Integrating Simulation and Experimental Approaches to Evaluate Impacts of SCTs: Evidence from Lesotho[*]

J. Edward Taylor[1], Anubhab Gupta[1, †], Mateusz Filipski[2], Karen Thome[3], Benjamin Davis[4], Luca Pellerano[5], and Ousmane Niang[6]

Abstract

This paper compares results from simulations and experiments to evaluate the impact of social cash transfers on local incomes, including spillovers to non-beneficiaries. Using data from a randomized experiment of Lesotho's Child Grants Programme (CGP) we find that, within treatment clusters, incomes increased in households eligible for cash transfers and also in those that did not receive the transfers. We estimate an average treatment effect on the treated local economy and decompose it into impacts on recipients and non-recipients. The findings reveal that impacts on the two household groups are heterogeneous across income sources, and quantile treatment effects reveal that they are heterogeneous across income distributions. To our knowledge, this is the first study to corroborate ex-ante simulations of income spillovers from development projects using experimental data, and the first time social cash transfer spillovers of any kind have been experimentally identified outside Latin America. Both simulation and experimental findings indicate that Lesotho's CGP stimulated real economic growth in treated village clusters.

JEL Codes: *I38, O12, C21, D50*

Keywords: *CGP, cash transfers, eligible and ineligible households, ATTLE, QTE, treatment effect heterogeneity*

[*] The authors would like to thank Dalia Ghanem and Shu Shen for their helpful suggestions and comments.
[1] University of California, Department of Agricultural & Resource Economics, 1 Shields Ave., Davis, CA 95616
[2] International Food Policy Research Institute (IFPRI)
[3] USDA Economic Research Service
[4] United Nations Food and Agricultural Organization (FAO)
[5] International Labour Organization (ILO)
[6] United Nations Children's Fund (UNICEF)
[†]Corresponding Author: angupta@ucdavis.edu

Integrating Simulation and Experimental Approaches to Evaluate Impacts of SCTs: Evidence from Lesotho

Many developing countries implement social cash transfers (SCTs), conditional or unconditional, in the hope of achieving a variety of social outcomes in targeted households. SCTs also inject cash into local economies, creating the potential for income spillovers. A rich literature evaluates impacts of SCTs on the treated (e.g., Davis et al., 2016; Duflo and Kremer, 2005; Gertler, 2004; Schultz, 2004; Skoufias et al., 2001; Case and Deaton, 1998). A few studies quantify spillover effects on non-targeted households in treated localities (usually villages or village clusters). Most of the latter use general equilibrium techniques (Filipski et al., 2015; Taylor et al., 2013; Thome et al., 2013). Only a few have used experimental data (notably, Angelucci and De Giorgi, 2009), mostly because of a lack of data on households that are ineligible for transfers. We use unique experimental data to test predictions from ex-ante simulations that SCTs benefit ineligible as well as eligible households; that is, they create positive income spillovers in local economies. We find significant differences in spillovers across activities and at different points along the eligible and ineligible-household income distributions.

The Child Grants Programme (CGP) implemented by the Ministry of Social Development (MoSD) of the Government of Lesotho is an unconditional social cash transfer program that targets poor households with vulnerable children. The main objective of the CGP is to improve the living standards of orphans and vulnerable children (OVC), reduce malnutrition, improve health status, and increase school enrolment. Viewed through the lens of a general equilibrium framework, cash transfers treat local economies by transferring cash to treated households. The households eligible for the treatment are a conduit through which cash enters the local economy. As eligible households spend their cash, impacts may travel from eligible to non-eligible households.

Using a local economy-wide impact evaluation (LEWIE) simulation approach, Filipski et al. (2015) and Taylor, Thome and Filipski (2013) showed that CGP transfers can have a positive multiplier effect on the incomes of households in the village clusters treated by the program, including significant spillovers to non-beneficiary (ineligible) households.[7] Most CGP spillovers accrue to non-beneficiary households, which are less asset-poor than beneficiary households and thus are in a relatively favorable position to increase the supply of goods and services in response to increased local demand.

The CGP evaluation is unique in that both eligible and ineligible households were included in the baseline and follow-on surveys conducted in the treatment and control clusters. This makes it possible to econometrically test whether the simulated spillovers using the LEWIE model did, indeed, materialize, and if so, whether simulated and actual spillovers are similar in magnitude. This paper tests whether cash aid increased beneficiary households' income by more than the amount transferred, while also creating benefits for non-beneficiaries, via spillovers. We also test whether impacts are heterogeneous across income sources and across the income distribution for both household groups.

Our analysis uses data collected as part of a randomized control trial (RCT), including a baseline survey in 2011 and a follow-on survey in 2013. Using the panel with pre-and post-intervention information, we derive the average treatment effects of the CGP on income in both eligible and ineligible households using a difference-in-differences (DD) estimator. We find an average treatment effect on the treated local economy and disaggregate it into the impact

[7] Confidence intervals around simulated CGP income multipliers were constructed using a Monte Carlo method, made possible by econometrically estimating model parameters. The procedure is described in Taylor and Filipski (2014) and Filipski et al. (2015).

on the treated and the spillover to households not eligible for the program. We estimate the treatment heterogeneity of CGP impacts on incomes earned from different sources including livestock, agricultural activity, and wage work.

Information about heterogeneous distributional impacts is valuable because it can guide policymakers in framing new interventions if program benefits become concentrated in certain segments of the income distribution. There is a sparse literature using quantile regression to test for heterogeneous treatment effects. Distributional treatment effects may be important if the treatment is heterogeneous across the population (Heckman and Robb, 1985; Heckman, Smith and Clements, 1997), or alternatively, if the treatment produces heterogeneous spillovers. The distributional treatment impacts of cash transfers could potentially vary along the income distribution for both the households that receive the transfers and indirect beneficiaries living within the treated village clusters.

Abadie, Angrist and Imbens (2002) use an instrumental variable quantile treatment effects model to estimate the impact of a training program on trainee income. Chernozhukov and Hansen (2005) use endogenous treatments to find heterogeneous returns to schooling in the U.S. Callaway and Li (2015) estimate quantile treatment effects on the treated in DD models. Dammert (2009) uses quantile treatment effects to find heterogeneous treatment impacts of conditional cash transfers on educational, health and nutritional outcomes in Nicaragua. Angelucci, Karlan and Zinman (2015) use a similar approach but fail to find any distributional impacts of a microcredit program in Mexico. Cummins (2017) finds treatment heterogeneity on test scores of students in a Kenya school tracking experiment.

To characterize treatment-effect heterogeneity, one needs to make assumptions about a subject's rank in the counterfactual distribution, e.g., rank invariance or, less restrictively, rank similarity (Dong and Shen, 2015; Frandsen and Lefgren, 2015). We test for rank similarity for both eligible and ineligible households in treatment and control clusters after the CGP transfer treatment using a regression-based test designed by Frandsen and Lefgren (2015). While the rank similarity test is sufficient to perform heterogeneous treatment analysis, at a minimum the covariates for which the test fails should be included as controls if the rank similarity assumption has to be imposed. We find that the rank similarity assumption holds for households eligible for CGP transfers; however, we reject it for some covariates in our test for ineligible households. In the quantile treatment effect (QTE) regression for ineligible households, we include the covariates on which we fail to find rank similarity, in the process obtaining insights into which ineligible households the CGP intervention affected most.

We use QTE in our DD estimation to test whether income impacts are heterogeneous across income percentiles for CGE-eligible households. We find heterogeneity of treatment impacts on ineligible households without imposing rank similarity. The failure of the rank similarity test leads to the conclusion that ineligible households with higher average education, older heads, and more members benefit most from spillovers. This analysis adds to the literature using quantile regression to estimate heterogeneous impacts of cash transfers on treated households, while extending it by estimating differential spillover effects on ineligible households in treated village clusters.

The villages targeted by the CGP contain roughly four times more ineligible than eligible households. Spillovers become diffused across households, like ripples in a pond. This creates challenges when it comes to identifying spillover impacts on ineligible households. To complicate matters further, for budgetary reasons the random sample of ineligible households was reduced by one-half in the follow-on survey, decreasing the degrees of freedom in our analysis considerably. Despite these challenges, our experimental results confirm the LEWIE simulation finding that the CGP generates significant positive income spillovers within treated clusters. There are income multipliers on the households that receive the payments and income spillovers to non-recipient households. Income multipliers obtained from our experimental results are higher than the LEWIE simulation findings in Filipski et al. (2015) for eligible households, consistent with human capital or asset accumulation effects over time. Overall, local economy multipliers obtained from LEWIE and experimental results are comparable.

The rest of this paper is organized as follows. Section 1 introduces the Lesotho CGP RCT data with a baseline balance of household summary variables. Section 2 describes our estimation strategy to obtain the average treatment effect on the treated local economy and we disaggregate it into effects on eligible and ineligible households. It also lays out the assumptions for evaluating the heterogeneous impact of cash transfers using quantile treatment effects. We present our estimation results in Section 3. Section 4 compares our experimental results with ex-ante simulation findings from Filipski et al. (2015). In Section 5, we discuss the robustness of our findings to alternative specifications and consider why experimental outcomes might differ from simulation results. Section 6 concludes.

1. The CGP Evaluation Design and Data

The Lesotho CGP was launched in 2009 with 1,250 beneficiary households. It expanded at the end of 2013 to cover five districts (Barea, Leribe, Mafeteng, Maseru and Qacha's Nek) and approximately 20,000 households.

