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Does "soft conditionality" increase the impact of cash transfers on desired outcomes? Evidence from a randomized control trial in Lesotho

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Abstract

Cash transfers programs have been shown to have positive effects on a variety of outcomes. While much of the literature focuses on the role of conditionality in achieving desired impact, this paper focuses on the role of 'soft conditionality' implemented through both 'labeling' and 'messaging' in evaluating the impact of the Child Grants Program in Lesotho, an unconditional cash transfer targeting poor households with orphans and vulnerable children. Beneficiary households received a clear message that the transfer should be spent on the interest and needs of children. Our findings are based on a randomized experiment and suggest that 'soft conditionality' does play a strong role in increasing expenditure for children, especially on education, clothing and footwear. Results indicate in fact that transfer income is spent differently from general income as it exerts both an income and a substitution effect. This behavioral change is confirmed by comparing the ex-ante expected behaviors with the ex-post actual response to the program. We find that for expenditure categories linked to the wellbeing of children the ex-post response was much higher than the ex-ante expected behavior.

JEL classification: C93, D12, I38, O18

Keywords: cash transfers; consumption; food security; impact evaluation; randomized experiment; soft conditionality.

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1. Introduction

Over the past twenty years, a growing number of African governments have launched social protection programs to provide assistance to households that are ultra-poor, labor-constrained, and/or caring for orphan or vulnerable children. Usually these programs aim at reducing poverty and vulnerability by improving consumption, nutrition, health status, school attendance and educational outcomes.[1] While most of the programs in Latin America provide cash transfers conditional on meeting certain requirements (mainly school attendance, regular visits to health center for growth monitoring and updating of vaccination cards), the majority of the cash transfer programs in African countries are 'unconditional': they are paid directly to beneficiary households without explicit conditions or labor requirements.

A number of papers have discussed the pros and cons of conditional cash transfers (CCTs) as opposed to unconditional cash transfers (UCTs), from both a public and private perspective (see, for example Baird et al. 2013, de Brauw and Hoddinott, 2011 and Handa et al. 2009). From the public perspective, imposing conditions may help the government to overcome information asymmetries: government may be aware of the benefits associated with preventive health care or education but individuals may be unconvinced or unaware of these benefits, they may have a shorter time horizon because of lack of risk management instruments, or there may be cultural barriers to investing in certain activities such as girls' education. From the private perspective, imposing conditionality on cash transfers can partially solve the disagreements within households regarding the allocation of resources, strengthening the bargaining position of individuals whose preferences are aligned with the government's preferences, and who may otherwise lack bargaining power within the household. Indeed, the majority of CCTs pays money preferentially to female recipients. Furthermore, insights from behavioral economics emphasize that conditionality can impose a constraint to those households who have hyperbolic discount functions, i.e. when they tend to choose a smaller-sooner reward over a larger-later reward as the delay occurs sooner rather than later in time, undertaking actions that can reduce their own welfare. In such cases, households may be better off when constraints are imposed that reduce or limit their ability to trade-off future for present consumption (Laibson, 1997).

However, there are also drawbacks to imposing conditionality. From a public perspective, it increases the administrative costs and complexity of running a cash transfer program (Caldes

[1] For a comprehensive overview of the impacts of cash transfers programs see Fizbein et al. (2009) and Tirivayi et al. (2016).

et al. 2006). From a private perspective, imposing conditionality may reduce the effectiveness of the targeting if the poorest households find the conditions too difficult to meet, *de facto* hindering their participation to the cash transfer program. Moreover, imposing conditionalities may be considered paternalistic and may induce beneficiaries to take options that are suboptimal, e.g. if returns to education are too low. Finally, from a human right perspective, some argue against attaching conditions to the receipt of the cash transfers, especially because the purpose of the programs is to reduce or mitigate the effects of extreme poverty (Freelander, 2007).

Within this debate, several contributions have sought to identify the isolated effect of conditionality as the key feature to optimize behavior and maximize the effectiveness of cash transfers programs (Bastagli et al. 2016). The results are mixed. Handa et al. (2009) evaluate the behavioral impact of conditions on spending behavior in rural Mexico by the *Progresa* CCT program. Their results show that transfer income is not spent on education, food and clothing differently from general income suggesting that cash transfers exert only an income effect, i.e. the 'hard conditionality' imposed by the program does not induce behavioral changes (the so called 'substitution effect'). Teixeira et al. (2011) evaluate the impact of the CCTs Takopora in Paraguay on healthcare utilization and school attendance. They exploit the heterogeneity with respect to knowledge of the need to comply with conditionalities as identification strategy and find no significant role of 'hard conditionality'. Akresh et al. (2013) conduct a randomized experiment in rural Burkina Faso to estimate the impact of alternative cash transfer delivery mechanisms (CCTs versus UCTs) on education. The results indicate that unconditional and conditional cash transfer programs have a similar impact, increasing enrollment for children who are traditionally favored by parents for school participation, including boys, older children, and higher ability children. However, the conditional transfers are significantly more effective than the unconditional transfers in improving the enrollment of "marginal children" who are initially less likely to go to school, such as girls, younger children, and lower ability children. They conclude that conditionality actually plays a critical role in benefiting children who are less likely to receive investments from their parents. Robertson et al. (2013) investigate the effects of CCTs and UCTs on birth certificates, vaccine uptake and school attendance in a randomized control trial in Zimbabwe and find inconclusive results on the role of 'hard conditionality'. Birth registration increased significantly more in the CCT group, vaccine uptake increased significantly more in the UCT group, and no differences were detected in school attendance.

As opposed to these findings, several contributions find that the 'hard' conditionality imposed in the CCTs significantly contribute to amplify the effects of the cash transfers on desired outcomes. Analyzing *Progresa* in Mexico, de Janvry et al. (2006) compare the effect of the 'explicit conditionality' and a pure income effect which in their analysis simulates the potential impact of a UCT on education. They find that a dollar spent on conditional cash transfers would give an effect on school enrollment eight times higher with respect to a dollar spent on increasing household's income. Using the same data, de Brauw and Hoddinott (2011) took advantage of the fact that some beneficiaries did not receive monitoring and compliance forms for a substantial period of time after the program was launched and, therefore, were unaware that the transfers were conditional. They found that receiving the form and understanding the condition exerted a stronger effect on school enrollment. Schady and Araujo (2008), exploiting differential parental beliefs on the school attendance requirement attached to the program, find similar results from the Ecuadorian program *Bono de Desarrollo Umano*. Using data from a randomized control trial in Malawi, explicitly designed to evaluate the differential impact of CCT and UCT on the school attendance rates of teenage girls, Baird et al. (2011) find that conditionality contributes to amplify the effects of the cash transfers on investments in human capital (i.e. better educational and health outcomes). 'Hard conditionality' has also been shown to matter for health behavior outcomes. Using data from the Colombian program *Familias en Acción* Attanasio et al. (2015) estimate that children would receive less preventive care visits if the program was not conditional on these visits.

Overall, while most of previous contributions focus on the role of the conditionality, as opposed to unconditionality, we argue that the difference between CCTs and UCTs is more nuanced for two reasons. First, as the meta-analysis by Baird et al. (2013) point out, the level of enforcement makes an important difference when it comes to measuring the role of 'hard conditionality'. Second, and more relevant for this paper, many existing UCTs impose some sort of informal or indirect conditionality (Pellerano and Barca, 2014). The informal or indirect conditionality, often referred as 'soft conditionality', may occur in several ways. The use of cash transfers can be implicitly conditioned by policy actions that are implemented in conjunction with the transfer. This happens, for example, when beneficiaries are involved in training/education sessions that provide information on the 'best use' of the transfers, or when community-based case management systems are put in place to oversee the 'good use' of the transfer. Examples of this 'soft conditionality' implemented through the messaging are the Child Grants Program in Lesotho and the Colombian *Familias en Acción*. In this latter program

beneficiary women are involved in training sessions to share information about adequate child care, health and nutrition. Sometimes the name of the transfer scheme itself signals the existence of an implicit contract between provider and recipient as to how the resources are expected to be used. Examples of this 'soft conditionality' implemented through the labeling are again the Child Grants Program in Lesotho and the *Tayssir* program in Morocco, a cash transfer program aimed at increasing the rural primary school completion rate with two main components: a 'hard' conditional component in which cash transfers are paid conditional on attendance or enrollment, and a 'labeled' unconditional component in which cash transfer are explicitly tied to an educational goal but without requirements on attendance or enrollment.

This paper focuses on the role of 'soft conditionality', implemented through both 'labeling' and 'messaging', to evaluate the effect of the Child Grants Program in Lesotho on household total consumption, food consumption, food security for children, schooling related expenses and school enrollment. As with many programs in Sub-Saharan Africa (SSA), the transfers are paid without imposing any kind of explicit conditionality. However, in practice recipients received at each payment round at the pay point a clear message that the *cash transfer should be spent on the interest and needs of children*. The clear 'messaging', evidenced also by a qualitative study (OPM, 2014), is the key feature of the program: it turned out to be a strong means to achieve the goals of improving consumption, education-related expenses, and food security for children.

