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Shaping Cash Transfer Impacts Through 'Soft-Conditions': Evidence from Lesotho†

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Abstract

Cash transfer programmes 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 'labelling' and 'messaging' in evaluating the impact of the *Child* Grants Program in Lesotho, an unconditional cash transfer programme 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 suggest that 'soft conditionality' does play a 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 behavioural change is confirmed by comparing the ex-ante expected behaviours with the ex-post actual response to the programme. We find that for expenditure categories linked to the well-being of children the ex-post response was much higher than the ex-ante expected behaviour.

Key words: cash transfers, consumption, food security, impact evaluation, soft conditionality, behavioural change

JEL classification: C93, D12, I38

1. Introduction

Over the past 20 years, a growing number of African governments have launched social protection programmes to provide assistance to households that are ultra-poor, labour-

constrained, and/or caring for orphan or vulnerable children. Usually these programmes aim at reducing poverty and vulnerability by improving consumption, nutrition, health status, school attendance and educational outcomes.[1] While most of the programmes in Latin America provide cash transfers conditional on meeting certain requirements (mainly school attendance, regular visits to health centre for growth monitoring and updating of vaccination cards), the majority of the cash transfer programmes in African countries are 'unconditional': they are paid directly to beneficiary households without explicit conditions or labour 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 Handa *et al.* 2009; de Braw and Hoddinott, 2011; Baird *et al.* 2013). From the public perspective, imposing conditions may help the government to overcome information asymmetries: government may be aware of the benefits associated with preventive healthcare 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 pay money preferentially to female recipients. Furthermore, insights from behavioural economics emphasise 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 programme (Caldes *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 programme. 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 programmes 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 optimise behaviour and maximise the effectiveness of cash transfers programmes (Bastagli *et al.* 2016). The results are mixed. Handa *et al.* (2009) evaluate the behavioural impact of conditions on spending behaviour 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 programme

1 For a comprehensive overview of the impacts of cash transfers programmes, see Fiszbein *et al.* (2009) and Tirivayi *et al.* (2016).

does not induce behavioural changes (the so called 'substitution effect'). Teixeira *et al.* (2011) evaluate the impact of the CCTs Takopora in Paraguay on healthcare utilisation 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'. Robertson *et al.* (2013) investigate the effects of CCTs and UCTs on birth certificates, vaccine uptake and school attendance in a randomised 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. Analysing *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 CCTs would give an effect on school enrolment eight times higher with respect to a dollar spent on increasing household's income. Using the same data, de Braw 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 programme 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 enrolment. Shady and Araujo (2008), exploiting differential parental beliefs on the school attendance requirement attached to the programme, find similar results from the Ecuadorian program *Bono de Desarrollo Umano*. Using data from a randomised 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 behaviour 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 programme was not conditional on these visits. Akresh *et al.* (2013) conduct a randomised 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 CCT programmes have a similar impact, increasing enrolment for children who are traditionally favoured 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 enrolment 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.

Overall, while most of the 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 (CGP) in Lesotho and the Colombian *Familias en Acción*. In this latter programme 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 labelling are again the CGP in Lesotho and the *Tayssir* program in Morocco, a cash transfer programme 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 enrolment, and a 'labelled' unconditional component in which cash transfers are explicitly tied to an educational goal but without requirements on attendance or enrolment.

This paper focuses on the role of 'soft conditionality', implemented through both 'labelling' and 'messaging', to evaluate the effect of the CGP in Lesotho on household total consumption, food consumption, food security for children, schooling-related expenses and school enrolment. As with many programmes 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 programme.

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 programme should lead to an income effect, but not necessarily behavioural change. However, a large body of empirical evidence reports relationships between income sources and the resulting behavioural response (for surveys, see Fraker, 1990; Thaler, 1990; Haveman and Wolfe, 1995). Moreover, the behavioural economics literature suggests that 'labelling' 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.

Few studies investigate the role of 'labelling' and 'messaging' in consumption and educational outcomes. Benhassine *et al.* (2015) use data from the Tayssir programme in Morocco to estimate the impact of the 'labelled' 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 labelled as an education support programme. 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 labelling on consumption in the context of a child benefit in Slovenia but finds no evidence for it.