The timing of the baseline and follow-on surveys brackets the rollout of the CGP experiment. Detailed descriptions of the surveys appear in Dewbre et al. (2015) and Daidone et al. (2014). The surveys used a randomized cluster design to avoid control group contamination. Villages were grouped into clusters isolated from one another geographically as well as economically to minimize the likelihood that CGP impacts find their way to the control clusters. Figure 1 illustrates the experimental design, which included 96 electoral divisions (EDs), of which 48 were randomly chosen for the CGP treatment and the rest were controls. Within the villages in the 48 treated EDs, all households eligible for the program received cash transfers. Pellerano (2011) details the sampling procedure.

Both baseline and follow-on surveys gathered detailed data on all household income sources, including production activities (crops, livestock, retail, other services, and other production), wage work, and private and public transfers. For each production activity the survey asked about all inputs, including purchased inputs, hired labor, family labor, and capital, as well as total production and sales. The crop and livestock modules provide information on products that are home-produced or purchased directly from neighboring households.

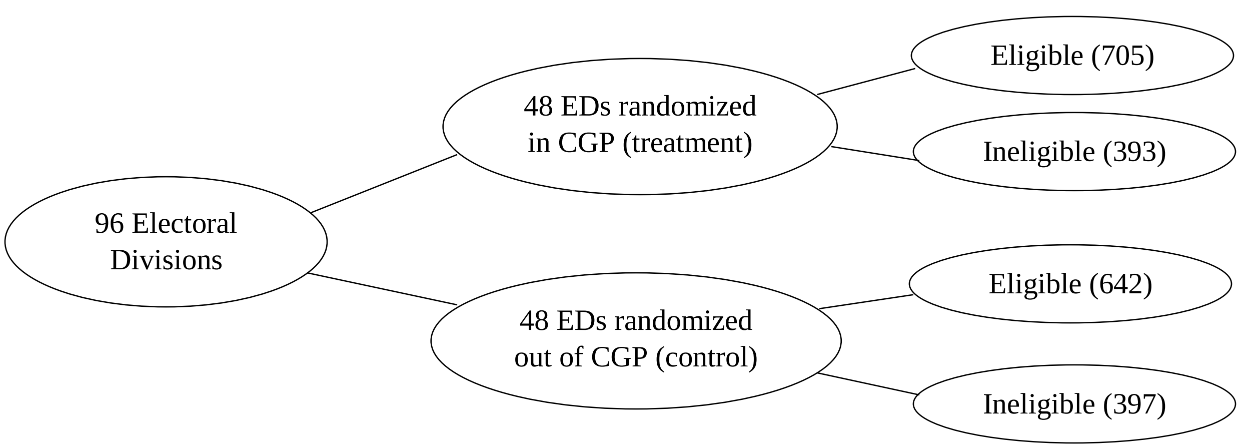


Figure 1: Lesotho CGP Experimental Design

Numbers in parenthesis give the sample size in each group in each round of survey.

Originally, the transfer amount was LSL 120 ($12) per month irrespective of the number of children in the household. In April 2013, the amount was indexed to the number of resident children. Using administrative data on the transfers, which are quarterly, Table 1 shows the distribution of eligible households in the treated clusters that received different transfer amounts. Of all eligible households in treated clusters receiving CGP transfers, 51.2 percent received a monthly transfer (in quarterly payments) of LSL 120 ($12), 38.8 percent received LSL 200 ($20), and 10 percent received LSL 250 ($25). In our sample the average CGP transfer amount is LSL 164 ($16.4) per month. The CGP-eligible households received an additional monthly transfer of LSL 200 from Department for International Development (DfID), UK and United Nations' Central Emergency Response Fund (CERF) as a Food Emergency Grant. This came as a bimonthly payment of LSL 400 ($40) in a separate envelope disbursed with the CGP (Pellerano et al., 2014).

Table 1: Distribution of Eligible Households in Treated Clusters by CGP Transfer Amount

CGP Monthly Transfer	Number of Children	% of Total Eligible Households
120 LSL ($12)	1-2	51.2
200 LSL ($20)	3-4	38.8
250 LSL ($25)	5+	10.0

Net income from each household production activity is the difference between total revenue and the cost of purchased inputs. Household total income is the sum of this and household members' wage income, rents received, and private and public transfers (including CGP transfers). Total and activity-specific incomes are the outcome variables in our econometric analysis. An alternative to constructing household incomes in this way is to use information on expenditures, also gathered in the surveys. In economies characterized by a high degree of seasonality in income and consumption, however, expenditure data, much of which is based on 7-day recall, is not likely to be a reliable basis for estimating annual income. Irregularity of CGP payments and a time lapse since last payment averaging 4 months at the time of the follow-on survey add to the difficulty of identifying consumption impacts.

The baseline survey provides information on household assets and social participation. Given asset heterogeneity, we construct an index of physical assets, including agricultural machinery and tools, house ownership status, type of floor, wall construction materials, and access to electricity. The index for each group was constructed using principal components analysis. Similarly, we construct a social network index using information on households' participation in giving and receiving food, labor and agricultural or non-agricultural inputs as gifts.

1.1. Baseline Summary Statistics and Balance

We use data from the baseline survey to compare selected characteristics of eligible and ineligible households in the treatment and control clusters. A more extensive and detailed presentation of summary statistics on household characteristics appears in Daidone et al. (2014). Table 2 compares baseline summary statistics on variables likely to influence income outcomes for eligible and ineligible households.

The asset and social network indexes for eligible households in control clusters have a normalized value of 100. CGP-ineligible households in the treatment clusters have 36% more physical capital, represented by the asset index, than CGP-eligible households. Social participation is higher for ineligible than eligible households in both treatment and control clusters. Differences in normalized asset and social network indexes between treatment and control clusters are not statistically significant for either household group.

Table 2: Summary Statistics of Baseline Household Characteristics of Each Group Used as Control Variables

Summary Statistics	Eligible Group			Ineligible Group		
	Control	Treatment	*Diff*	Control	Treatment	*Diff*
Normalized Asset Index	100	103.3	-3.3	149.6	140	9.6
Normalized Social Network Index	100	101.3	-1.3	120.9	110.5	10.4
Household Size	5.5	5.8	-0.3**	5.1	5.3	-0.2
Land Owned (Acres)	1.2	1.4	-0.2**	1.4	1.9	-0.5***
Total Livestock Units (TLU)	0.8	1.0	-0.2**	1.8	2.2	-0.4*
Average Education of members (0-17 years)	3.3	3.2	0.1	3.1	3.1	0.0
Average Education of members (18-59 years)	5.9	5.7	0.2	6.3	6.4	-0.1
Proportion of Female Headed Households	0.5	0.5	0.0	0.4	0.3	0.1
Age of Household Head	51.9	52.0	-0.1	55.6	57.4	-1.8

*Significant- * at 10%, ** at 5%, *** at 1% respectively*

Household size is about 5 percent smaller for the eligible group in control clusters than in treated clusters. Eligible households in the control clusters also own less land and livestock than eligible households in the treated clusters. These differences are statistically significant at the 5% level. Among ineligible households, there is evidence of smaller land (significant at the 5% level) and livestock (significant at the 10% level) holdings in the control cluster. For other variables—average education, age of household head, proportion of female-headed households—we do not see any significant differences between treatment and control clusters, for eligible or ineligible households.

A test of balance using baseline summary statistics suggests that, while there are some significant differences for each group between treated and control clusters, most baseline variables are not different, suggesting that the rollout of the treatment was mostly random. We control for baseline variables in our analysis to isolate the impact of transfers on income. Part of the CGP's impact on income could be through asset accumulation. Previous studies suggest that rural households invest in assets that provide income returns (Baulch and Hoddinott, 2000; Rosenzweig and Wolpin, 1993; Adams Jr, 1998), and these returns could be both short and long-term (Taylor, 1992) depending on household-specific risks, consumption and asset smoothing patterns (Carter and Barrett, 2006; Zimmerman and Carter, 2000). Asset accumulation could have a short-term negative impact on income due to investment in assets; however, by increasing the asset holdings of households in treated clusters, the CGP could generate long-term income benefits.

For budgetary reasons, there was a reduction in the random sample of ineligible households from 1569 in 2011 to 797 in 2013.[8] This undoubtedly reduces the precision of our estimates. We account for this in our regression analyses by including a switching household weight for ineligible households and an eligibility ratio at the cluster level. We adjust the household weights by calculating the probability of households remaining in the sample based on

[8] The reduction in sample size for ineligible households in the follow-on survey reflects the difficulty of convincing donors to budget for data collection to identify spillovers from social programs; hence, the paucity of data like those used in our analysis of the CGP.

characteristics at baseline. Details on attrition and selection in the sample of eligible and ineligible households appear in Pellerano et al. (2014). In addition, we include a ratio of eligible to ineligible households at the cluster level to control for any sampling error due to the reduction in sample size. The reduction in the ineligible household sample size lowers the degrees of freedom in our analysis; however, inasmuch as the sample of ineligible households in the follow-on survey was random, it does not result in biased estimates. We control for baseline confounding factors that might influence income changes.

1.2. Household Monthly Income at Baseline and Follow-up

Table 3 presents average incomes of eligible and ineligible households in treatment and control clusters at baseline (2011) and follow-up (2013). Household income sources include production activities such as crops, livestock, retail and other services; wage work; and private and non-CGP public transfers. Eligible households' income also includes CGP transfers, distributed quarterly, and FMG payments, given bi-monthly, in the treatment clusters. As mentioned earlier, the amount of CGP transfers depended on the number of resident children in the household, but every eligible treated household received the FMG transfer of LSL 200 monthly.