Under standard models of decision-making, such 'soft conditionality' should have no bearing on how the money is spent - the cash transfers should be fully fungible with other income sources, and the program should lead to an income effect, but not necessarily behavioral change. However, a large body of empirical evidence reports relationships between income sources and the resulting behavioral response (for surveys, see Thaler, 1990; Fraker, 1990; Haveman and Wolfe, 1995). There are a number of explanations for why 'soft conditionality' implemented through 'labeling' and 'messaging' is an effective instrument to reaching the desired outcomes of the programs. The behavioral economics literature suggests that 'labeling' the additional source of income and 'messaging' on the desired use of the additional income could matter if they facilitate mental accounting (Thaler, 1990): beneficiaries may consider the cash transfers as entering into a mental account specifically addressed to the improvement of education, nutrition and health of children, not fungible with other accounts. Another explanation, not alternative to the previous one, is the important role of social sanctioning in contexts such as Lesotho and SSA in general. The communities in which the program has been

implemented were exerting close scrutiny over how cash transfers were used through structures created *ad hoc* by the program – the village assistance committees – and the traditional structures – village chiefs. This kind of social pressure can be very binding, particularly given the fact that selection into the program is partly associated with a community based targeting process. The direct involvement of the community on the selection of beneficiary households determined a credible threat of being excluded from the program if not complying with the expected behavior.

Few studies investigate the role of 'labeling' and 'messaging' in consumption and educational outcomes. Benhassine et al. (2015) use data from the Tayssir program in Morocco to estimate the impact of the 'labeled' cash transfer component, which consisted of small cash transfers made to fathers of school-aged children in poor rural communities, not conditional on school attendance but explicitly labeled as an education support program. They find evidence of large gains in school participation. Moreover, their analysis shows that adding 'hard conditionality' made almost no difference in their context. On the contrary, Edmons (2002) investigates the effect of labeling on consumption in the context of a child benefit in Slovenia but finds no evidence for it.

Our contribution to the existing literature is three-fold. First, our paper assesses the effectiveness of CGP in positively affecting behaviors that are meant to be influenced by the conditionality. Contributions on the role of social protection programs in SSA is rapidly growing as more data on randomized control trials become available (Davis et al. 2012). We add to this new collection of evidence emerging from SSA by evaluating Lesotho's CGP. Second, the paper investigates the specific role of 'soft conditionality' in affecting the desired outcomes, the improvement of the well-being of children and schooling. Unfortunately, our identification strategy cannot be based on an experimental design, since there is no heterogeneity in the implementation of two key components of the 'soft conditionality', the labeling and messaging. The message that the cash transfer should serve for the improvement of the well-being of children was spread equally in all community councils and 98 percent of the treated households declared to be aware of the main use of the transfers.

Nevertheless we adopt two intuitive empirical strategies that allow us to investigate whether the CGP has changed the preferences of households in favor of outcomes that improve the well-being of children. First, we test the hypothesis that households spend transfer income differently from earned income. Following Handa et al. (2009), we compare the marginal

propensity to spend out of transfer to the marginal propensity to spend out of income. We expect that if program 'soft conditionality' is binding, and transfer income is used to support children, then transfer income will be spent at a higher rate on goods such as education and clothing relative to general income. Second, we compare standard difference-in-difference (DID) program effects with the ex-ante expected effects given baseline expenditure elasticities to test whether the program simply moves households along their total expenditure Engel curve or in fact shifts that curve, suggesting a behavioral change in favor of children well-being induced by the messaging. With the analysis of the specific role of 'soft conditionality', the paper also partially contributes to the debate on pros and cons of CCTs versus UCTs because it aims to provide evidence of the role of "soft conditionality" in reaching desired outcomes.

Finally, the paper contributes to the literature on gender differences on the impact of cash transfers programs focusing on the gender of household head as determinant of both changes in behaviors and role of 'soft conditionality'.[2] Here we do not refer to models of household resources allocation *á la* Bourguignon and Chiappori (1998) and Browning et al. (1994). Indeed, in our sample, female headed households, which represents 50 per cent of the total sample, have different characteristics than the male ones. The large majority of female household heads are widows, therefore there is no bargaining taking place within the family.[3] We are interested in testing the hypothesis that female household heads are more sensitive to 'labeling' and 'messaging' than male counterparts in meeting the desired use of the cash transfers.

The rest of the paper is organized as follows. Section 2 describes the program, data collection and the characteristics of the evaluation sample. Section 3 presents the estimation methods and the main results. Finally, section 4 concludes.

[2] Several studies directly test whether unearned income in the form of transfer is spent differently by males and females. Using data from South-Africa, Duflo (2003) shows that pension received by women had a large impact on the anthropometric status of girls, specifically on granddaughters. In contrast, when pension is received by men, no effect is found on the nutritional status of children. There is some evidence that women have different preferences over expenditures from men, including expenditures on food (Hoddinott and Haddad, 1995; Shady and Rosero, 2008). Moreover, qualitative studies conducted in *Progresa* communities in Mexico are consistent with the hypothesis that women are more likely to spend income on health and education relative to men (Adato et al. 2000). Asfaw et al. (2014) find that an unconditional cash transfers program in Kenya had larger and positive impact on female-headed household compared to male-headed households. One of few exception is Benassine et al. (2015) which find no differences between cash transfers paid out to fathers and to mothers. Additional contributions are reviewed by Yoong, Rabinovic and Diepeveen (2012).

[3] Eighty-five per cent of the female household heads are widows, two per cent are married, and the remaining thirteen per cent are single mothers. The characteristics of male household heads are completely different: nine per cent of them are widowers, eighty-three per cent are married, and the remaining eight per cent are single father.

2. Background and experimental design

2.1 Description of the program, targeting and data collection

The Kingdom of Lesotho ranks 161 out of 188 countries on the United Nations Human Development Index. Gross domestic product has grown considerably in the past two decades, at an average annual 3.9 percent rate (World Bank, 2014). However, agriculture has lagged behind other sectors and about 90 percent of farmers depend on subsistence agriculture for their livelihoods. Further, between 10 and 30 percent of the population suffers from food insecurity (Ministry of Health and Social Welfare, 2009). Despite economic growth, poverty rates remain high: 57 percent of the population are still estimated to live below the basic needs poverty line of US$1.08 per day, and 34 percent below the food poverty line of US$0.61 per day. HIV/AIDS prevalence in Lesotho is estimated to be the second highest in the world. The epidemic has also left behind over 300,000 orphans.

In response to the challenges of poverty, vulnerability and social exclusion, the Government of Lesotho indicated in the National Strategic Development Plan 2012-2017 its commitment to promote social protection. An important component of the country's social protection response is the Child Grants Program (CGP). At the time of evaluation the CGP was implemented in ten community councils spread across five districts (Berea, Leribe, Mafeteng, Maseru and Qacha's Nek).[4] Initially the CGP provided a transfer of M360 (USD 36) every quarter to poor and vulnerable households selected through a combination of proxy means testing (PMT) and community validation. As of April, 2013 the payment was adjusted to take into account the family size as follows: 1-2 household members (M360), 3-4 members (M600) and 5 and above members (M750) per quarter. The amount of cash transfers was equivalent to 17% of the beneficiary average baseline expenditure.

The initial stated objective of the CGP is to "provide a social protection system through regular and predictable cash transfers to families living with orphans or vulnerable children (OVCs) in order to encourage, fostering and retention of OVCs within their families and communities, and to promote their human capital development" (Ayala Consulting, 2011). [5] In

[4] As of August 2016, the CGP is provided to 26,681 beneficiary households in 36 community councils across the ten districts of the country.

[5] OVC are defined as household residents between zero and seventeen years old with at least one deceased parent, or a parent who is chronically ill, or whose main caregiver is chronically ill.

practice, due to operational issues around identifying OVCs, the program focused on poor households with at least one child (under seventeen years) as a more appropriate targeting criterion. The program is currently managed and financed by the Ministry of Social Development, though during the pilot phase considered in this study the program received financial support from the European Union and the technical support from the United Nations Children's Fund (UNICEF)-Lesotho.

The Child Grants Program was designed and implemented in three phases. Phase 1 started in October 2009/April 2010 in three community councils, reaching about 1,250 households. The pilot was expanded in early 2010 under phase 1 to include three additional councils and then again under phase 2, covering an additional 3,400 households. Scale up during phase 3 was used to implement an impact evaluation using a randomized control trial design. First, in each community council, public lotteries randomly selected half of all the electoral divisions (EDs) into the group of cash transfers recipients, the so called treatment EDs. The other half, excluded from the disbursement of the payments, constitute the control EDs.[6] Second, in both treatment and control locations, targeting of the eligible and non-eligible households was carried out according to a combination of proxy means testing (PMT) and community validation. Household information was collected through a community-wide census following a community mobilization event, where households were informed about the program. The collected information was used to create the National Information System for Social Assistance (NISSA), a repository of household socio-economic information intended to be used for future social assistance programs by the Government of Lesotho. The PMT predicts the likelihood of a household having a certain level of consumption expenditure (used as an indicator of poverty) based on a number of indicators of wealth such as dwelling conditions, household assets and other household socio-economic characteristics. Households were categorized into five distinct groups: ultra-poor, very poor, poor, less poor and better off. The community validation exercise was completely independent from the PMT and only households categorized as "ultra-poor" or "very-poor" by the PMT and selected by members of their community as being the "poorest of the poor", with at least one child 0-17 years old, were defined eligible for the program. This procedure was adopted in order to limit inclusion errors as much as possible. After selection and notification through printed certificates, households were enrolled in the program in July and August 2011 and the first payments started in September 2011. Further details on the

[6] The total number of EDs were 96, a number which ensures that the randomization was done across a sufficient number of clusters.

program, targeting procedure, randomization and survey design can be found in Pellerano et al. (2012).