Our contribution to the existing literature is threefold. First, our paper assesses the effectiveness of CGP in positively affecting behaviours that are meant to be influenced by the

conditionality. Contributions on the role of social protection programmes in SSA are rapidly growing as more data on randomised 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 the 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% 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 favour 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 programme '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) programme effects with the ex-ante expected effects given baseline expenditure elasticities to test whether the programme simply moves households along their total expenditure Engel curve or in fact shifts that curve, suggesting a behavioural change in favour of children well-being induced by the messaging.

Moreover, we provide a comparison of the impact of the CGP on expenditure in favour of children (clothing for children, uniform and shoes, education) in Lesotho with respect to the impact of similar UCT programmes implemented in the same period in other African countries included in the Transfer Project (TP).[2]

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

2. Background and experimental design

2.1 Description of the programme, 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% rate (World Bank, 2014). However, agriculture has lagged behind other sectors and about 90% of farmers depend on subsistence agriculture for their livelihoods. Further, between 10% and 30% of the population suffers from food insecurity (Ministry of Health and Social Welfare (MOHSW) and ICF Macro, 1010). Despite economic growth, poverty rates remain high: 57% of the population are still estimated to live below the basic needs poverty line of US$1.08 per day, and 34% 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.

2 The TP is a joint Food and Agriculture Organization (FAO), UNICEF and University of North Carolina effort which supports and systemises lessons from impact evaluations of cash transfer programmes in sub-Saharan Africa (for details, see Davis *et al.*, 2016).

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 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).[3] 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).[4] In practice, due to operational issues around identifying OVCs, the programme focused on poor households with at least one child (under seventeen years) as a more appropriate targeting criterion. The programme is currently managed and financed by the Ministry of Social Development, though during the pilot phase considered in this study the programme received financial support from the European Union and the technical support from the United Nations Children's Fund (UNICEF)-Lesotho.

The CGP 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 randomised 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.[5] Second, in both treatment and control locations, targeting of the eligible and non-eligible households was carried out according to a combination of PMT and community validation. Household information was collected through a community-wide census following a community mobilisation event, where households were informed about the programme. 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 programmes 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

3 As of August 2016, the CGP is provided to 26,681 beneficiary households in 36 community councils across the 10 districts of the country.

4 OVC are defined as household residents between 0 and 17 years old with at least one deceased parent, or a parent who is chronically ill, or whose main caregiver is chronically ill.

5 The total number of EDs was 96, a number which ensures that the randomisation was done across a sufficient number of clusters.

were categorised 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 categorised 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 programme. 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 programme in July and August 2011 and the first payments started in September 2011. Further details on the programme, targeting procedure, randomisation and survey design can be found in Pellerano *et al.* (2012).

Beneficiary and non-beneficiary households 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.

In the same period, no accompanying intervention, specifically meant to contribute on the direct well-being of children, took place. The only intervention that has been implemented on the same beneficiary households in the period 2012–2013 was the Food Emergency Grant (FEG). It consisted on an emergency response to the poor harvest that strongly affected household livelihood and food supply in Lesotho. The FEG took the form of a bi-monthly top-up of 400 Maloti (200 Maloti/month) that was disbursed together with the CGP, but in a separate envelope. While the additional transfer had certainly a positive impact on beneficiaries' income, there is no reason to expect that such intervention could affect directly expenditure on education and/or clothing for children.

2.2 Qualitative evidence on the role of messaging

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 was 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 programme 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.' (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. 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. An example of greater awareness of children is the story of Thabo, a 13 years old, in grade 4 at primary school, reported in OPM (2014):

> *He explained things have changed since they started to receive the payment: 'before we sometimes did not wear shoes, we had very old torn uniforms. Now we have clean uniforms, we can change shoes (the school shoes he was wearing were indeed sturdy and in good condition), we eat meat and vegetables with pap'. Now he has breakfast before school. Thabo also explained he has a new ball to play with. He says, 'Kese ke khona ho ja ke khore. Bopheto bo hantle' [my life is fine because I can now afford to eat and my stomach is full]. Before, some children at school would talk about him, saying bad things about his torn uniform.* (OPM, 2014, page 48).