A comparison of incomes (Table 3) shows that on average, for both eligible and ineligible households, incomes in the treated village clusters were higher at follow-on than baseline. Average incomes also increased for both groups in the control clusters. The increase in income was larger for eligible households in the treated than control clusters; the first differences are LSL 498.6 ($49.9) and LSL 110.9 ($11.1), respectively. An increase in average household income in the control clusters between the baseline and follow-on surveys likely reflects a general trend in income growth during this period in Lesotho. According to the UNDP Human Development Report (2014), PPP-adjusted per-capita GNI increased from $2605 in 2011 to $2798 in 2013.

A naïve DD estimate reveals an income impact for eligible households of LSL 387.7 ($38.7), statistically significant below the 1% level. The DD estimate for ineligible households is insignificant. However, naïve DD estimates do not control for household characteristics found to be different at baseline between treatment and control clusters, including household size, land owned and TLU for eligible households, and land owned and TLU for ineligible households (Table 2). Controlling for these requires a regression DD approach.

Table 3: Monthly Income of Household Groups (in LSL)

Survey Round	Treated Clusters		Control Clusters	
	Eligible	Ineligible	Eligible	Ineligible
2013 (Follow-up)	905.4	792.8	555.4	932.9
2011 (Baseline)	406.8	661	444.5	732.5
Difference between 2013 and 2011	498.6*** (0.000)	131.8** (0.025)	110.9*** (0.001)	200.4*** (0.004)
Difference-in-difference	387.7*** (0.000)	-68.7 (0.450)		

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2. Estimation Strategy

We use ordinary least squares estimation under a variety of specifications to estimate the impact of CGP transfers on the treated local economy, which includes both eligible and ineligible households. The regressions control for household-specific baseline characteristics that could lead to potential endogeneity and biased estimates. Randomization of the CGP at the cluster level provides a basis for identifying the impact. We disaggregate the average treatment effect on the treated clusters into impacts on eligible and ineligible households.

Heterogeneous transfers based on the number of resident children make it possible to evaluate intensity-of-treatment effects on the eligible households. Our preferred strategy is to include the intensity variable (amount of CGP transfer received by eligible households) in a regression with both eligible and ineligible households. Later, we test the robustness of this specification by estimating the intensity of treatment in a sample restricted to eligible households. We also perform regression DD estimation by income source and use QTE to test for heterogeneity of impacts across the income distribution for both eligible and ineligible household groups, after testing for rank similarity.

2.1. Estimation of the Average Treatment Effect on the Treated Economy

We estimate the average effect of the CGP treatment on the local economy using data on eligible and ineligible household incomes in the treatment and control clusters from the baseline (2011) and follow-up (2013) surveys. Consider the following equation:

$$(1) \qquad D_{ijt} = \alpha + \beta T_j + \rho Y_t + \theta\left(T_j Y_t\right) + \epsilon_{ijt}$$

with variables defined as follows:

D_{ijt}: Income of household i in cluster j and year t

T_j: Treatment dummy equal to 1 if household is in a treatment cluster j, 0 otherwise

Y_t: Year dummy, 1 for follow − on and 0 for baseline

ϵ_{ijt}: Idiosyncratic error term

In equation (1), α is the average income of households in the control clusters. The parameter ρ is the change in average monthly income due to the time effect. The parameter on the year-treatment interaction term, θ, represents the average effect of the treatment on the treated local economy (ATTLE). This parameter is akin to a DD estimate of the average treatment effect on the treatment group if there are only treatment and (treatment-eligible) control groups. However, each cluster also contains ineligible households. Income spillovers in theory may accrue to eligible or ineligible households, but the ATTLE does not separate these impacts by household group. Spillovers to eligible households are implicit in measured impacts from conventional evaluations that exclude ineligible households—that is, they are a (generally unknown) part of the average effect of a treatment on the treated.

2.2. Disaggregating ATTLE

In equation (2), we decompose the ATTLE into impacts on eligible and ineligible households by replacing θ with $\gamma + \xi E_i$, where E_i is an indicator variable equal to one if household *i* is eligible for the transfers and zero otherwise. This decomposition separates the ATTLE into impacts on ineligible (γ) and eligible ($\gamma + \xi E_i$) households:

$$(2) \qquad D_{ijt} = \alpha + \beta T_j + \rho Y_t + \gamma\left(T_j Y_t\right) + \xi T_j Y_t E_i + \epsilon_{ijt}$$

In equation (2), γ represents the average effect of the treatment on the ineligible households in the treated clusters, and $(\gamma + \xi)$ is the average treatment effect on the eligible households. Note that we do not have a separate eligibility indicator, because our objective is to evaluate the treatment effect at the village-cluster and not the individual-household level. We do not run separate regressions for the eligible and ineligible groups for the same reason. A separate regression with ineligible households might produce biased estimates due to the omission of eligible households.

We can introduce intensity of treatment due to varying levels of transfers by controlling for the amount transferred, $CGP_{i,t}$:

$$(3) \qquad D_{ijt} = \alpha + \beta T_j + \rho Y_t + \gamma\left(T_j Y_t\right) + \xi T_j Y_t E_i + \delta T_j Y_t E_i CGP_{i,t} + \epsilon_{ijt}$$

Note that $CGP_{i,t}$ can be written as $T_j Y_t E_i CGP_{i,t}$, because eligible households in treated clusters only receive the transfers in 2013. The parameter δ captures the intensity of treatment on eligible households. Equation (3) is our preferred specification, which we later use to obtain multiplier and spillover effects in the treated clusters and compare experimental findings with the simulation results from Filipski et al. (2015).

Households spend some income on assets that could generate future income; thus, we control for household specific baseline variables. In equations (1)-(3), we allow the intercepts to be household-specific, i.e., $\alpha = (\alpha_1 + \emptyset X_{i,2011})$, where $X_{i,2011}$ is a vector of baseline household characteristics that might influence income outcomes, including the asset and social network indices and a set of household characteristics described earlier. We only include the baseline values of these variables; also including follow-on values could lead to endogeneity bias through simultaneity if some of the program's impact on income is via changes in baseline variables.

Table 4: Definition of Parameter Estimates from Equations (1)-(3)

Used in Equation	Parameter	Definition
(1), (2) & (3)	α	Intercept
(1), (2) & (3)	β	Treatment cluster dummy
(1), (2) & (3)	ρ	Yearly dummy
(1)	θ	ATTLE
(2) & (3)	γ	Average Effect for being in treatment cluster
(2) & (3)	ξ	Average Effect for being eligible
(3)	δ	Intensity of Treatment on treated eligible household
-	α_1	Intercept when allowed for baseline variables
-	$\emptyset$	Vector of parameter estimates on baseline variables

Table 4 defines the parameters in equations (1)-(3). In equation (3), which includes the intensity of treatment, $(\gamma + \xi + \delta * CGP_{i,t})$ is the treatment effect on the eligible households. Equation (2) does not allow for treatment intensity; $(\gamma + \xi)$ captures the total average impact on eligible households. The average treatment effect for ineligible households in both equations (2) and (3) is captured by γ. The inclusion of ineligible households makes it possible to identify direct impacts on eligible households while also identifying spillover effects on ineligible households in the treated clusters.

2.3. Heterogeneous Treatment Effects

The average treatment effect does not offer insight into heterogeneous impacts due to households' relative position in the income distribution. Heckman, Smith and Clements (1997) argue that answers to some of the most interesting evaluation questions require information on the distribution of program gains. We use the regression framework in equation (2) and (3) to obtain QTEs on the treated eligible and ineligible households.

Identifying and estimating distributional effects of cash transfer treatments requires the assumption of rank invariance or more generally rank similarity. Rank invariance implies that households' rank with respect to monthly income is the same in both treated and control cluster income distributions before and after the treatment. Rank similarity, a weaker condition, implies that, post intervention, the distribution of ranks remains identical in treatment and control clusters conditional on observables that might have confounding effects on monthly income (Dong and Shen, 2015; Frandsen and Lefgren, 2015). Incomes are correlated with household characteristics including household size, composition, and human, physical and social capital prior to cash transfers. Rank similarity implies that after cash transfers the income distribution of the treatment group is shifted to the right in a uniform manner without changing the relative position of a household in the original rank distribution. Rejection of rank similarity on some covariate implies that cash transfers benefit some households differently than others. For example, failing to reject rank similarity on higher levels of our constructed asset index would imply that households with larger asset holdings benefit more from cash transfers than those do with smaller holdings. We use a regression-based test for rank similarity (Frandsen and Lefgren, 2015).

Identification in our quantile analysis relies on the assumption that rank similarity holds for both eligible and ineligible households.[9] Since the rollout of CGP was random and that we adjust for any remaining biases by including the baseline covariates in our estimations, we can assume treatment unconfoundedness.