Beneficiary and non-beneficiary households, both eligible and non-eligible were interviewed at baseline before the first payment, between June and August 2011, and were tracked and interviewed again at follow-up, between June and August 2013. The two rounds of the survey took place at the same time of the year to avoid seasonality bias.

2.2 'Messaging': how did it work in practice

The CGP was introduced with the objective of improving the living standards of children. As a result, beneficiary households were reminded at every payment date that the money was meant for the welfare of their children and to ensure they had enough food, adequate clothing and shoes. There were also a strong emphasis on education, particularly on school uniforms. They were also reminded that the money was not for meeting their own needs or for purchasing household items and furniture. All the CGP recipients interviewed during the follow-up survey report having received instructions at the pay point to spend the money on children.

A qualitative study reports that CGP program officials provided regular and consistent messages to beneficiaries on the purpose and use of the CGP at pay points (OPM, 2014):

"*We are told by the social workers that we must buy food, clothes and school needs for our children, not to buy household furniture. We are also told that there are people who monitor how the money is being spent*" (beneficiary in Mefeteng district).

"[*We are told that] the children should look in a manner that shows they are taken care of*" (beneficiary in Leribe district).

The message was further reinforced by the oversighting of community members to make sure the beneficiaries did not 'misspend' the transfer. Qualitative evidence suggest that the oversight provided by fellow community members arose from dissatisfaction with not being selected and wanting to make sure that at least those receiving the money would use it in the way it 'should' be spent (OPM, 2014). Moreover, efforts were made to encourage families to inform the children of the CGP's purpose, to increase children's awareness of their entitlement and right.

2.3 Characteristics of evaluation sample, attrition and balance

The baseline survey comprised 3,054 households roughly equally distributed between treatment and control areas and across eligible and non-eligible households. Table 1 reports the baseline sample size, by population group. Due to budgetary restrictions, in the follow-up survey the sample size of non-eligible households was reduced to roughly half of the baseline survey, leading to an overall follow-up sample of about 2,300 households (1,484 eligible households and 800 non-eligible). The follow-up survey aimed to re-interview the households with the same children present in the baseline study (the objective was to "*follow the children*"). Sample attrition made this panel unbalanced. The issue of sample attrition was tackled in different ways for eligible and non-eligible households. For the former group, a tracking protocol was established to regulate how children in households that moved outside their original community should have been followed. In particular, the protocol established the inclusion into the study sample for those households that moved to a district capital in one of the regions of the study or the capital city Maseru, or moved to a location within thirty minutes or ten kilometers distance from the village where the household was originally sampled. For the latter group, replacement took place, i.e. households that moved outside their original community were replaced by new households. Overall 2,150 of the 3,054 households interviewed at baseline were tracked in the follow-up study, which represents a number of households lower than the target follow-up sample (2,300). Sample attrition for the overall sample was therefore 6% (150 over 2,300 households not tracked at follow-up). Further analysis shows that there were systematic differences in the non-response to the follow-up survey between treatment and control groups (respectively, 12% and 8%). To address these issues and obtain unbiased estimates of the impact of the program, the sampling weights have been adjusted for selective non-response, by calculating the probability of households being retained in the sample on the basis of key household characteristics at baseline.

Statistical test of mean differences are performed to compare baseline control and treatment groups. We find that the randomization was accurate. Treatment and control households are comparable across household characteristics (with the exception of the number of children aged 0-5 and the number of female adults aged 18-59 that are higher in the treatment group), poverty indicators, household assets (with the exception of the proportion of households that own pigs, which is higher in control group), and community level indicators. Full details on the characteristics of evaluation sample, attrition and balance are in Pellerano et al. 2014). This paper is based exclusively on data from panel households that were observed both at baseline

and follow-up. Table 2 shows the distribution of the sample used for the analysis across treatment and control areas, and eligible and non-eligible status.

3. Empirical analysis

We conduct the analysis in three steps. First, we adopt a difference-in-difference (DID) approach to estimate the direct impact on CGP on the outcomes that are meant to be directly affected by the 'soft conditionality', namely i.e. household expenditure, food security (for adults and children), schooling related expenses and school enrollment (section 3.1). Second, to assess the role of 'soft conditionality', we "unpack" the impact of the cash transfers from the impact of conditions softly imposed through the messaging in affecting the outcomes. We adopt a direct test of the hypothesis that households spend transfer income differently from earned income. In particular, we estimate the 'substitution' (behavioral change) and 'income effect' through a comparison of the marginal propensity to consume out of general income and out of cash transfers. A marginal propensity to consume out of transfer significantly greater than the marginal propensity to consume out of general income would suggest that 'soft conditionality' plays a role (section 3.2). Third, we further investigate the role of conditionality studying whether the CGP changed the preferences of households in terms of their consumption behavior, inducing relative greater expenditure on clothing for children and school-related expenses. We compare DID effects with ex-ante expected effects given baseline expenditure elasticities. If the program simply moves households along their total expenditure Engel curve (no behavioral change), the ex-ante expected behavior should line up with the ex-post actual response of households to the program. On the contrary, if the program lead to a shift of the Engel curve (behavioral change) the program ex-ante expected behavior in matters related to the conditionality should be underestimated with respect to the actual impact (section 3.3).

3.1 Difference in Difference approach

The framework for the basic analysis of the effect of CGP is based on a comparison of program beneficiaries with a group of non-beneficiaries serving as controls, all interviewed before the program began and again two years later, adopting a difference-in-difference approach (DID).

To estimate the potential impact of the program on the variables that are meant to be directly affected by the conditionality, or on any other variable, one would like to observe average outcomes in treatment areas both with and without the program. The difference between the two would be entirely attributable to the program, and the parameter of interest, Δ, would be estimated as

$$(1)\quad \Delta = E(Y_{1,A}-Y_{0,A}|T=1)$$

Where $Y_{j,k}$ is the outcome of interest and the two subscripts denote whether a household lives in a treated area ($j=1$) or not ($j=0$) and whether the observation is collected before receiving the cash transfers ($k=B$) or after ($k=A$). T equal to 1 (0) denotes treatment (control) areas and E denotes the expected value. The problem with this approach is that it is impossible to observe the outcomes of interest without treatment in treatment areas and therefore it is impossible to compute the second term on the right-hand side of *(1)*. The approach to this problem extensively used in the literature is to use control areas to estimate the counterfactual. By comparing outcomes between treatment and control groups, the average impacts of the cash transfer program can be estimated under two weak assumptions. The first assumption states that, in the absence of the program, there are common time effects across treatment and control areas, i.e.

$$(2)\ E(Y_{0,A}-Y_{0,B}|T=1)= E(Y_{0,A}-Y_{0,B}|T=0)$$

The assumption specifies that control households must evolve from the baseline to the follow-up period in the same way treatments would have done had they not been treated. Moreover, it implies that treatment and control households may be affected in the same way by macro shocks or by any other policy implemented simultaneously. This assumption allows us to estimate the effects of the program on the outcomes as

$$(3)\ \Delta = E(Y_{1,A}|T=1,X)-E(Y_{0,B}|T=1,X)+ E(Y_{0,B}|T=1,X)-E(Y_{0,A}|T=1,X)$$
$$= \underbrace{[E(Y_{1,A}|T=1,X)-E(Y_{0,A}|T=0,X)]}_{A'} - \underbrace{[E(Y_{0,B}|T=1,X)-E(Y_{0,B}|T=0,X)]}_{B'}$$

where X is the set of observable covariates at household and community level that are likely to affect the outcome variables.

The second assumption underlying this estimator is that, if there are differences in the outcome variables across treatment and control areas due to unobservable factors, these are fixed over time. By netting *B'* out of *A'*, one obtains the effect of the program on the outcome variables.

Both assumption (2) and (3) must hold in order for the DID estimation of the program impact to be unbiased.

Equation (4) represents the regression equivalent of DID:

(4)

$$Y_{i,t} = \beta_0 + \beta_1 d\_CGP_{i,t} + \beta_2 d\_2013_i + \beta_3 (d\_CGP_{i,t} * d\_2013_i) + \sum \beta X_i + \mu_{i,t}$$

Y represents the outcome of interest; *d_CGP* is a dummy equal to 1 if household received the treatment; *d_2013* is a dummy equal to 0 (1) if the observation is a baseline (follow up) one; *d_CGP*d_2013* is the interaction between the intervention and the time dummies, *X* is the set of household and community baseline characteristics which includes household demographic composition, education, age and marital status of the household head, community prices for individual items, community wages and community shocks (the full set of covariates is reported in Table A2 in the appendix). $\mu_{i,t}$ is an error term. As for the coefficients, we are mainly interested in estimating β_3 which is the double difference estimator capturing the treatment effect.