2.3 Characteristics of evaluation sample, attrition and balance

The baseline survey comprised 1,486 households roughly equally distributed between treatment and control areas. Table 1 reports the baseline sample size by treatment group. Overall 1,353 of the 1,486 households interviewed at baseline were tracked in the follow-up study. Sample attrition for the overall sample was therefore 9% (133 over 1,486 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, 5% and 12%). To address these issues and obtain unbiased estimates of the impact of the programme, 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 is performed to compare baseline control and treatment groups. We find that the randomisation 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), and community level indicators (see Table A1 in the Appendix). 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.

3. Empirical analysis

We conduct the analysis in three steps. First, we adopt a 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 enrolment (Section 3.1). Second, to assess the role of

Table 1: Beneficiary Status at Baseline

Beneficiary Status	Area		Total
	Treatment	Control	
HH eligible for CGP	747	739	1,486
HH eligible for CGP (balanced panel)	706	647	1,353

'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' (behavioural change) and 'income effect' through a comparison of the marginal propensity to consume (MPC) out of general income and out of cash transfers. A MPC out of transfer significantly greater than the MPC 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 behaviour, 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 programme simply moves households along their total expenditure Engel curve (no behavioural change), the ex-ante expected behaviour should line up with the ex-post actual response of households to the programme. On the contrary, if the programme leads to a shift of the Engel curve (behavioural change) the programme ex-ante expected behaviour 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 programme beneficiaries with a group of non-beneficiaries serving as controls, all interviewed before the programme began and again two years later, adopting a DID approach.

To estimate the potential impact of the programme 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 programme. The difference between the two would be entirely attributable to the programme, and the parameter of interest, Δ, would be estimated as

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

where $Y_{j,\mathrm{k}}$ 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 programme can be estimated under two weak assumptions. The first assumption states that, in the absence of the programme, there are common time effects across treatment and control areas, i.e.,

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

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 programme on the outcomes as

$$\begin{aligned}\Delta = & E(Y_{1,A} \mid T=1, X) - E(Y_{0,B} \mid T=1, X) + E(Y_{0,B} \mid T=1, X) - E(Y_{0,A} \mid T=1, X) \\ = & \underbrace{[E(Y_{1,A} \mid T=1, X) - E(Y_{0,A} \mid T=0, X)]}_{A'} - \underbrace{[E(Y_{0,B} \mid T=1, X) - E(Y_{0,B} \mid T=0, X)]}_{B'}\end{aligned} \tag{3}$$

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 programme on the outcome variables.

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

Equation (4) represents the regression equivalent of DID:

$$Y_{i,t} = \beta_0 + \beta_1 d\_CGP_{i,t} + \beta_2 d\_2013_i + \beta_3 (d\_CGP_{i,t} * d\_2013_i) + \sum \beta X_i + \mu_{i,t} \tag{4}$$

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).

$$\begin{aligned}Y_{i,t} = & \gamma_0 + \gamma_1 d\_CGP_{i,t} + \gamma_2 d\_2013_i + \gamma_3 (d\_CGP_{i,t} \cdot d\_2013_i) \\ & + \delta_0 d\_femhd_i + \delta_1 (d\_femhd_i \cdot d\_CGP_{i,t}) + \delta_2 (d\_femhd_i \cdot d\_2013_i) \\ & + \delta_3 (d\_femhd_i \cdot d\_CGP_{i,t} \cdot d\_2013_i) + \sum \beta X_i + \mu_{i,t}\end{aligned} \tag{5}$$

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.[6]

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

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

In Table 2, we report our estimates of the impact of the programme 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 DID 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 2 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 3. 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

Table 2: 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					

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

Table 3: Impact of CGP on Various Indicators of Food Security

	Food Shortage		Average 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				

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

Table 4: 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	11.909*	−0.635	−2.291	10.528**
($\gamma_3 + \delta_3$)	(1.87)	(−0.40)	(−1.04)	(2.90)
Observations		2,701		

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

Table 5: 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	

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

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.

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 4).[7] Furthermore, no impact is detected on expenditure on health, fuel and housing (see Table 5). 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 enrolment

Table 6 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. This result is coherent with the qualitative findings in OPM (2014) which documents that expenditure on clothing was strongly encouraged at pay point as an example of expenditure in favour of the well-being of children.

Table 7 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 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.