We describe the regression-based test procedure for eligible households. The one for the ineligible households is analogous.[10] Let $\hat{F}_j$ be the estimated cumulative density function (cdf) of monthly income for eligible households in cluster j where $j = \{0, 1\}$. First we construct sample ranks as

$$\hat{U}_{ijt} = (1 - T_j)\hat{F}_0(D_{ijt}) + T_j\hat{F}_1(D_{ijt})$$

Then we estimate the following regression, where X_i' denotes the values of observable variables before the intervention:

$$\hat{U}_{ijt} = \alpha_0 + \alpha_1 T_j + X_i'\Gamma + T_j X_i'\Theta.$$

$\hat{\Theta} = (W'W)^{-1}W'\hat{U}_{ijt}$ where $W_{ijt}' = (1 \quad T_j \quad X_i' \quad T_j X_i')$. We test the following null hypothesis for rank similarity conditional on covariates X_i', against the alternative that the rank similarity assumption is violated:

$$\mathrm{H}_0: \Theta = \mathbf{0}$$

We perform the above test for ineligible as well as eligible household groups then perform QTE. For the eligible households, the quantile treatment effects are obtained for the incidence as well as intensity of treatment. We do

[9] Kernel density plots of income distribution for eligible households and ineligible households appear in Appendix Figure A1 (a) and (b).
[10] For details of how to construct the test, see Frandsen and Lefgren (2015).

not need to perform the rank similarity test separately for treatment intensity, since transfer levels are indexed on the number of resident children and we include household composition as a covariate.[11]

3. Econometric Results

Table 5 reports the results from estimating equation (1) to obtain the ATTLE. The table shows the impacts of transfers on monthly nominal as well as real (CPI-adjusted) income. The models corresponding to columns (3)-(4) include district fixed effects, household weights, and the cluster eligibility ratio. Our preferred specification, in columns (5)-(6), includes these as well as the household baseline variables. The results in Table 5 reveal that the estimates are robust across these different specifications.

Table 5: Average Effect of Treatment on the Treated Local Economy

Estimates	(1) Nominal Income	(2) Real Income	(3) Nominal Income	(4) Real Income	(5) Nominal Income	(6) Real Income
*T*Y (θ)*	218.9***	177.2***	224.3***	181.6***	249.5***	202.0***
	(45.37)	(36.73)	(45.07)	(36.49)	(52.25)	(42.29)
Y (ρ)	146.6***	118.6***	136.8***	110.8***	949.9***	768.9***
	(34.74)	(28.12)	(42.73)	(34.59)	(97.25)	(78.73)
T (β)	-56.74**	-45.93**	-60.82**	-49.24**	-86.53**	-70.04**
	(28.38)	(22.98)	(27.79)	(22.49)	(34.44)	(27.88)
Constant	554.5***	448.9***	503.1***	407.3***	-354.8***	-287.2***
	(22.30)	(18.05)	(43.23)	(35.00)	(98.99)	(80.13)
N	4210	4210	4210	4210	4210	4210
R-squared	0.036	0.036	0.052	0.052	0.104	0.104
Baseline Household Characteristics	No	No	No	No	Yes	Yes
District Fixed Effects	No	No	Yes	Yes	Yes	Yes
Household Weights	No	No	Yes	Yes	Yes	Yes
Cluster Eligibility Ratio	No	No	Yes	Yes	Yes	Yes

Cluster robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Income in each column is monthly household income, either nominal or real (inflation-adjusted). Cluster eligibility ratio is the ratio of eligible to ineligible households in each cluster. Household weights are different for eligible and ineligible households to account for attrition in the follow-up survey (details in Pellerano et al., 2014). Four district dummies are included and there are a total of 127 clusters. Full regression results appear in Appendix Table B1.

The ATTLE is LSL 249.5 ($25) in nominal and LSL 202 ($20.2) in real income terms, significant at well below the 0.01 level. The ATTLE are significantly higher than total transfers in the treated clusters, which averaged LSL 81.9 ($8.2) per household. Monthly transfers in the treated clusters include both CGP and FMG payments. Ineligible households are included to calculate the average transfer in the treated local economy. The difference between the ATTLE and the transfer amount (LSL 167.6 ($16.7) nominal and 120.1 ($12) real per household) is evidence of income spillovers in treated clusters. The numbers in columns in columns (1)-(4) probably represent lower bounds on spillover estimates due to omission of the baseline covariates, some of which were found to be significantly different (Table 2).

[11] Kernel plots of income distribution for eligible households with different children size are in Appendix Figure A2 (a), (b) and (c).

Table 6: Econometric Results from Estimation of Equation (2)

Parameter Estimates	(1) Nominal Income	(2) Real Income	(3) Nominal Income	(4) Real Income	(5) Nominal Income	(6) Real Income	(7) Nominal Income	(8) Real Income
*T*Y* (γ)	87.19	70.58	143.5**	116.1**	120.1*	97.25*	143.5**	116.2**
	(61.93)	(50.13)	(67.49)	(54.63)	(63.10)	(51.08)	(67.46)	(54.61)
T (β)	-59.94*	-48.52*	-85.70**	-69.38**	-85.38**	-69.12**	-85.46**	-69.18**
	(35.25)	(28.54)	(34.49)	(27.92)	(34.53)	(27.96)	(34.50)	(27.93)
Y (ρ)	137.4***	111.2***	943.1***	763.4***	940.5***	761.3***	941.2***	761.9***
	(46.09)	(37.31)	(97.38)	(78.83)	(97.49)	(78.92)	(97.40)	(78.85)
*T*Y*E* (ξ)	218.5***	176.9***	165.6***	134.1***			-177.3	-143.5
	87.19	70.58	(60.49)	(48.97)			(120.5)	(97.56)
*T*Y*E*CGP* (δ)					1.225***	0.992***	2.079***	1.683***
					(0.306)	(0.248)	(0.588)	(0.476)
Constant	485.0***	392.6***	-358.4***	-290.1***	-355.8***	-288.0***	-352.6***	-285.5***
	(52.93)	(42.85)	(99.59)	(80.62)	(99.59)	(80.62)	(99.57)	(80.60)
N	4210	4210	3893	3893	3893	3893	3893	3893
R-squared	0.057	0.057	0.106	0.106	0.108	0.108	0.109	0.109
Impact on Mean Eligible with 120LSL					267.1***	216.3***	215.7***	174.7***
Impact on Mean Eligible with 200LSL					365.1***	295.7***	382.0***	309.3***
Impact on Mean Eligible with 250LSL					426.4***	345.3***	486.0***	393.5***
Impact on Mean Eligible with 164LSL (average transfer)	218.5***	176.9***	165.6***	134.1***	321.0***	259.9***	307.2***	248.7***
Impact on Ineligible	87.2	70.6	143.5**	116.1**	120.1*	97.3*	143.5**	116.2**
Baseline Household Characteristics	No	No	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Eligibility Ratio	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Cluster robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Income in each column is monthly household income, either nominal or real (inflation-adjusted). Cluster eligibility ratio is the ratio of eligible to ineligible households in each cluster. Household weights are different for eligible and ineligible households to account for attrition in the follow-up survey (details in Pellerano et al., 2014). Four district dummies are included and there are a total of 127 clusters. Columns (1)-(4) do not allow for the intensity of CGP treatment effect and impact on eligible households are calculated are the average effects. Columns (5)-(8) include the intensity of treatment at different levels of CGP transfers by including the transfer amount. Full regression results appear in Appendix Table B2.

The ATTLE gives the average effect of the CGP treatment on total income in the treated clusters. Table 6 reports the results of disaggregating this average total effect into impacts on eligible and ineligible households using equations (2) and (3). The top panel in Table 6 presents the parameter estimates, while the bottom panel reports impacts on eligible and ineligible households, allowing for treatment intensity on eligible households. The first four columns in Table 6 present findings for equation (2), in which we decompose the ATTLE into average treatment effects on ineligible and eligible households. Columns (4)-(8) present the results for equation (3), which includes the intensity of treatment effect.

The average treatment effect on ineligible households, given by the parameter γ, is statistically significant in columns (3)-(8). It is not significant but has the expected sign in columns (1)-(2), which do not control for baseline variables significant in explaining variations in household income, some of which differ between treatment and control clusters for ineligible households (Table 2).[12] Results are robust, both in magnitude and significance, between columns (3)-(4) and (7)-(8).

The average treatment effects on ineligible households' nominal and real incomes are LSL 143.5 ($14.3) and LSL 116.2 ($11.6), respectively. In columns (5)-(6), where we exclude the triple interaction term and only allow for intensity of treatment, the impact on ineligible households is significant though slightly smaller in magnitude. Although the estimated parameters on the triple interaction term in (7)-(8) are not significant, this is our preferred specification because it provides consistent and conservative estimates of impacts on the treated households. The results provide evidence of positive and significant income spillovers to ineligible households in the treated clusters.

Columns (1)-(4) report the treatment effect on eligible households receiving the average transfer. The increase in mean income for an eligible household is statistically significant and positive. Allowing for treatment intensity gives differential impacts on eligible households by amount of transfer received. Income in all cash recipient households increases significantly more than the amount of the transfer, regardless of whether we evaluate impacts on nominal or real income or use different model specifications. For a household receiving LSL 120, inflation-adjusted income increases by LSL 174.7 ($17.5), or about 46% more than the original transfer amount. In households receiving LSL 200 and LSL 250, respectively, household incomes increase by 55% and 57% more than the transfer amount. These findings imply significant and positive income multipliers in the treated households.

3.1. Heterogeneous Impacts by Income Source

The CGP's impact on both eligible and ineligible households depends on income source. Most households with higher incomes are livestock herders, while those in the lower tail of the income distribution typically are engaged in crop production, wage work or self-employment. We estimated equation (3) to obtain impacts on activity-specific real income earned by eligible and ineligible households in treated clusters, specifically, livestock income, wage income, and income from crop and self-employment. These regressions restrict the sample to the households that participated in each income activity. The DD results are reported in Table 7 with real (inflation-adjusted) activity income as the dependent variable. The regressions use the same controls as in columns (7)-(8) of Table 6, including the baseline household variables.