Since we are also interested in investigating whether the impact of CGP is different in female headed households with respect to male headed households, we estimated equation (5) which represents the regression equivalent of a triple difference in outcomes (treatment vs control, follow-up versus control, female headed households versus male headed households).

(5)

$$\begin{aligned} Y_{i,t} = {} & \gamma_0 + \gamma_1 d\_CGP_{i,t} + \gamma_2 d\_2013_i + \gamma_3 (d\_CGP_{i,t} \cdot d\_2013_i) + \\ & + \delta_0 d\_femhd_i + \delta_1 (d\_femhd_i \cdot d\_CGP_{i,t}) + \delta_2 (d\_femhd_i \cdot d\_2013_i) + \\ & + \delta_3 (d\_femhd_i \cdot d\_CGP_{i,t} \cdot d\_2013_i) + \sum \beta X_i + \mu_{i,t} \end{aligned}$$

In this case we are mainly interested in estimating γ_3 which is the treatment effect for male headed households and the sum of γ_3 and δ_3 which represents the treatment effect for female headed households.

We identify the intention-to-treat (ITT) effect.[7] The average effect of the treatment on the actual treated (ATT) is also estimated but, since the results are fully consistent to those indicated by the ITT models and the magnitudes of the effects are only marginally higher, we decided to report only the ITT models estimates. Results based on the ATT models are available upon request.

3.1.1 Impact of CGP on total expenditure, food and non-food expenditure

In Table 3, we report our estimates of the impact of the program on total expenditure, food and non-food expenditure. For these outcome variables, and also for the others shown in the following tables, we present the results in two different columns: the first reports the difference in difference estimates for the whole sample without considering the potential heterogeneous impacts by gender of the household heads, i.e. β_3 from equation (4); the second column reports the difference-in-difference estimates of the impact of CGP for male and female household heads, respectively, γ_3 and ($\gamma_3 + \delta_3$). In all estimates we control for a large set of household and community characteristics listed in the appendix (Table A2). Moreover, estimates are adjusted using sampling weights by calculating the probability of households being retained in the sample on the basis of key household characteristics at baseline, and selective non response (for details see Pellerano et al. 2014). Moreover, the significance testing accounts for clustering of standard errors due to sampling design.

Table 3 shows that the CGP had a positive effect on total and food expenditure, especially in households with female household head, and did not affect expenditure on non-food items. While for food expenditure we are not able to disentangle which members of the family were enjoying more food available, we believe that this will have positive effect on food security of all members, including children. This result is indeed reflected on several indicators of food security in Table 4. We consider whether the household incurred in food shortage, the period spent in extreme shortage, whether any member of the household (adults and children separately) had access to smaller or fewer meals, and went to sleep hungry in the previous week. The results indicate that CGP significantly contribute to the reduction of the months of extreme food shortage, independently on the household head gender, and a significant increase in the number of meals for both adults and children, and a reduction of both adults and children that went to bed hungry in households with female household head.

[7] Unsurprisingly, the degree of compliance for picking up money was very high (93% according to the survey response, 96% according to administrative records).

As mentioned above, expenditure on non-food items was not affected by CGP. However, if we distinguish different items of expenditure, we find a heterogeneous impact of the CGP. While it did not affect expenditure for adults' clothing, the impact for children's clothing is positive and statistically significant for both female and male headed households (see Table 5). Furthermore, no impact is detected on expenditure on health, fuel and housing (see Table 6). The results on expenditure on health deserve a particular note. Public primary healthcare in Lesotho is officially free and the cash transfer was rarely used to pay for formal healthcare. In some cases participants in qualitative research (OPM, 2014) reported that recipients felt better able to purchase over-the-counter medicine (such as those available from small shops without prescription), but this was not detected in the quantitative survey.

3.1.2 Impact on CGP on school-related expenditure and school enrollment

Table 7 reports the impact of CGP on expenditure on education (total and per pupil 6-12, 13-19) as well as the impact on several school-related expenditure, including i.e. school fees, exams fees, textbooks and photocopies, stationary and school bags, uniform and school shoes. The results show a large and highly significant effect on expenditure in education. For pupils 6-12 the impact is positive and significant for both male and female headed households, while for pupils 13-19 the impact is significant only for male headed households. When we disentangle by item groups, the strongest impact is detected for expenditure on school uniforms and shoes, for both female and male headed households. There are two factors that explain this strong results: first, qualitative analysis (OPM, 2014) documents that expenditure on clothing was strongly encouraged at pay point as an example of expenditure in favor of the wellbeing of children. Moreover, expenditure on new uniforms and shoes is also a highly 'visible' way to spend money for children and this evidence would contribute to support the hypothesis that 'soft-conditionality' may work because of social pressure.

Table 8 shows the results for school enrolment. We consider whether the child is currently enrolled in any educational grade, distinguishing between boys and girls education[8]. The results are striking and are consistent with Sebastian et al. (2016)[9]. CGP positively and significantly affect school enrolment of boys but not of girls, especially in female headed households. Several factors can explain these findings. Poor households tend to invest more on education of male

[8] We also analyzed the impact of CGP on school enrollment by age groups. The results are not reported here but are available upon request.

[9] Using the same data but adopting a different analytical framework, Sebastian et al. (2016) specifically looks at impact of CGP on children schooling, labor and time use, with a particular focus on differentiated impacts by gender and household structure.

children because the returns to education of girls for the household as a whole is much lower than the returns to education of boys: in Lesotho, as in many other developing countries, girls become part of the husband family and do not contribute anymore to the maintenance of the original family. This argument is even stronger for female headed households, mostly widow and unmarried, for which education of boys represents their insurance for the old age.

3.2 "Unpacking" the role of program's conditionality

Focusing only on expenditure items, we employ an intuitively appealing approach, proposed by Breunig and Dasgupta, (2005) and Handa et al. (2009), to test whether 'soft conditionality' is playing a role in affecting behaviors of beneficiary households. If conditionality is binding, program transfers will exert an income and substitution effect on household spending behavior, while general income only exerts an income effect on such behavior. If the substitution effect is big, the marginal propensity to consume (MPC) out of transfer income for items related to children' wellbeing will be larger than the MPC out of general income; if the substitution effect is small or zero, then the program only exerts an income effect and the two MPCs will be statistically equal.

We estimate the following equation:

$$(6) \qquad Y_{i,t} = \beta_0 + \beta_1 CGP\_value_{i,t} + \beta_2 income_{i,t} + \beta_3 d\_2013_i + \sum \beta X_i + \mu_{i,t}$$

Where *Y* represents the logarithm of annual household expenditure of the i_{th} household (either total expenditure or expenditure on each of the other items), food security or school enrollment. *CGP_value* is the logarithm of annual transfers from administrative data. The variable *income* is the logarithm of the annual monetary income (not including the cash transfers). *X* is a vector representing the same set of control variables as in (4) and (5), and *u* is the error term. Expenditure, monetary income, and transfer amounts are logged to normalize values and account for skewed distributions. Therefore, our equations for expenditure items are estimated in double logarithmic form: our hypothesis test translates into a test of the equality of elasticities of transfers and general income. β_1 represents the MPC out of transfer income and β_2 represents the MPC out of general income.

To determine if the impact of a CGP maloti is different from a monetary income maloti, we test the following null and alternative hypothesis:

(7) $H_0 : \beta_1 = \beta_2 \quad H_a : \beta_1 \neq \beta_2$

Soft conditionality plays a role if, for outcome variables related to the conditionality, the MPC out of transfer income (β_1) is significantly greater than the MPC out of general income (β_2). In this case the null hypothesis will be rejected in favour of the alternative and transfer income plays both a 'substitution' and an 'income' effect. We expect the substitution effect of the transfer income to be strongest for expenditure items related to the children wellbeing that should be directly affected by the conditionality, i.e. clothing and footwear for children and education.

The monetary income variable includes the following component (real annual values): wage income[10], income from livestock sales, income from livestock by-products sales, income from crop sales, net profit from non-agricultural activities, public transfers (excluding CGP transfers), private transfers, and transfers from residents and non-residents family members or friends. From this measure of monetary income an important component is missing: the value of livestock purchased. To cope with potential income underestimation and partially solve the issue of a missing component, we added the number of livestock purchased as additional control.[11] However, the inclusion of these additional control does not change significantly the main results.

3.2.1 Endogeneity of household income

The income variable included in the previous analysis presents a problem of endogeneity since income and consumption expenditure are jointly determined by the households through the allocation of time between work and leisure. Moreover, unobserved ability or tastes may determine both income and the allocation of that income to different consumption items (food, alcohol and tobacco, schooling, etc). This unobserved heterogeneity may cause bias estimates. In order to minimize this potential bias, we also estimate household fixed-effects models which allows to get rid of the fixed unobserved household level component. The fixed-effect model generate consistent estimates under the underlying assumption that the unobserved component affecting both earning capacity and expenditure decisions is fixed over time. We believe this is a plausible assumption given the two year time frame used in our analysis.