7 Self-reporting bias on expenditure for items related to the messaging could be in principle a problem. However, we believe that in the case of the CGP impact evaluation this should not represent a major concern. The increase in expenditure in clothing for children and uniform and shoes is not only documented by the direct beneficiaries but also by community representatives, which were mostly not CGP beneficiaries because they did not meet the targeting criteria. As documented in Pellerano *et al.* (2014), community representatives reported CGP beneficiaries using the transfer for shoes and clothing for children, and education. This was also facilitated by the fact that better clothing for children and new uniforms and shoes are verifiable because they are tangible goods visible to anyone living in the same community.

8 We also analysed the impact of CGP on school enrolment 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 look at impact of CGP on children schooling, labour and time use, with a particular focus on differentiated impacts by gender and household structure.

Table 6: 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		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					

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

Table 7: Impact of CGP on School Enrolment

	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					

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

3.2 'Unpacking' the role of programme'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 behaviours of beneficiary households. If conditionality is binding, programme transfers will exert an income and substitution effect on household spending behaviour, while general income only exerts an income effect on such behaviour. If the substitution effect is big, the MPC out of transfer income for items related to children' well-being will be larger than the MPC out of general income; if the substitution effect is small or zero, then the programme only exerts an income effect and the two MPCs will be statistically equal.

We estimate the following equation:

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

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 enrolment. *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 normalise 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:

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

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 well-being 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

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.

purchased as additional control.[11] However, the inclusion of this 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 minimise 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 generates 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.

3.2.2 Comparison of MPCs

Tables 9–11 present the 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 MPC out of income is significantly greater than the MPC 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 (behavioural 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

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.

Table 8: 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 expenditure—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		

Note: ***, ** and * 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 in the Appendix. MHH and FHH stand for male household head and female household head, respectively.

uniforms and shoes both an income and substitution effects (behavioural change) are taking place. In this section, focusing only on expenditure items, we propose another test for potential behavioural changes induced by the programme. Following The Kenya CT-OVC Evaluation Team (2012), we unpack how the CGP has affected behaviour by using standard demand theory to predict how the programme ought to impact spending in favour of children, based on pre-programme expenditure elasticities. Our approach consists on deriving theoretically consistent expenditure elasticities from baseline (pre-programme) and uses these to predict household responses to the programme. The rational of this kind of analysis is the following: if the programme simply moves households along their total expenditure Engel curve, the ex-ante expected behaviour should line up with the ex-post actual response of households to the programme. If this occurs, no behavioural 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 programme it is greater than the ex-ante expected one, behavioural 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

Table 9: 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 (0.03)	0.021** (2.46)	0.1034	0.006 (0.59)	0.027** (2.41)	0.234
Clothing expenditure adult males—MHH	0.003 (0.19)	0.031** (1.99)	0.2016	0.001 (0.07)	0.046** (2.09)	0.199
Clothing expenditure adult males—FHH	−0.007 (−0.62)	0.012* (1.99)	0.1662	0.027** (2.36)	0.007 (0.9)	0.152
Clothing expenditure adult females	−0.004 (−0.36)	0.022*** (3.02)	0.083	−0.019* (−1.8)	0.007 (0.8)	0.113
Clothing expenditure adult females—MHH	0.005 (0.38)	0.017 (1.54)	0.515	−0.024 (−1.44)	−0.003 (−0.23)	0.395
Clothing expenditure adult females—FHH	−0.012 (−0.68)	0.027** (2.75)	0.085*	−0.021 (−1.26)	0.003 (0.17)	0.354
Clothing expenditure children	0.174*** (6.94)	0.064*** (4.32)	0.0005***	0.188*** (6.05)	0.069*** (3.02)	0.002***
Clothing expenditure children—MHH	0.202*** (6.25)	0.091*** (3.45)	0.010***	0.193*** (5.42)	0.106** (2.75)	0.065*
Clothing expenditure children—FHH	0.142*** (4.28)	0.035* (1.93)	0.010***	0.206*** (5.14)	0.023 (0.86)	0.000***
Fuel expenditure	0.027 (1.48)	0.040*** (3.15)	0.5642	−0.017 (−0.85)	0.059*** (3.62)	0.004***
Fuel expenditure—MHH	0.026 (1.28)	0.041** (2.71)	0.5522	−0.026 (−1.17)	0.036 (1.39)	0.067*
Fuel expenditure—FHH	0.031 (1.27)	0.045** (2.55)	0.6095	0.012 (0.44)	0.082*** (3.66)	0.020**
Health expenditure	1.067 (0.26)	7.371** (2.39)	0.2700	−3.523 (−1.02)	9.563** (2.29)	0.017**
Health expenditure—MHH	−1.534 (−0.23)	4.357 (1.25)	0.4826	−3.264 (−0.69)	12.208** (2.54)	0.029**
Health expenditure—FHH	0.408 (0.10)	8.045* (1.84)	0.1777	−3.946 (−0.70)	9.996 (1.69)	0.082*
Housing and other expenditure	7.251 (0.73)	28.233*** (3.42)	0.1178	0.932 (0.08)	34.986*** (4.07)	0.018**
Housing and other expenditure—MHH	18.974 (1.27)	22.357** (2.29)	0.8514	11.339 (0.71)	14.941 (1.15)	0.859
Housing and other expenditure—FHH	−7.040 (−0.61)	31.701** (2.91)	0.0280	0.709 (0.05)	44.207*** (3.48)	0.032**
Observations		2,701			2,701	

