.06
	(0.145)
nissa4	−0.13
	(0.149)
Constant	−0.50**
	(0.206)

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$; SE clustered at ED level; Community Councils fixed effect included as controls.

Table A2 Heterogeneous impacts—Quartiles of asset index (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(13)	(14)	(15)
	Work	**Hours**	**Days**	**Farm work**	**Hours: farm**	**Days: farm**	**Enrolled at school**	**Time to study**	**School expenditures**	**School expenditures: fees**	**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)
Observation	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.