[10] Wage income is calculated as follows: "average days of work in a week" * "wage level for agricultural activities" * 52 (number of weeks per year). Gender specific wage levels are taken into account.
[11] We could not calculate the value of livestock purchases because data on prices were not available and we opted for adding the number of livestock purchased instead of imputing the prices.

3.2.2 Comparison of MPCs

Tables 9, 10 and 11 present summary results of the spending responses out of transfer and general income. The OLS results and the household fixed effect model results are reported, respectively, on the left-hand and right-hand columns. The analysis for total expenditure, food expenditure and non-food expenditure is reported in Table 8. For these broad outcome variables, the differences between the two propensities to consume on non-food items are not statistically significant, while for food items the marginal propensity to consume out of income is significantly greater than the marginal propensity to consume out of transfer. Both results seem to suggest that substitution effect is not taking place. However, if we disentangle expenditure in non-food items in the different components, the results are more heterogeneous. Table 9 shows the MPCs comparison for expenditure on clothing (for male and female adults and children), health, fuel, housing and other. The results are striking: MPCs out of general income and out of transfer on clothing for adults, health, fuel, housing and other are not statistically significant. On the contrary, the MPC out of transfer on clothing for children is positive and significantly greater than the MPC out of income, meaning that, in this case, both a substitution and an income effect are taking place. These results hold for both male and female headed households. We get similar results for school-related expenditure. Table 10 reports the comparison between MPCs for expenditure on total education and per different items (school fees, exam fees, expenditure on uniforms and school shoes, school maintenance and expenditure for stationery and books). These results show that a substitution effect (behavioral change) is taking place for expenditure on education, especially for expenditure on school uniforms and shoes.

3.3 Testing for elasticity changes

The findings presented in the previous section suggest that transfer income is spent differently than general income for items that were meant to be affected by the 'soft-conditionality'. Indeed, for clothing for children, education and especially for expenditure in school uniforms and shoes both an income and substitution effects (behavioral change) are taking place. In this section, focusing only on expenditure items, we propose another test for potential behavioral changes induced by the program. Following The Kenya CT-OVC Evaluation Team (2012), we unpack how the CGP has affected behavior by using standard

demand theory to predict how the program ought to impact spending in favor of children, based on pre-program expenditure elasticities. Our approach consists on deriving theoretically consistent expenditure elasticities from baseline (pre-program) and use these to predict household responses to the program. The rational of this kind of analysis is the following: if the program simply moves households along their total expenditure Engel curve, the ex-ante expected behavior should line up with the ex-post actual response of households to the program. If this occurs, no behavioral change is taking place and the 'soft conditionality' does not play any role. On the contrary, if the ex-post actual response of households to the program it is greater than the ex-ante expected one, behavioral changes are taking place and 'soft conditionality' actually plays a role.

The principal analytical tool we use to build the baseline elasticities is the Engel curve, which relates budget shares devoted to various spending groups to total household expenditures and other households characteristics. We estimate the following specification, commonly known as the Working-Leser functional form, for which applications can be found in Deaton and Muellbauer (1980), Handa (1996) and The Kenya CT-OVC Evaluation Team (2012):

$$(8) \qquad w_i = \alpha + \beta_1 X + \beta_2 \ln(EXP) + \beta_3 CGP + \varepsilon_i$$

Where w_i is the budget share for commodity *i*, *EXP* is household total consumption expenditure, *CGP* is a dummy variable equal to one if the household is in the treatment group, and *X* is the same vector of control variable used in equation (4).

Using equation (8), the marginal effect on the budget share of a change in total household expenditure is given by equation (9), while the total elasticity expenditure can be derived using the formula in equation (10) (Deaton et al., 1989):

$$(9) \qquad \frac{\partial w_i}{\partial \ln(EXP)} = \beta_2$$

$$(10) \qquad E_i = 1 + \frac{\left[\dfrac{\partial w_i}{\partial \ln(EXP)}\right]}{w_i} = 1 + \frac{\beta_2}{w_1}$$

Table 12 shows the results of equation (8) for the following expenditure items: food, clothing (for male and female adults and children), education (uniform as special educational expenditure item), fuel, health, housing and other. The last row provides the calculated

elasticities at the mean share. Panel A shows the results for the whole sample, while panel B and C, respectively, for male and female headed households. The elasticities suggest that fuel and housing are basic needs (elasticity less than one), while health, education and clothing are luxuries (elasticity greater than one). Food has unit elasticity, a finding that can be explained taking into account the fact that our sample is largely composed by agricultural households that are partially able to meet their nutritional needs with home production.

We can now predict the impact of the program on expenditure patterns. The CGP provides transfers that correspond to 17 per cent of the beneficiaries average baseline total consumption expenditure. Using the elasticity estimates in Table 12, we can predict the percentage change in expenditure (at the mean) for each expenditure item considered. These are the ex-ante predicted program impacts assuming no behavioral changes. Table 13 summarizes this exercise. For example, in column 5 in panel A we estimated an elasticity for education of 1.121 at baseline. This implies that a 17 per cent increase in total expenditure will result in 19.057 per cent increase in expenditure for education (17*1.121=19.057), which corresponds to 4.91 maloti when evaluated at the mean level of expenditure at baseline, i.e. 25.75 maloti (mean level baseline expenditure*per cent increase in education=25.75*19.057/100=4.91). In contrast, the actual impact of the CGP on expenditure on education is 15.94. This means that the ex-ante simulation under-predicts expenditure on education by 11.03 maloti. The impact of CGP is 11.03 maloti more than what we would expect at baseline. The difference between actual CGP impact and ex-ante simulation is even greater for male headed households (16.56 maloti). Overall, the results presented in Table 13 suggest that there are some important differences between actual program effects and what we would expect given baseline preferences of targeted households. Indeed, looking across the other household items groups, we see that actual program impacts are lower than expected for food, clothing for male and female adults, health, fuel and housing and other expenditure, but they are higher than expected for clothing for children, education and expenditure for school uniforms and shoes, suggesting a behavioral change in favor of children wellbeing induced by the program.

4. Conclusions

This paper uses data collected from a two years randomized experimental design impact evaluation (2011-2013) to analyze the effects of the CGP in Lesotho. We focus on the role of ‘soft conditionality’ implemented through both ‘labeling’ and ‘messaging’ in affecting

outcomes that should be influenced by the implicit conditionality. It aims to contribute to the literature on the effectiveness/appropriateness of explicit/implicit/lack (of) conditionality in Sub-Saharan Africa. The difference-in-difference estimates show that the program had a positive impact on food expenditure, expenditure for clothing (especially for children), school-specific expenditures (expenditure for maintenance and especially school uniforms and shoes), food security for adults and children and school enrollment of boys.

The main contribution of this paper is our analysis of whether the program has shifted preferences due to the 'soft conditionality' implicitly imposed by the program. Most impact studies calculate the program impact using a difference-in-difference approach, which we present here. However, we go further proposing two different approaches to test whether the program may have caused preferences to shift in favor of some goods that are meant to be affected by the 'soft conditionality'.

First, we test whether households spent transfer income differently from earned income comparing the marginal propensity to consume (MPC) out of transfers with the MPC out of general income on goods that ought to be affected by the conditionality. This analysis shows that 'soft conditionality' did play a role on outcomes most directly associated with the labelling of the program (a "child" grant) as well as with the program messaging (heavily focused on schooling). The MPC out of transfer is indeed positive and significantly larger than the MPC out of general income for expenses on clothing and footwear for children and expenditure on education, especially on school uniforms and shoes.

Second, we further investigate the role of conditionality studying whether CGP changed the preferences of households in terms of their consumption behavior, inducing relative greater expenditure on clothing for children and school-related expenses. We compare DID effects with ex-ante expected effects given baseline expenditure elasticities. If the program simply moves households along their total expenditure Engel curve (no behavioral change), the ex-ante expected behavior should line up with the ex-post actual response of households to the program. Our findings show that the ex-post actual program effects are higher than the ex-ante expected ones for clothing for children, education and expenditure for school uniforms and shoes, suggesting a behavioral change in favor of children wellbeing induced by the program.

Overall, our findings provide support to the effectiveness of 'soft conditionality'.

Furthermore, we explore potential gender differences on the impact of CGP comparing male and female headed households. Our findings do not provide support for such gender differences.

A key characteristic of the program may have affected the effectiveness of program messaging and labelling: the strong involvement of the community in program implementation and strong community oversight systems. A community validation exercise was carried out jointly with proxy mean testing to select CGP beneficiary households. Village Assistance Committees were also constituted to oversee the smooth implementation of the programme, often with direct or indirect involvement of traditional authorities. This potential link between community monitoring and surveillance over the "good" use of transfers, and the role of "high-stature" community members in selection of beneficiaries (and possibly in upcoming recertification processes) may create a fear of sanction that resembles hard conditionalities.