Note: ***, ** and * 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 in the Appendix. MHH and FHH stand for male household head and female household head, respectively

Table 10: 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: ***, ** and * 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 in the Appendix. MHH and FHH stand for male household head and female household head, respectively

in Deaton and Muellbauer (1980); Handa (1996) and The Kenya CT-OVC Evaluation Team (2012):

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

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 variables 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):

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

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

Table 11 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 panels 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 programme on expenditure patterns. The CGP provides transfers that correspond to 17% of the beneficiaries average baseline total consumption expenditure. Using the elasticity estimates in Table 11, we can predict the percentage change in expenditure (at the mean) for each expenditure item considered. These are the ex-ante predicted programme impacts assuming no behavioural changes. Table 12 summarises 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% increase in total expenditure will result in 19.057% 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 12 suggest that there are some important differences between actual programme effects and what we would expect given baseline preferences of targeted households. Indeed, looking across the other household items groups, we see that actual

Table 11: Engel Curve Estimates and Expenditure Elasticities, Pooled Baseline Sample.

	Food	Clothing Ad male	Clothing Ad fem	Clothing children	Education	Uniform	Health	Fuel	Housing and other
Panel A. all sample									
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									
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									
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

Note: ***, ** and * indicate significance at 1%, 5% and 10%.

Table 12: Ex-Ante Prediction of Programme Impact on Expenditure Shares

	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)
Panel A. All sample									
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									
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									
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

programme 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 behavioural change in favour of children well-being induced by the programme.

4. Comparison with other cash transfer programmes in Sub-Saharan countries

In order to further support the evidence on the role of the 'soft conditionality' of the CGP, we compare the findings on children-related expenditure (education, clothing, and uniform and shoes) with the results of six evaluations on large-scale government UCT programmes in Sub-Saharan African countries, conducted within the TP. We consider the following evaluations: Ethiopia Tigray Social Cash Transfer Pilot Program (SCTPP); Ghana Livelihood Empowerment Against Poverty (LEAP); Kenya Cash Transfers for Orphans and Vulnerable Children (CT-OVC); Malawi Social Cash Transfer Program (SCTP), Zambia Child Grant Program (CGP), and Zimbabwe Harmonized Social Cash Transfer (HSCT).

Table 13 summarises the key components of the suite of the six evaluations, in addition to Lesotho, and the main findings regarding children-related expenditure. Although specific programme objectives vary, all programmes were designed with poverty-related objectives, including the improvement of food security, health and education of children, and household resilience to negative shocks (Handa *et al.* 2017). One key component of all programmes reviewed here is the fact that they are unconditional. However, beside the 'soft conditionality' in Lesotho, the Livelihood Empowerment Against Poverty (LEAP) program in Ghana was originally conceived as conditional but it was never enforced, and the SCTP in Malawi provides a 'top-up' benefit for school-aged children, although enrolment status was not a condition nor it is verified. The CGP in Lesotho shares with the LEAP in Ghana the irregularity of payments. The transfer size of the CGP in Lesotho as percentage of baseline consumption was similar to the SCTP in Malawi and the HSCT programme in Zimbabwe.