We find significant CGP effects on ineligible households' income via livestock production. The CGP creates monthly livestock-income spillovers of LSL 48.6 ($4.9) for this group. For non-livestock activities, spillovers to ineligible households are not significantly different from zero. The impact on livestock income appears to be shaped by the eligibility criteria of the program, which targets asset-poor (including livestock-poor) households. There is

[12] For full regression results, see Appendix Table B2.

no significant impact via livestock production for eligible households at any level of CGP transfer. It appears that eligible households are too asset poor to reap spillovers in the form of higher livestock income. There are no impacts on earnings from wage work in eligible or ineligible households. The estimated impacts are positive for the ineligible group and for eligible households with transfers greater than LSL 120, but they are not significant.

Table 7: DD Impact of CGP on Eligible and Ineligible Households from Equation (2), by Income Source with Real Income as Dependent Variable

CGP Impacts on Real (Inflation-adjusted Income)	Income from Livestock	Income from Wage Work	Income from Only Crop and Self-employment
Impact on Ineligible households (θ)	48.6***	18.73	-121.6
	(0.007)	(0.745)	(0.298)
Impact on Eligible households at LSL 120 ($\gamma + \xi + \delta \cdot 120$)	-0.6	-9.1	112.8
	(0.958)	(0.866)	(0.192)
Impact on Eligible households at Mean ($\gamma + \xi + \delta \cdot 164$)	-0.3	13.8	198.3**
	(0.975)	(0.788)	(0.012)
Impact on Eligible households at LSL 200 ($\gamma + \xi + \delta \cdot 200$)	-0.1	32.7	268.6***
	(0.992)	(0.531)	(0.002)
Impact on Eligible households at LSL 250 ($\gamma + \xi + \delta \cdot 250$)	0.2	58.9	365.9***
	(0.989)	(0.300)	(0.001)
N	2487	1430	882

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is real (inflation-adjusted) activity-specific monthly income. All specifications control for baseline household characteristics, district fixed effects, household weights, and cluster eligibility ratio. Full regression results appear in Appendix Table B3.

The CGP generates significant positive income multipliers for eligible households involved in agricultural activities or earning income primarily from farm or non-farm work, including self-employment. The magnitude of the impact depends on the level of CGP transfer. The impact is significant and large for households receiving LSL 200; they reap an additional real income spillover of LSL 68.7 ($6.9). It is almost twice as large, LSL 115.9 ($11.6), for a household receiving LSL 250. The impact on those receiving LSL 120 is positive but not statistically significant.

3.2. Heterogeneous Impacts Along the Income Distribution

One might expect income spillovers from cash transfer programs to be distributed differently among eligible than ineligible households. Among eligible households, we would expect to find the largest benefits in households for which cash transfers loosen production constraints most. CGP transfers do not have a direct impact on production constraints in ineligible households, so positive income spillovers to ineligible households are likely to favor those with the resources to increase their supply of goods and services in response to the program's stimulus to local demand.

We carry out the regression-based rank similarity test (following Frandsen and Lefgren, 2015) as proposed in subsection 3.3 and present the results of the test for eligible (Appendix Table C1) and ineligible (C2) households, respectively. The null hypothesis of rank similarity is rejected if the parameter estimate on the interaction term involving the treatment T and covariates X_i' at baseline is statistically different from zero, i.e., the vector $\Theta \neq \mathbf{0}$.

Rank similarity holds for eligible households (Table A1); the interaction terms are not significantly different from zero for any of the included covariates, and we fail to reject the null hypothesis of rank similarity. The results hold at mean income as well as at the three different transfer quantiles (0.25, 0.5 and 0.75), demonstrating that eligible households in treated and control clusters exhibit rank similarity with respect to the baseline covariates at different positions in the income distribution. However, we find that the rank similarity condition for ineligible households (Table A2) fails for average adult education, household size and household head age. Although the rank similarity condition fails, the regression test implies that ineligible households with higher average adult education, more household members, and older household heads benefit most from the CGP treatment.

We carry out the QTE estimation using all of these covariates as controls, as suggested by Frandsen and Lefgren (2015), to measure heterogeneous treatment effects on three different income quantiles: 0.25, 0.5 (the median) and 0.75. Measuring inter-quantile impacts may be complicated due to partial identification (see Callaway and Li, 2015), and we do not perform any inter-quantile treatment effects in this paper. The QTE estimation is based on equations (2) and (3). Results appear in Table 8.

Table 8: Quantile Treatment Effects on the Treated Eligible and Ineligible Households

Dependent Variable: Real Income	*Quantile = 0.25*		*Quantile = 0.50*		*Quantile = 0.75*	
*T*Y (γ)*	-15.11	-16.83	77.39*	76.10*	159.4**	159.6**
	(24.16)	(22.72)	(43.45)	(45.84)	(68.80)	(65.52)
T (β)	-8.452	-8.030	-27.25	-25.62	-74.41*	-74.62**
	(13.87)	(13.05)	(24.95)	(26.32)	(39.51)	(37.62)
Y (ρ)	136.9***	137.8***	561.1***	565.8***	930.6***	930.5***
	(45.79)	(43.06)	(82.36)	(86.88)	(130.4)	(124.2)
*T*Y*E (ξ)*	156.8***	316.0***	-213.8**	168.1***	-324.2**	140.6**
	(48.32)	(20.61)	(86.92)	(41.58)	(137.6)	(59.43)
*T*Y*E*CGP (δ)*	0.998***		2.317***		2.481***	
	(0.261)		(0.470)		(0.744)	
Constant	-73.39	-75.61*	-232.3***	-243.9***	-262.0**	-261.8**
	(46.34)	(43.57)	(83.34)	(87.91)	(132.0)	(125.6)
N	3893	3893	3893	3893	3893	3893
R-squared	-	-	-	-	-	-
Impact on Mean Eligible with 120LSL	261.5***		141.6***		132.9*	
Impact on Mean Eligible with 200LSL	341.3***		327.0***		331.4***	
Impact on Mean Eligible with 250LSL	391.2***		442.8***		455.5***	
Impact on Mean Eligible with 164LSL (average transfer)	305.3***	316.0***	243.3***	244.2***	241.8***	300.2***
Impact on Ineligible	0	0	77.39*	76.10*	159.4**	159.6**

Cluster robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: The dependent variable is real (inflation-adjusted) monthly income. All specifications control for baseline household characteristics, district fixed effects, household weights, and cluster eligibility ratio. Full regression results appear in Appendix Table B4.

The estimated impacts on ineligible households vary by income quantile. The impact is insignificant at the 0.25 quantile but large and significant for households at the median and 0.75 quantiles. These findings are consistent across the model specifications we consider. Ineligible households with low income fail to benefit from income spillovers. Column (8) in Table 6 shows that the impact on ineligible households at mean income is LSL 116.2 ($11.6), higher than the impact at median income, which is LSL 77.4 ($7.7). In short, most of the indirect benefits accrue to non-recipient households with higher monthly income.

In contrast, among eligible households the benefits from transfers are larger for those at the lower end of the income distribution. The quantile treatment effects on eligible households depend on the transfer level. The impact at the 0.25 quantile of eligible households receiving LSL 120 per month is LSL 261.5 ($26.2). At the same transfer level, the income effects are LSL 141.6 ($14.2) at the middle and LSL 132.9 ($13.3) at the 0.75 quantile. There is little variation in impacts across the eligible-household income distribution at the LSL 200 transfer level. At LSL 250, the largest benefits go to households at the upper end of the distribution. This transfer amount appears sufficiently large to loosen liquidity constraints on investments by less-poor eligible households.

4. Experiments and Ex-ante Simulations Compared

We use the results from column (7) and (8) in Table 6 to calculate income multipliers and spillovers for eligible and ineligible households and compare our experimental findings with the ex-ante simulations in Filipski et al. (2015). Table 8 reports the results. The *average* real and nominal multipliers for eligible households at the mean transfer level (LSL 164, or US$16.4) are 1.52 and 1.87, respectively. These are higher than the income multipliers for eligible households simulated ex-ante in Filipski et al. (2015), which are 1.03 and 1.15, respectively.[13] The nominal and real multipliers generated by experimental results are higher for larger transfers, and they are roughly proportional to the amount transferred.

We use the average transfer amount ($16.4) to derive an estimate of the multiplier effect on non-beneficiary incomes per dollar transferred to eligible households in the treated clusters. The bottom row of Table 9 reports that $1 given to an eligible household in the treated cluster generates spillovers of 71 cents in real income and 88 cents in nominal income, respectively.

Table 9. CGP Multipliers Implied by the Econometric Results

Household Type	CGP Transfer Amount (per month)	Real (Inflation-adjusted) Income Impact	Nominal Income Impact	Real Multiplier*	Nominal Multiplier*
Beneficiary	120	174.7	215.7	1.46	1.80
	164 (Mean)	248.7	307.2	1.52	1.87
	200	309.3	382.0	1.55	1.91
	250	393.5	486.0	1.57	1.94
Non-beneficiary	0	116.2	143.5	0.71	0.88

* For non-beneficiaries, the multiplier reported is the increase in income per maloti transferred to beneficiary households

Figure 2 shows multipliers on eligible-household income at different transfer levels and compares these to the results from the ex-ante LEWIE simulations. For eligible households the income multipliers implied by our experimental results are higher than the simulated multipliers at all transfer levels. The LEWIE simulations appear to understate the true impact of CGP transfers on the incomes of eligible households. The difference between the two is consistent with changes in household behavior and in the structure of local economies induced by the CGP

[13] We evaluate the multiplier at the average stepped-up transfer level, LSL 164 (US$16.4), whereas Filipski et al. used the original, LSL 120 level.

program. Because the LEWIE model is parameterized using data from the baseline survey, simulation results do not reflect these structural changes. We discuss this further in Section 6.