To conclude, the results suggest two main policy implications. First, social programs can incentivize the achievement of the desired goals of the program through 'labeling' and 'messaging', without necessarily imposing any explicit conditionality. Soft-conditioned programs tend to be administratively simpler hence less costly to implement for the government, they also have reduced transactional costs for beneficiaries, due to the lack of an explicit conditionality monitoring system. Second, programs adopting a soft-conditionality approach should carefully consider how to tailor the communication strategy to reflect the full array of program objectives. A too narrowly specified message, if effectively conveyed to the beneficiaries and enforced through social monitoring, may limit the potential impact of the program. In the case of Lesotho the message was focused on school expenditure and was strictly adhered to, but may have hindered impacts on other areas, such as access to health or livelihoods diversification. Programs should adopt messaging and labelling approaches that empowers beneficiaries in exercising choice, including embarking in higher-risk investment that may lead to higher human capital or productivity gains in the long run.

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Tables

Table 1: Beneficiary status at baseline

Beneficiary Status	Area		Total
	Treatment	Control	
Eligible for CGP	747 (*98%*)	739 (*97%*)	1,486 (*98%*)
Non Eligible for CGP	779 (*100%*)	789 (*99%*)	1,568 (*99%*)
Total	1,526 (*99%*)	1,528 (*98%*)	3,054 (*98%*)

Table 2: Beneficiary status in the paneled sample

Beneficiary Status	Area		Total
	Treatment	Control	
Eligible for CGP	706	647	1,353
Non Eligible for CGP	396	401	797
Total	1,102	1,048	2,150

Table 3: Impact of CGP on monthly expenditure – Maloti, real values (2013 prices)

	Total Expenditure		Food Expenditure		Non-food Expenditure	
Household level						
DID (β_3)	75.795		64.186*		14.56	
	(1.57)		(1.66)		(0.66)	
DID male hh (γ_3)		11.167		4.805		11.157
		(0.18)		(0.1)		(0.4)
DID female hh ($\gamma_3+\delta_3$)		146.980**		130.600***		17.180
		(2.76)		(3.00)		(0.73)
Per capita						
DID (β_3)	18.155*		13.981*		4.986	
	(1.68)		(1.67)		(0.90)	
DID male hh (γ_3)		14.766		6.192		5.139
		(1.25)		(0.64)		(0.65)
DID female hh ($\gamma_3+\delta_3$)		20.865*		22.510**		4.319
		(1.76)		(1.97)		(0.66)
Observations			2,701			

Notes: Robust t-statistics clustered at the community level are in brackets. Sample weights adjusted for selective non-response have been used. ***, **, * indicate significance at 1, 5 and 10%. All regressions include the set of control variables listed in Table A2.

Table 4: Impact of CGP on various indicators of food security

	Food shortage		Av. months extreme shortage		Smaller Meals Adults		Smaller Meals Children	
DID (β_3)	-0.046		-1.765***		-0.018		-0.065	
	(-1.43)		(-4.45)		(-0.39)		(-1.38)	
DID male hh (γ_3)		-0.06		-1.546***		-0.006		-0.035
		(-1.31)		(-2.93)		(-0.10)		(-0.59)
DID female hh ($\gamma_3+\delta_3$)		-0.029		-1.989***		-0.032		-0.082
		(-0.70)		(-3.82)		(-0.59)		(-1.39)
	Fewer Meals Adults		**Fewer Meals Children**		**Went to sleep hungry Adults**		**Went to sleep hungry Children**	
DID (β_3)	-0.058		-0.078*		-0.090**		-0.053	
	(-1.34)		(-1.65)		(-2.24)		(-1.34)	
DID male hh (γ_3)		-0.027		-0.05		-0.064		0.034
		(-0.45)		(-0.79)		(-0.98)		(0.62)
DID female hh ($\gamma_3+\delta_3$)		-0.083*		-0.095		-0.161***		-0.150***
		(-1.7)		(-1.54)		(-3.08)		(-3.00)
Observations	2,705							

Notes: Robust t-statistics clustered at the community level are in brackets. Sample weights adjusted for selective non-response have been used. ***, **, * indicate significance at 1, 5 and 10%. All regressions include the set of control variables listed in Table A2.

Table 5: Impact of CGP on monthly expenditure in clothing – Maloti, real values (2013 prices)

	Clothing							
	Total		**Men**		**Women**		**Children**	
DID (β_3)	11.207*		-1.451		-1.876		13.064***	
	(1.92)		(-1.11)		(-1.22)		(4.82)	
DID male hh (γ_3)		10.235		-2.198		-1.49		15.075***
		(1.2)		(-0.96)		(-0.69)		(4.16)
DID female hh ($\gamma_3+\delta_3$)		11.909*		-0.635		-2.291		10.528**
		(1.87)		(-0.40)		(-1.04)		(2.90)
Observations	2,701							

Notes: Robust t-statistics clustered at the community level are in brackets. Sample weights adjusted for selective non-response have been used. ***, **, * indicate significance at 1, 5 and 10%. All regressions include the set of control variables listed in Table A2. Note that expenditure on clothing and footwear does not include expenditure on school uniforms and school shoes.

Table 6: Impact of CGP on expenditure in other monthly non-food group items (excluding education) – Maloti, real values (2013 prices)

	Health	Fuel	Housing and other
DID (β3)	-0.121	-0.365	-9.977
	(-0.04)	(-0.03)	(-1.19)
DID male (γ3)	-0.369	-6.623	-8.109
	(-0.09)	(-0.48)	(-0.68)
DID female (γ3+δ3)	0.086	6.290	-12.345
	(0.02)	(0.46)	(-1.37)
Observations	2,701		

Notes: Robust t-statistics clustered at the community level are in brackets. Sample weights adjusted for selective non-response have been used. ***, **, * indicate significance at 1, 5 and 10%. All regressions include the set of control variables listed in Table A2.

Table 7: Impact of CGP on schooling-specific expenditure items – Maloti, real values (2013 prices)

	Education - total	Education - per pupil 6-12	Education per pupil 13-19
DID (β_3)	15.941**	5.729**	6.46
	(2.01)	(2.81)	(0.74)
DID male (γ_3)	21.027**	6.127*	27.203**
	(2.16)	(1.89)	(2.19)
DID female ($\gamma_3+\delta_3$)	10.01	5.316**	-11.78
	(0.96)	(2.14)	(-1.01)
Household level	**School fees for the year**	**Exam fees and other school fees**	**School maintenance**
DID (β_3)	5.102	1.163	0.550**
	(1.25)	(0.89)	(2.13)
DID male (γ_3)	10.312*	2.059	0.287
	(1.78)	(1.25)	(1.24)
DID female ($\gamma_3+\delta_3$)	-0.907	0.088	0.838*
	(-0.16)	(0.05)	(1.84)
Household level	**Text books and photocopies**	**Stationery and school bags**	**Uniform and/or school shoes**
DID (β_3)	-0.119	1.045	6.554***
	(-0.09)	(1.5)	(3.23)
DID male (γ_3)	0.488	1.712*	7.091***
	(0.24)	(1.73)	(2.97)
DID female ($\gamma_3+\delta_3$)	-0.857	0.324	5.993**
	(-0.63)	(0.34)	(2.01)
Observations	2701		

Notes: Robust t-statistics clustered at the community level are in brackets. Sample weights adjusted for selective non-response have been used. ***, **, * indicate significance at 1, 5 and 10%. All regressions include the set of control variables listed in Table A2

Table 8: Impact of CGP on school enrollment

	Currently enrolled	Currently enrolled - Boys	Currently enrolled - Girls
DID (β_3)	0.036	0.063*	0.023
	(1.48)	(1.83)	(0.69)
DID male hh (γ_3)	0.047	0.052	0.057
	(1.41)	(1.13)	(1.27)
DID female hh ($\gamma_3+\delta_3$)	0.026	0.078**	-0.016
	(0.92)	(2.02)	(-0.43)
Observations	2701		

Notes: Robust t-statistics clustered at the community level are in brackets. Sample weights adjusted for selective non-response have been used. ***, **, * indicate significance at 1, 5 and 10%. All regressions include the set of control variables listed in Table A2.