The final three columns of Table 13 summarise the estimated impact of the key variables used to test the role of the 'soft conditionality', namely expenditure on education, expenditure on clothing for children, and expenditure for uniforms and shoes. The CGP in Lesotho shares with the SCTP in Malawi the positive and significant impact on expenditure in education. The evaluations of the Zambian CGP document an impact on expenditure for children's clothing and for uniforms and shoes similar to that found for the CGP in Lesotho.

Given the key characteristics of the programmes and the main results on the three key variables, we conducted for Malawi the same exercise, described in Section 3.2, consisting on the comparison between the MPC out of transfer and the MPC out of general income. For this exercise, we use available data collected for the SCTP midline impact evaluation (UNC, 2015). Table 14 shows the results for Malawi (panel A) and Lesotho (panel B – same results already reported in Table 10 to facilitate the comparison): no significant differences between the MPC out of transfers and MPC out of general income are detected for Malawi.

Although this exercise is not meant to be exhaustive, it strengthens our main findings by providing both a descriptive comparison between the CGP in Lesotho and similar cash

Table 13: Comparison of Cash Transfer Programs in Sub-Saharan Countries

Countries	Program	Years of data collection	Target group	Transfer size (% of baseline consumption)	Transfer type	Regularity of transfer	Exp. on education	Exp. on clothing for children	Exp. on uniform/ shoes
Lesotho	*Child Grant Programme (CGP)*	*2011, 2013*	*Poor households with a child under 18 years old*	*~17%*	*Flat transfer until April 2013; then variable transfer by number of eligible HH members*	*No*	*191.29*** Maloti (yearly) equivalent to 58% increase with respect to baseline mean*	*156.77*** Maloti (yearly) equivalent to more than 100% increase with respect to baseline mean*	*78.65*** Maloti (yearly) equivalent to 84% increase with respect to baseline mean*
Ethiopia	Tigray Social Cash Transfer Programme Pilot (SCTPP)	2012, 2014	Ultra-poor, labour-constrained, female or child headed households with elderly, or disabled members	~25%	Base Flat + additional variable transfer by number of eligible HH members	Yes	Qualitative evidence on increase of expenditure on education	No significant change	N/A
Ghana	Livelihood Empowerment Against Poverty (LEAP)	2010, 2012	Extreme poor with elderly, disabled or OVC member	~7%	Variable transfer by number of eligible HH members	No	No significant change	No significant change	N/A
Kenya	Cash Transfers for Orphans and Vulnerable Children (CT-OVC)	2007, 2011	Poor households with OVC	~22%	Flat transfer	Yes	No significant change	No significant change	N/A

Continued

Table 13: Continued

Countries	Program	Years of data collection	Target group	Transfer size (% of baseline consumption)	Transfer type	Regularity of transfer	Exp. on education	Exp. on clothing for children	Exp. on uniform/ shoes
Malawi	Social Cash Transfer Programme (SCTP)	2013, 2015	Ultra-poor labour-constrained	~18%	Variable transfer by number of eligible HH members	Yes	202.72*** Kwacha (yearly) from endline evaluation equivalent to 60% increase with respect to baseline mean; 197.70*** Kwacha (yearly) from midline evaluation equivalent to 59% increase with respect to baseline mean	N/A	Qualitative evidence on increase of expenditure on uniforms
Zambia	Child Grant (CG) model of the Social Cash Transfer (SCT) Programme	2011, 2013	Household with a child under 5 years old	~27%	Flat transfer	Yes	No significant change	14 pp increase equivalent to 22% increase with respect to baseline mean;	33 pp increase for shoes equivalent to 100% increase with respect to baseline mean
Zimbabwe	Harmonized Social Cash Transfer (HSCT)	2013, 2014	Food poor and labour-constrained	~20%	Variable transfer by number of eligible HH members	Yes	No significant change	No significant change for clothing	Qualitative evidence on the increase of expenditure on uniforms

Note: *** indicates significance at 1%.