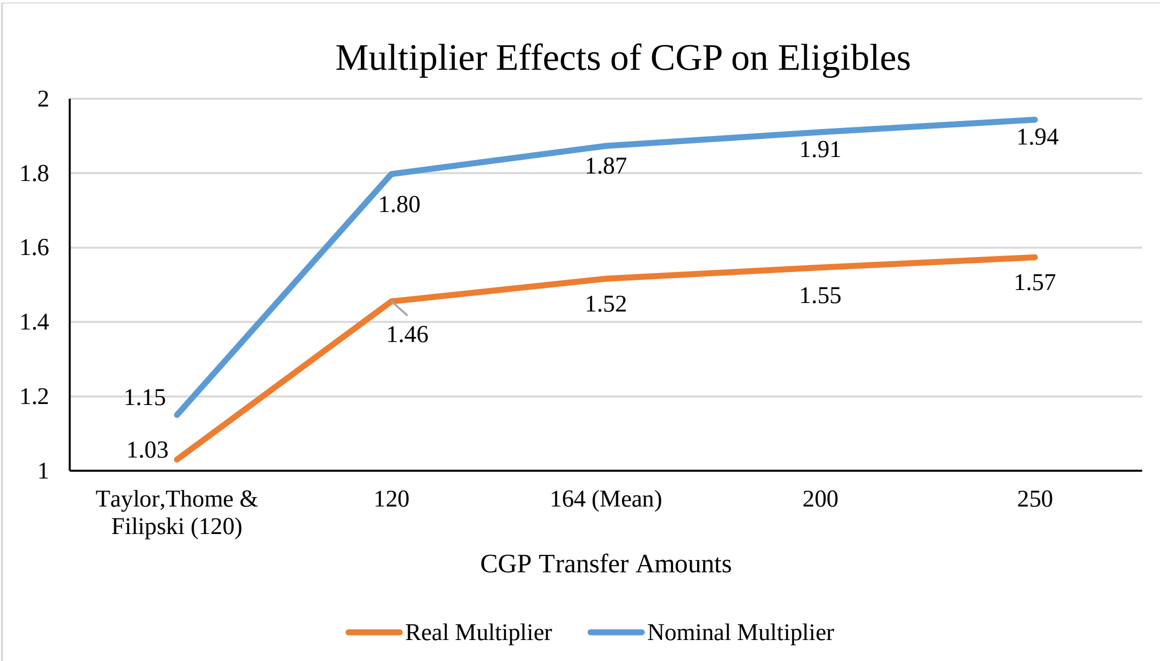


Figure 2: Real and nominal income effects of the CGP on beneficiary households in the LEWIE and experimental analyses

4.1. Total Income Multipliers in the Local Economy

The total local-economy income multiplier includes income effects on eligible households as well as income spillovers to ineligible households. Our experimental findings show that there is intensity of treatment effect on eligible households, i.e. heterogeneous impacts at different transfer amounts. We calculate the total impact on income in the local economy as the expected value of income effects for eligible households, with weights reflecting the population in each recipient category, plus income spillovers to ineligible households.

Ineligible households constitute a larger share of the local population than eligible households; the ratio of ineligible to eligible population sizes is 3.4:1. Because of this, a given average income gain for ineligible households translates into a larger total income gain for the local economy than does the same gain for eligible households.

Table 10 reports the total real and nominal multiplier impacts of CGP transfers in the treated clusters, and it compares them to the ex-ante simulation findings in Filipski et al. (2015). We obtain confidence intervals around multipliers using standard errors of parameter estimates in Table 6. The real multiplier obtained from experimental data is 1.86 with a confidence interval of (1.81, 1.91), while the nominal multiplier is 2.20 [CI: (2.14, 2.26)].

The nominal multipliers obtained from experiments are almost identical to the simulated LEWIE multipliers. The real multiplier estimated with experimental data is 1.86, which is higher than what Filipski et al. (2015) obtained using LEWIE simulations. In LEWIE models, price impacts are simulated for each household group according to consumption patterns and Filipski et al. (2015) use price deflators of 1.66 and 1.40, respectively for eligible and ineligible households. Using their price deflators for eligible and ineligible households, we obtain a real multiplier of 1.64 with a confidence interval of [1.60, 1.67]. Confidence intervals from the experimental estimates are narrower than from the LEWIE simulations.

Table 10. Local Economy Multipliers Compared

Estimation	Real Multiplier	Nominal Multiplier
Experimental results using baseline and follow-up data	1.86 (1.81, 1.91)	2.20 (2.14,2.26)
LEWIE simulation results using baseline data*	1.53 (1.43,1.62)	2.21 (2.07,2.39)

* Source: Filipski et al. (2015)

Note: We use Laspeyres price index of 1.24 for both household groups calculated using base quantities and prices from 2013 and 2011. Filipski et al. (2015) use 1.66 for recipients and 1.40 for non-recipients using the base level of prices only.

5. Discussion

The findings are robust with respect to model specification in both the OLS and quantile regressions. Panel (a) of Table 11 reports checks for the robustness of treatment effects on eligible households, and panel (b) does the same for the quantile specification used in Section 4.2. To check the robustness of the intensity of treatment, we estimate the treatment effect on the treated eligible households while restricting the sample to eligible households. We also allow for nonlinearity of impacts by including a squared CGP term. Comparing results in panel (a) of Table 11 with columns (5)-(8) of Table 6, we find that the CGP impact estimates are significant and positive in both, and they are comparable in magnitude. For example, Table 6 reports that in households receiving a transfer amount of LSL 120, real income increases by LSL 174.7 to LSL 216.3, depending on the model used. In Table 11 the range of impacts is LSL 224 to LSL 241.7. Findings for other transfer amounts are similarly robust. Because we base our total income multipliers on the findings in Table 6, our estimates are arguably conservative.

Panel (b) of Table 11 shows that the quadratic term is significant and negative for the 0.25 quantile, suggesting a diminishing positive impact of CGP transfers on eligible-household incomes. For quantile 0.5 and 0.75, the squared term is positive and the linear term is insignificant. Findings of treatment effects at different quantiles are robust and similar in magnitude.

The total real income multiplier estimated from experimental data is higher than in LEWIE simulations, but the nominal multipliers for both simulations and experiments are almost identical with tighter confidence bounds on the experimental outcomes. The LEWIE model assumes that the capital stock, behavioral parameters, production technologies, and local market structures remain the same. CGP-induced changes in any of these variables could alter income multipliers in ways not captured by ex-ante simulations using baseline data.

Daidone et al. (2014) found evidence that the CGP increased ineligible households' investments in some farm implements, including cultivators and scotch-carts, as well as in crop inputs, including pesticides and inorganic fertilizers. It stimulated their livestock sales, reduced their debt, increased their savings, and strengthened their participation in social networks, as suggested by an increase in both giving and receipt of in-kind support to/from other households in the treated clusters. We do not have follow-on information regarding the location of purchases and sales. An elastic supply response, consistent with small price changes, might induce households to purchase more goods and services locally, which could contribute to higher multipliers.

Other assumptions in ex-ante structural models might constrain simulated program multipliers. For example, as in conventional agricultural household models, land is treated as a fixed input in the LEWIE model. If new land can easily be brought into livestock and crop production, this could increase the supply elasticity and result in larger multipliers. Technological change that raises the productivity of inputs could have a similar effect.

Table 11: Alternative Specifications--Intensity of Treatment on Treated Eligible Households and Non-linear Effects of CGP

	(a) OLS Regressions *Only Eligible Households*			(b) Quantile Regressions *All Households*		
Dependent Variable: Real Income	(1)	(2)	(3)	(4) q = 0.25	(5) q = 0.5	(6) q = 0.75
*T*Y*	38.18			-12.86	77.99*	157.5**
	(93.44)			(23.30)	(43.12)	(68.87)
T	-44.82	-40.14	-41.26	-8.171	-27.66	-72.67*
	(28.65)	(26.43)	(28.69)	(13.40)	(24.80)	(39.61)
Y	513.9***	518.4***	517.3***	133.5***	559.8***	926.6***
	(86.81)	(85.46)	(86.79)	(44.24)	(81.86)	(130.7)
CGP	1.696***	1.870***	1.944**	2.864***	-0.479	-1.703
	(0.474)	(0.233)	(0.762)	(0.370)	(0.685)	(1.095)
CGP-squared			-0.000333	-0.00516***	0.00842***	0.0124**
			(0.00322)	(0.00166)	(0.00306)	(0.00489)
Constant	-72.47	-74.20	-73.76	-67.68	-231.2***	-256.3*
	(84.37)	(83.83)	(84.25)	(44.76)	(82.83)	(132.3)
N	2485	2485	2485	3893	3893	3893
R-squared	0.163	0.162	0.162	-	-	-
Impact on Mean Eligible with 120LSL	241.7***	224***	228.5***	256.5***	141.8***	131.7***
Impact on Mean Eligible with 200LSL	377.4***	374***	375.5***	353.5***	318.9***	312.9**
Impact on Mean Eligible with 250LSL	462.2***	467.5***	465.2***	380.6***	484.5***	506.8***
Impact on Mean Eligible with 164LSL (average transfer)	316.3***	306.7***	309.9***	318.1***	225.9***	211.7***
Impact on Ineligible	-	-	-	0	77.99*	157.5**

Cluster robust standard errors in parentheses
* p < 0.10, ** p < 0.05, *** p < 0.01
Note: The dependent variable is real (inflation-adjusted) monthly income. All specifications control for baseline household characteristics, district fixed effects, household weights, and cluster eligibility ratio. Panel (a) uses only eligible households and uses OLS regressions. Panel (b) uses quantile regression using data on all households. For specification (3) in panel (a), we use the estimate on CGP-squared in calculating the intensity of treatment even though it is not significant because we used it in our specification. Full regression results appear in Appendix Table B5.