Table 9: Soft-conditionality results for household expenditure (total, food and non-food)

	OLS regression			Fixed-effect regression		
	CGP transfers (log)	**HH income (log)**	**P-value for difference**	**CGP transfers (log)**	**HH income (log)**	**P-value for difference**
Total expenditure	0.007	0.034***	0.0002***	0.016**	0.032***	0.055*
	(1.07)	(9.29)		(2.28)	(6.42)	
Total expenditue - MHH	0.004	0.033***	0.0021***	0.006	0.026**	0.112
	(0.57)	(5.85)		0.66	(2.76)	
Total expenditure - FHH	0.011*	0.035***	0.0051***	0.030***	0.036***	0.564
	(1.70)	(6.58)		(3.98)	(5.16)	
Food expenditure	-0.001	0.032***	0.000***	0.014*	0.020***	0.435
	(-0.09)	(8.35)		(1.89)	(3.5)	
Food expenditure - MHH	-0.008	0.040***	0.000***	0.001	0.020	0.211
	(-0.99)	(6.37)		(0.13)	(1.59)	
Food expenditure - FHH	0.009	0.028***	0.019**	0.026***	0.023**	0.721
	(1.35)	(4.99)		(3.43)	(2.85)	
Non-food expenditure	0.030***	0.043***	0.321	0.020*	0.057***	0.013*
	(2.95)	(6.77)		(1.78)	(7.14)	
Non-food expenditure - MHH	0.038***	0.036***	0.853	0.013	0.036**	0.203
	(3.73)	(4.24)		(0.95)	(2.98)	
Non-food expenditure - FHH	0.022	0.050***	0.1382	0.043***	0.068***	0.136
	(1.58)	(5.01)		(3.56)	(5.78)	
Observations	2,701			2,701		

Notes: ***, **, * indicate significance at 1, 5 and 10%. The MPCs have been estimated with the inclusion of the set of control variables listed in Table A2. MHH and FHH stand for Male Household Head and Female Household Head, respectively.

Table 10: Soft-conditionality results for non-food expenditure (excluding education)

	OLS regression			Fixed-effect regression		
	CGP transfers (log)	**HH income (log)**	**P-value for difference**	**CGP transfers (log)**	**HH income (log)**	**P-value for difference**
Clothing expenditure adult males	0	0.021**	0.1034	0.006	0.027**	0.234
	(0.03)	(2.46)		(0.59)	(2.41)	
Clothing expenditure adult males - MHH	0.003	0.031**	0.2016	0.001	0.046**	0.199
	(0.19)	(1.99)		(0.07)	(2.09)	
Clothing expenditure adult males - FHH	-0.007	0.012*	0.1662	0.027**	0.007	0.152
	(-0.62)	(1.99)		(2.36)	(0.9)	
Clothing expenditure adult females	-0.004	0.022***	0.083	-0.019*	0.007	0.113
	(-0.36)	(3.02)		(-1.8)	(0.8)	
Clothing expenditure adult females - MHH	0.005	0.017	0.515	-0.024	-0.003	0.395
	(0.38)	(1.54)		(-1.44)	(-0.23)	
Clothing expenditure adult females - FHH	-0.012	0.027**	0.085*	-0.021	0.003	0.354
	(-0.68)	(2.75)		(-1.26)	(0.17)	
Clothing expenditure children	0.174***	0.064***	0.0005***	0.188***	0.069***	0.002***
	(6.94)	(4.32)		(6.05)	(3.02)	
Clothing expenditure children - MHH	0.202***	0.091***	0.010***	0.193***	0.106**	0.065*
	(6.25)	(3.45)		(5.42)	(2.75)	
Clothing expenditure children - FHH	0.142***	0.035*	0.010***	0.206***	0.023	0.000***
	(4.28)	(1.93)		(5.14)	(0.86)	
Fuel expenditure	0.027	0.040***	0.5642	-0.017	0.059***	0.004***
	(1.48)	(3.15)		(-0.85)	(3.62)	
Fuel expenditure - MHH	0.026	0.041**	0.5522	-0.026	0.036	0.067*
	(1.28)	(2.71)		(-1.17)	(1.39)	
Fuel expenditure - FHH	0.031	0.045**	0.6095	0.012	0.082***	0.020**
	(1.27)	(2.55)		(0.44)	(3.66)	
Health expenditure	1.067	7.371**	0.2700	-3.523	9.563**	0.017**
	(0.26)	(2.39)		(-1.02)	(2.29)	
Health expenditure - MHH	-1.534	4.357	0.4826	-3.264	12.208**	0.029**
	(-0.23)	(1.25)		(-0.69)	(2.54)	
Health expenditure - FHH	0.408	8.045*	0.1777	-3.946	9.996	0.082*
	(0.10)	(1.84)		(-0.70)	(1.69)	
Housing and other expenditure	7.251	28.233***	0.1178	0.932	34.986***	0.018**
	(0.73)	(3.42)		(0.08)	(4.07)	
Housing and other expenditure - MHH	18.974	22.357**	0.8514	11.339	14.941	0.859
	(1.27)	(2.29)		(0.71)	(1.15)	
Housing and other expenditure - FHH	-7.040	31.701**	0.0280	0.709	44.207***	0.032**
	(-0.61)	(2.91)		(0.05)	(3.48)	
Observations		2,701			2,701	

Notes: ***, **, * indicate significance at 1, 5 and 10%. The MPCs have been estimated with the inclusion of the set of control variables listed in Table A2. MHH and FHH stand for Male Household Head and Female Household Head, respectively

Table 11: Soft-conditionality results for schooling related expenditure

	OLS regressions			Fixed-effect regression		
	CGP transfers (log)	**HH income (log)**	**P-value for difference**	**CGP transfers (log)**	**HH income (log)**	**P-value for difference**
Expenditure in Education	0.127***	0.082***	0.082*	0.174***	0.096***	0.024**
	(5.99)	(4.82)		(6.43)	(3.71)	
Expenditure in Education - MHH	0.111***	0.033	0.068*	0.167***	0.028	0.064*
	(3.44)	(1.25)		(3.68)	(0.57)	
Expenditure in Education - FHH	0.154***	0.134***	0.643	0.214***	0.092**	0.013**
	(5.09)	(5.24)		(6.64)	(2.46)	
Exp. School fees	0.036	0.041**	0.867	0.094***	0.067**	0.434
	(1.56)	(2.73)		(4.00)	(2.71)	
Exp. School fees - MHH	0.041	-0.006	0.246	0.094***	-0.008	0.037**
	(1.34)	(-0.23)		(3.15)	(-0.23)	
Exp. School fees - FHH	0.044	0.104***	0.213	0.142***	0.117**	0.678
	(1.28)	(4.15)		(3.72)	(2.7)	
Exp. Uniform/school shoes	0.162***	0.091***	0.010***	0.224***	0.092***	0.001***
	(8.09)	(4.81)		(8.88)	(3.39)	
Exp. Uniform/school shoes - MHH	0.139***	0.040*	0.009***	0.239***	0.041	0.002**
	(4.70)	(1.68)		(5.7)	(0.92)	
Exp. Uniform/school shoes - FHH	0.190***	0.126***	0.100*	0.234***	0.086**	0.013**
	(5.93)	(4.58)		(6.68)	(2.05)	
Exp. Exams fees	-0.001	0.025***	0.070*	0.007	0.035**	0.098
	(-0.10)	(3.04)		(0.53)	(2.52)	
Exp. Exams fees - MHH	0.008	0.273**	0.394	0.016	0.023	0.806
	(0.46)	(2.13)		(0.87)	(1.13)	
Exp. Exams fees - FHH	-0.005	0.022**	0.192	0.017	0.046**	0.304
	(-0.28)	(2.18)		(0.9)	(1.96)	
Exp. School maintenance	0.008	0.013**	0.574	0.019**	0.018	0.950
	(1.00)	(2.01)		(2)	(1.44)	
Exp. School maintenance - MHH	-0.002	0.008	0.403	0.006	-0.022	0.138
	(-0.22)	(0.91)		(0.45)	(-1.4)	
Exp. School maintenance - FHH	0.019*	0.018**	0.934	0.034**	0.047**	0.516
	(1.73)	(1.98)		(2.59)	(2.37)	
Exp. Stationery/school bags	0.032*	0.067	0.151	0.064**	0.068***	0.895
	(1.70)	(5.67)		(2.88)	(3.73)	
Exp. Stationery/school bags - MHH	0.033	0.042**	0.795	0.057	0.051	0.909
	(1.22)	(2.14)		(1.54)	(1.57)	
Exp. Stationery/school bags - FHH	0.038	0.094***	0.114	0.106***	0.066**	0.346
	(1.39)	(5.55)		(3.74)	(2.39)	
Observations		2701			2701	

Notes: ***, **, * indicate significance at 1, 5 and 10%. The MPCs have been estimated with the inclusion of the set of control variables listed in Table A2. MHH and FHH stand for Male Household Head and Female Household Head, respectively

Table 12: Engel curve estimates and expenditure elasticities, pooled baseline sample.