Source: Adapted from Handa *et al.* (2017). Additional sources: Ethiopia SCTPP (Berhane *et al.*, 2015); Ghana LEAP (Handa *et al.*, 2014); Kenya CT-OVC (Ward *et al.* 2010); Malawi SCTP (UNC, 2016); Zambia CGP (AIR, 2013); Zimbabwe HSCT (AIR, 2014).

Note on years of data collection: additional rounds of data collection were undertaken in some countries, however because of comparability we report the rounds which make up the majority of estimates presented here.

Table 14: Soft Conditionality Results for Expenditure in Education: Comparison Between the CGP in Lesotho and the SCTP in Malawi

Panel A	Malawi Social Cash Transfer Program					
	OLS regression			Fixed-effect regression		
	SCTP transfers (log)	HH income (log)	*P*-value for difference	SCTP transfers (log)	HH income (log)	*P*-value for difference
Expenditure in education	0.098***	0.122***	0.459	0.110***	0.035	0.11
	(8.29)	(4.28)		(7.71)	(1.54)	
Expenditure in education—MHH	0.054**	0.085	0.675	0.064*	0.068	0.937
	(2.37)	(1.17)		(1.94)	(1.27)	
Expenditure in education—FMM	0.106***	0.121***	0.634	0.116**	0.039	0.110
	(8.78)	(4.44)		(6.77)	(1.02)	
Panel B	Lesotho Child Grant Program					
	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
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—FMM	0.154***	0.134***	0.643	0.214***	0.092**	0.013**
	(5.09)	(5.24)		(6.64)	(2.46)	

Note: ***, ** and * indicate significance at 1%, 5% and 10%.

transfer programmes implemented in other Sub-Saharan countries, and a parallel with Malawi, in which no behavioural change seems to have taken place despite the strong impacts observed on the outcomes of interest.

5. Conclusions

This paper uses data collected for the impact evaluation of the CGP in Lesotho. We focus on the role of 'soft conditionality' implemented through both 'labelling' 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 DID estimates show that the programme 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 enrolment of boys.

The main contribution of this paper is our analysis of whether the programme has shifted preferences due to the 'soft conditionality' implicitly imposed by the programme. Most impact studies calculate the programme impact using a DID approach, which we present here. However, we go further proposing two different approaches to test whether the

programme may have caused preferences to shift in favour 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 MPC out of transfers with the MPC out of general income on goods that ought to be affected by the 'soft conditionality'. This analysis shows that 'soft conditionality' did play a role on outcomes most directly associated with the programme messaging (heavily focused on well-being of children). 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 'soft conditionality' studying whether CGP changed the preferences of households in terms of their consumption behaviour, 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 programme simply moves households along their total expenditure Engel curve (no behavioural change), the ex-ante expected behaviour should line up with the ex-post actual response of households to the programme. Our findings show that the ex-post actual programme effects are higher than the ex-ante expected ones for clothing for children, education and expenditure for school uniforms and shoes, suggesting a behavioural change in favour of children well-being induced by the programme.

In both approaches, we also 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.

Finally, we compare the findings on children-related expenditure (education, clothing, and uniform and shoes) with the results of six evaluations on large-scale government UCT programs in Sub-Saharan African countries, namely Ethiopia, Ghana, Kenya, Malawi, Zambia and Zimbabwe, all designed with poverty-related objectives, including the improvement of food security, health and education of children, and household resilience to negative shocks. Given the key characteristics of the programmes and the main results found on expenditure on education, clothing for children and uniforms and shoes, we conducted for Malawi a comparison between the MPC out of Social Cash Transfer Program and the MPC out of general income. We find that in the case of Malawi, where no 'messaging' took place, there is no behavioural change and money from the programme is used exactly in the same way of money from general income.

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

To conclude, the results suggest two main policy implications. First, social programmes can incentivize the achievement of the desired goals of the programme through 'messaging', without necessarily imposing any explicit conditionality. Soft-conditioned programmes 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, programmes adopting a soft-conditionality approach should carefully consider how to tailor the communication strategy to reflect the full array of programme objectives. A too narrowly specified message, if effectively conveyed to the beneficiaries and enforced through social monitoring, may limit the potential impact of the programme. 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. Programmes could 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.

Supplementary material

Supplementary material is available at *Journal of African Economies* online.

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