We performed sensitivity analysis with the LEWIE model to test the importance of these modeling assumptions. Loosening land, capital, and technological constraints in crop and livestock production increases the real income multiplier, but not to the levels found in the experimental analysis. While the local supply response increases, larger supplies tend to put downward pressure on prices, which in turn mitigate profit and nominal income effects. However, lower price inflation raises real income multipliers. Thus, the main impact of increases in the local supply response is to narrow the gap between nominal and real multipliers. Sensitivity analysis findings thus tend to support the hypothesis that CGP-induced changes in household behavior and in the structure of local economies may be responsible for the higher multipliers obtained in the experimental analysis.

6. Conclusions

Our analysis using experimental data from Lesotho's Child Grants Program finds positive and significant income gains that significantly exceed cash payments to eligible households, including income spillovers to households not eligible for cash transfers. It corroborates ex-ante simulation findings from LEWIE methods. To our knowledge, this is the first time experiments have been used to validate general equilibrium simulations of income spillovers from development projects, and the first time SCT spillovers of any kind have been experimentally identified in a setting outside Latin America.

Local general-equilibrium models estimated from baseline survey data can provide insights into likely project impacts long before follow-on survey data become available, but they require assumptions about market closure, asset holdings and productivity parameters, as well as about household behavior. These may change as a result of the policy intervention, causing ex-post estimates of transfer multipliers to diverge from impacts simulated ex-ante. It appears that the CGP caused livestock production by ineligible households and crop and other production activities in eligible households to expand more than simulation models predicted. Most of the spillover benefits to ineligible households accrued at the upper end of the ineligible-household income distribution, while among eligible households spillovers were larger at the bottom of the income distribution.

Both experimental and simulation findings indicate that, in the case of Lesotho's Child Grants Program, a poverty intervention based on social cash transfers has the potential to generate real economic growth as well as social benefits in poor economies. That is a message the social welfare minister can take to the finance minister.

References

Abadie, Alberto, Joshua Angrist, and Guido Imbens. "Instrumental variables estimates of the effect of subsidized training on the quantiles of trainee earnings." *Econometrica* 70, no. 1 (2002): 91-117.

Adams Jr, Richard H. "Remittances, investment, and rural asset accumulation in Pakistan." *Economic Development and Cultural Change* 47, no. 1 (1998): 155-173.

Angelucci, Manuela, Dean Karlan, and Jonathan Zinman. "Microcredit impacts: Evidence from a randomized microcredit program placement experiment by Compartamos Banco." *American Economic Journal: Applied Economics* 7, no. 1 (2015): 151-182.

Angelucci, M. and De Giorgi, G. (2009), 'Indirect effects of an aid program: how do cash transfers affect ineligible households' consumption?', *American Economic Review*, 99:486-508.

Baulch, Bob, and John Hoddinott. "Economic mobility and poverty dynamics in developing countries." *The Journal of Development Studies* 36, no. 6 (2000): 1-24.

Callaway, Brantly, and Tong Li. "Quantile Treatment Effects in Difference in Differences Models with Panel Data." *Working Paper, Vanderbilt University* (2015).

Carter, Michael R., and Christopher B. Barrett. "The economics of poverty traps and persistent poverty: An asset-based approach." *The Journal of Development Studies* 42, no. 2 (2006): 178-199.

Case, Anne, and Angus Deaton. "Large cash transfers to the elderly in South Africa." *The Economic Journal* 108, no. 450 (1998): 1330-1361.

Chernozhukov, Victor, and Christian Hansen. "An IV model of quantile treatment effects." *Econometrica* 73, no. 1 (2005): 245-261.

Cummins, Joseph R. "Heterogeneous treatment effects in the low track: Revisiting the Kenyan primary school experiment." *Economics of Education Review* 56 (2017): 40-51.

Daidone, S., Davis, B., Dewbre, J. and Covarrubias, K. (2014). Lesotho's Child Grant Programme: 24-month impact report on productive activities and labour allocation. Rome: *Food and Agriculture Organization of the United Nations (FAO)*, http://www.fao.org/economic/ptop/publications/reports/en/.

Dammert, Ana C. "Heterogeneous impacts of conditional cash transfers: Evidence from Nicaragua." *Economic Development and Cultural Change* 58, no. 1 (2009): 53-83.

Davis, Benjamin, Marie Gaarder, Sudhanshu Handa, and Jenn Yablonski. "Evaluating the impact of cash transfer programmes in sub-Saharan Africa: an introduction to the special issue." *Journal of development effectiveness* 4, no. 1 (2012): 1-8.

Dewbre, J., Daidone, S., Davis, B., Miguelez, B., Niang, O. and Pellerano, L. (2015). Lesotho Child Grant Programme and Linking Food Security to Social Protection. Rome: *Food and Agriculture Organization of the United Nations (FAO)*, http://www.fao.org/economic/ptop/publications/reports/en/.

Dong, Yingying, and Shu Shen. "Testing for rank invariance or similarity in program evaluation: The effect of training on earnings revisited." *Unpublished working paper* (2015). https://bfi.uchicago.edu/sites/default/files/research/Rank_Invariance_Test_091815.pdf

Duflo, Esther, and Michael Kremer. "Use of randomization in the evaluation of development effectiveness." *Evaluating development effectiveness* 7 (2005): 205-231.

Filipski, M.J., Taylor, J.E., Thome, K.E. and Davis, B. (2015). "Effects of Treatment Beyond the Treated: A General Equilibrium Impact Evaluation of Lesotho's Cash Grants Program." *Agricultural Economics* 46, no. 2: 227-243.

Frandsen, Brigham R., and Lars J. Lefgren. "Testing rank similarity." *Unpublished working paper* (2015). https://economics.byu.edu/frandsen/Documents/testingranksimilarity20151111.pdf

Gertler, Paul. "Do conditional cash transfers improve child health? Evidence from PROGRESA's control randomized experiment." *The American Economic Review* 94, no. 2 (2004): 336-341.

Heckman, James J., Jeffrey Smith, and Nancy Clements. "Making the most out of programme evaluations and social experiments: Accounting for heterogeneity in programme impacts." *The Review of Economic Studies* 64, no. 4 (1997): 487-535.

Heckman, James J., and Richard Robb. "Alternative methods for evaluating the impact of interventions: An overview." *Journal of econometrics* 30, no. 1-2 (1985): 239-267.

Pellerano, L., M. Moratti, M. Jakobsen, M. Bajgar, and V. Barca. "The Lesotho Child Grants Programme Impact Evaluation: Follow-up Report." *Maseru: UNICEF-Lesotho (with EU funding and technical support from FAO). Prepared by Oxford Policy Management* (2014).

Pellerano L. (2011). *CGP Impact Evaluation, Sampling Design and Targeting Evaluation Research*. Oxford Policy Management.

Rosenzweig, Mark R., and Kenneth I. Wolpin. "Credit market constraints, consumption smoothing, and the accumulation of durable production assets in low-income countries: Investments in bullocks in India." *Journal of political economy* (1993): 223-244.

Schultz, T. Paul. "School subsidies for the poor: evaluating the Mexican Progresa poverty program." *Journal of development Economics* 74, no. 1 (2004): 199-250.

Skoufias, Emmanuel, Susan W. Parker, Jere R. Behrman, and Carola Pessino. "Conditional cash transfers and their impact on child work and schooling: Evidence from the progresa program in mexico [with comments]." *Economia* 2, no. 1 (2001): 45-96.

Taylor J. E., Thome K. and Filipski M. (2013). “Evaluating Local General Equilibrium Impacts of Lesotho’s Child Grants Programme.” *Food and Agricultural Organization of the United Nations (FAO)*, Rome (2013).

Taylor, J. Edward, Justin Kagin, Mateusz Filipski, Karen Thome, and S. Handa. "Evaluating general equilibrium impacts of Kenya’s cash transfer program for orphans and vulnerable children (CT-OVC)." *Department of Agricultural and Resource Economics, University of California, Davis* (2013).