Panel A. All sample	**Food**	**Clothing Ad male**	**Clothing Ad fem**	**Clothing children**	**Education**	**Uniform**	**Health**	**Fuel**	**Housing and other**
lnEXP	0.013*	0.002	0.004***	0.006***	0.004	0.002	0.008***	-0.041***	-0.003
	(1.7)	(3.64)	(5.91)	(5.64)	(1.25)	(0.09)	(3.62)	(-7.67)	(-0.78)
Treatment	-0.04	0.001*	0.002**	0.003**	-0.002	-0.013	0.001	0.013**	0.019***
	(-4.6)	(1.89)	(2.02)	(2.44)	(-0.63)	(-0.61)	(0.34)	(2.08)	(4.02)
Constant	0.632***	-0.008*	-0.024***	-0.045***	-0.027	-0.052	-0.048*	0.413***	0.129
	(9.57)	(-1.83)	(-3.84)	(-4.7)	(-1.08)	(-0.26)	(-2.61)	(9.06)	(3.65)
Budget share at baseline	0.667	0.003	0.006	0.012	0.033	0.011	0.018	0.16	0.105
Elasticity	1.02	1.63	1.73	1.53	1.12	1.18	1.43	0.75	0.97
Panel B. MHH	**Food**	**Clothing Ad male**	**Clothing Ad fem**	**Clothing children**	**Education**	**Uniform**	**Health**	**Fuel**	**Housing and other**
lnEXP	0.021	0.003**	0.003***	0.007***	0.000	-0.008	0.007**	-0.044***	-0.007
	(1.56)	(2.76)	(3.35)	(4.25)	(0.09)	(-0.25)	(2.17)	(-4.62)	(-0.96)
Treatment	-0.050***	0.002**	0.002**	0.005**	-0.006	-0.044	0.002	0.023**	0.013
	(-3.48)	(2.46)	(2.24)	(2.65)	(-1.54)	(-1.47)	(0.78)	(2.06)	(1.51)
Constant	0.599***	-0.010*	-0.025***	-0.068***	0.015	0.117	-0.060**	0.407***	0.171**
	(5.92)	(-1.86)	(-3.65)	(-4.02)	(0.46)	(0.49)	(-2.73)	(5.09)	(2.56)
Budget share at baseline	0.663	0.004	0.006	0.015	0.032	0.012	0.018	0.160	0.106
Elasticity	1.03	1.71	1.56	1.49	1.01	0.37	1.39	0.73	0.94
Panel C. FHH	**Food**	**Clothing Ad male**	**Clothing Ad fem**	**Clothing children**	**Education**	**Uniform**	**Health**	**Fuel**	**Housing and other**
lnEXP	0.005	0.001	0.005***	0.004**	0.011**	0.022	0.008*	-0.039***	0.000
	(0.33)	(1.85)	(3.18)	(2.98)	(2.11)	(0.67)	(1.92)	(-4.13)	(0.00)
Treatment	-0.028*	0	0.001	0.001	0.003	0.024	-0.004	0.002	0.022**
	(-1.71)	(-1.1)	(0.97)	(0.9)	(0.69)	(0.94)	(-0.99)	(0.19)	(2.91)
Constant	0.649***	-0.005*	-0.029**	-0.013	-0.079**	-0.335	-0.032	0.423***	0.107
	(6.37)	(-1.71)	(-2.57)	(-1.29)	(-2.35)	(-1.29)	(-0.89)	(5.44)	(1.62)
Budget share at baseline	0.671	0.002	0.006	0.008	0.034	0.011	0.018	0.160	0.104
Elasticity	1.01	1.51	1.96	1.51	1.32	3.04	1.44	0.75	1.00

Table 13: Ex-ante prediction of program impact on expenditure shares

Panel A. All sample	**Food**	**Clothing**			**Education**		**Health**	**Fuel**	**Housing and other**
		Adults male	**Adults female**	**Children**	**Total**	**Uniform and shoes**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pooled Elasticity	1.020	1.628	1.730	1.532	1.121	1.176	1.426	0.746	0.970
% change in total EXP	17	17	17	17	17	17	17	17	17
% change of spending on group	17.332	27.676	29.404	26.037	19.057	19.990	24.248	12.679	16.488
mean spending at baseline	476.883	0.958	2.016	4.025	25.752	8.225	13.748	107.188	75.124
Ex-ante predicted impact	82.6520	0.2652	0.5929	**1.0479**	**4.91**	**1.6441**	3.3336	13.5905	12.3867
Actual DiD impact estimate	64.186	-1.451	-1.876	**13.064**	**15.94**	**6.554**	-0.121	-0.365	-9.977
Panel B. MHH	**Food**	**Clothing**			**Education**		**Health**	**Fuel**	**Housing and other**
		Adults male	**Adults female**	**Children**	**Total**	**Uniform and shoes**			
Pooled Elasticity	1.031	1.706	1.561	1.495	1.012	0.371	1.390	0.726	0.937
% change in total EXP	17	17	17	17	17	17	17	17	17
% change of spending on group	17.535	29.001	26.535	25.410	17.212	6.309	23.638	12.337	15.931
mean spending at baseline	487.974	1.291	2.179	5.503	25.929	8.767	14.469	107.654	77.160
Ex-ante predicted impact	85.567	0.374	0.578	**1.398**	**4.463**	**0.553**	3.420	13.282	12.292
Actual DiD impact estimate	4.805	-2.198	-1.49	**15.075**	**21.027**	**7.091**	-0.369	-6.623	-8.109
Panel C. FHH	**Food**	**Clothing**			**Education**		**Health**	**Fuel**	**Housing and other**
		Adults male	**Adults female**	**Children**	**Total**	**Uniform and shoes**			
Pooled Elasticity	1.01	1.51	1.96	1.51	1.32	3.04	1.44	0.75	1.00
% change in total EXP	17	17	17	17	17	17	17	17	17
% change of spending on group	17.127	25.736	33.362	25.643	22.358	51.726	24.409	12.820	17.001
mean spending at baseline	466.149	0.636	1.859	2.594	25.581	7.699	13.050	106.737	73.153
Ex-ante predicted impact	79.836	0.164	0.620	**0.665**	**5.719**	**3.982**	3.185	13.684	12.437
Actual DiD impact estimate	130.6	-0.635	-2.291	**10.528**	**10.009**	**5.993**	0.086	6.29	-12.345

Appendix

Table A1: Household and community characteristics at baseline

	Treatment	Control	Difference
Household characteristics			
Female headed household (1=yes)	46.30	48.70	-2.40
Age of the head (years)	51.10	51.30	-0.20
Household size	5.79	5.42	0.37*
Higher education of household head	4.14	4.08	0.06
Number of household members			
Children age 0-5	0.90	0.80	0.1**
Children age 6-12	1.10	1.10	0.00
Children age 13-17	0.70	0.80	-0.10
Male Adult 18-59	1.20	1.10	0.10
Female Adults 18-59	1.40	1.20	0.2**
Male elderly 60+	0.10	0.10	0.00
Female elderly 60+	0.30	0.30	0.00
Orphan children 0-17	1.20	1.20	0.00
Household Assets			0.00
Proportion of households with good quality roof	65.10	68.50	-3.40
Proportion of household that own pigs	18.20	12.60	5.6*
Poverty Indicators			
Poverty headcount	76.20	72.70	3.50
Poverty gap	31.80	29.10	2.70
Severity of poverty	16.60	15.00	1.60
Community Level Indicators			
Average return journey time to nearest (hours)			
Health clinic	2.7	2.7	0.00
Place to get public transport	1.1	0.7	0.40
Food market or shop to buy groceries	1.4	1.2	0.20
Source of drinking water	0.6	0.5	0.10
Primary school	1.2	1.2	0.00
Price reported: maize	4.10	3.80	-0.30
Price reported: wheat	6.00	5.80	-0.20
Price reported: sorghum	6.30	6.30	0.00
Price reported: rice	13.70	15.30	1.60
Price reported: milk	13.80	15.00	1.20
Price reported: eggs	1.30	1.20	-0.10
Price reported: edible oil	20.60	19.90	-0.70
Price reported: dried beans	15.40	14.20	-1.20
Price reported: sugar	10.20	10.60	0.40
Price reported: salt	7.60	8.60	1.00
Price reported: paraffin:	10.80	9.50	-1.30
Price reported: candle	2.90	2.90	0.00
Price reported: rubber boots	107.80	196.70	88.9**
Average daily wage for: men in crop act.	23.00	28.90	5.90
Average daily wage for: men in livestock act.	323.40	308.60	-14.80
Average daily wage for: women in crop act.	23.60	25.10	1.50
Average daily wage for: women in livestock act.	244.70	300.40	55.7**

Table A2: List of covariates included in the regression analysis

Community Prices for individual items	Maize, wheat, sorghum, rice, milk, eggs, oil, beans, sugar, salt, paraffin candle, rubber boots
Community wages	Average daily wage for men in crop activities;
	Average monthly wage for men in livestock activities;
	Average daily wage for women in crop activities;
	Average monthly wage for women in domestic work
Community Shocks	At least 25% of hh in the community experienced death in community
	At least 25% of hh in the community experienced livestock death/disease
	At least 25% of hh in the community experienced livestock theft
	At least 25% of hh in the community experienced crop loss
	Any household in the community experienced drought
	Any household in the community experienced flooding
	Any household in the community experienced agricultural input price shock
	Any household in the community experienced agricultural input price shock
	Any household in the community experienced agricultural product price shock
	Any household in the community experienced livestock price shock
	Any household in the community experienced food price shock
	Any household in the community experienced reduced trading
Household characteristics interacted with time dummy	Number of household members age 0-5
	Number of household members age 6-12
	Number of household members age 13-17
	Number of male household members age 18-59
	Number of female household members age 18-59
	Number of elderly male household members age 60+
	Number of elderly female household members age 60+
	Number of orphans
	Highest level of education held by any person in the household
	Household head is widow
	Household head is elderly
	Series of District Dummies