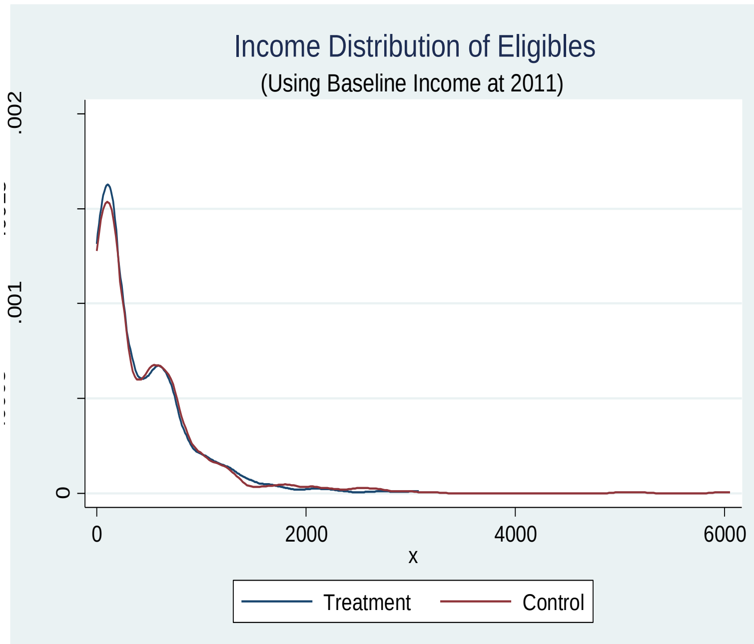
Thome, Karen, Mateusz Filipski, Justin Kagin, J. Edward Taylor, and Benjamin Davis. "Agricultural spillover effects of cash transfers: What does LEWIE have to say?." *American Journal of Agricultural Economics* (2013): aat039.

United Nations. (2014). *Human Development Report*. http://hdr.undp.org/sites/all/themes/hdr_theme/country-notes/LSO.pdf

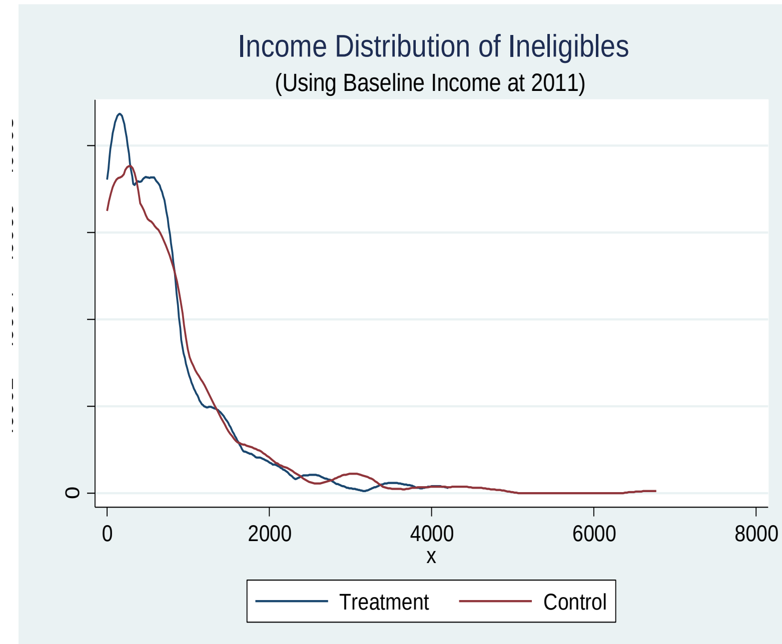
Zimmerman, Frederick J., and Michael R. Carter. "Asset smoothing, consumption smoothing and the reproduction of inequality under risk and subsistence constraints." *Journal of Development Economics* 71, no. 2 (2003): 233-260.

Appendix

Appendix A: Summary Figures



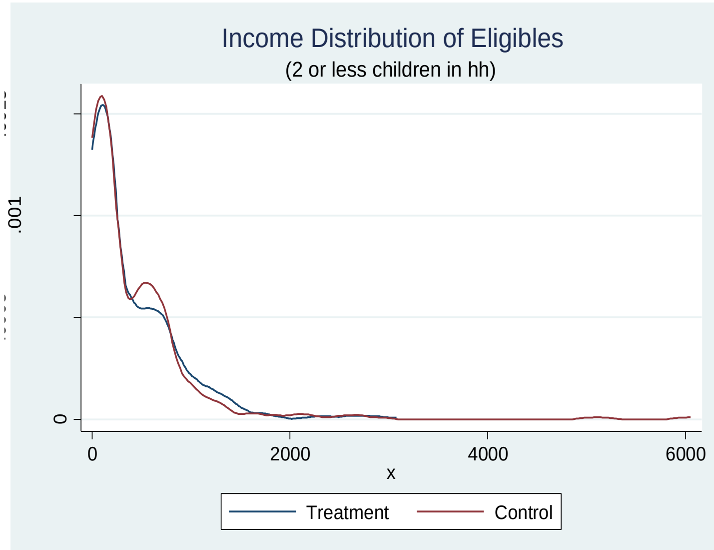
(a)



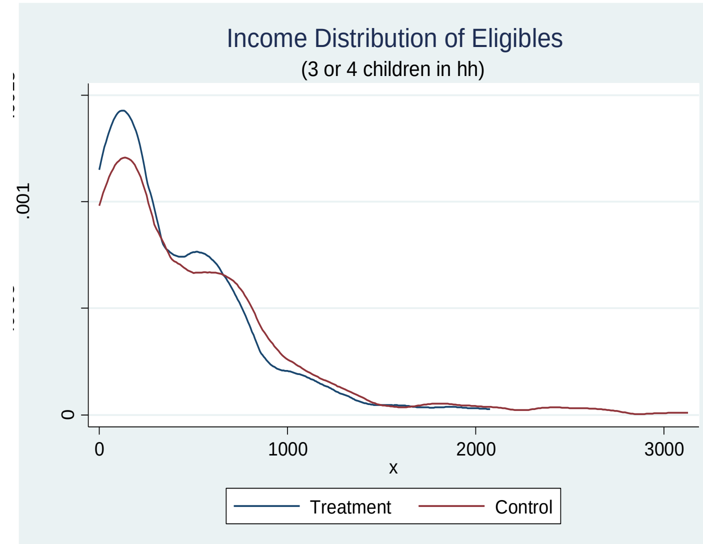
(b)

Figure A1: Kernel density plots of income distribution for eligible households and ineligible households

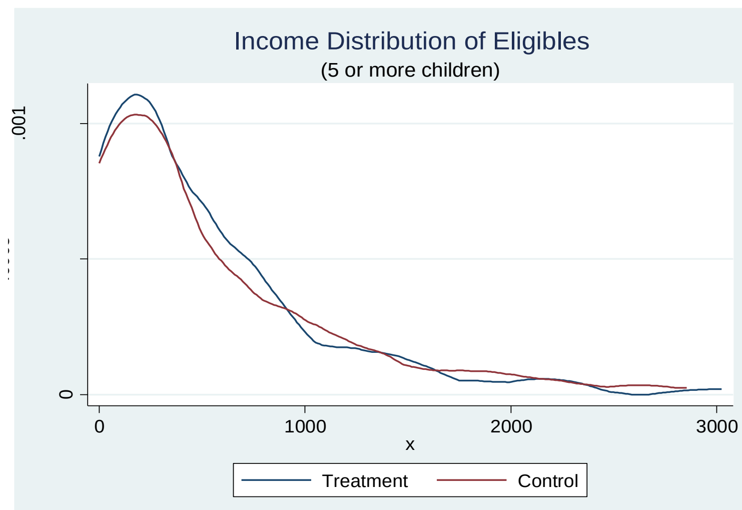
Both panels compare the income distribution between treatment and control groups for eligible households and ineligible households. The distributions are identical for most income deciles. We only use baseline incomes because the assumption is that the income distributions are similar for those treated versus the non-treated, for both eligible households and ineligible households.



(a) With LSL 120 CGP Transfers for Treated



(b) With LSL 200 CGP Transfers for Treated



(c) With LSL 250 CGP Transfers for Treated

Figure A2: Kernel density plots of income for treatment and control eligible households with different resident children

Appendix B: Full Regression Results

Table B1. Full Regression Results of Table 5 'Average Effect of Treatment on the Treated Local Economy'

Estimates	(1) Nominal Income	(2) Real Income	(3) Nominal Income	(4) Real Income	(5) Nominal Income	(6) Real Income
Y	146.6***	118.6***	136.8***	110.8***	949.9***	768.9***
	(34.74)	(28.12)	(42.73)	(34.59)	(97.25)	(78.73)
T	-56.74**	-45.93**	-60.82**	-49.24**	-86.53**	-70.04**
	(28.38)	(22.98)	(27.79)	(22.49)	(34.44)	(27.88)
*T*Y*	218.9***	177.2***	224.3***	181.6***	249.5***	202.0***
	(45.37)	(36.73)	(45.07)	(36.49)	(52.25)	(42.29)
Dummy Leribe			11.19	9.055	-2.266	-1.835
			(42.27)	(34.21)	(45.64)	(36.95)
Dummy Berea			-160.3***	-129.8***	-137.6***	-111.4***
			(37.45)	(30.32)	(37.11)	(30.04)
Dummy Mafeteng			129.6***	104.9***	71.73	58.07
			(43.89)	(35.53)	(56.73)	(45.93)
Dummy Qachas Nek			60.16	48.70	11.14	9.021
			(57.79)	(46.78)	(82.67)	(66.92)
Cluster Eligibility Ratio			38.92	31.50	71.70	58.04
			(84.26)	(68.21)	(102.6)	(83.09)
Household Weights			57.39***	46.46***	43.23***	35.00***
			(12.53)	(10.14)	(12.90)	(10.44)
Baseline Variables						
Asset Index					0.166	0.134
					(0.142)	(0.115)
HH members 0-5					95.48***	77.29***
					(30.44)	(24.65)
HH members 6-12					89.01***	72.05***
					(32.75)	(26.51)
HH members 13-17					74.29*	60.14*
					(39.06)	(31.62)
HH male mem 18-59					37.29	30.18
					(28.47)	(23.04)
HH female mem 18-59					144.8***	117.2***
					(28.67)	(23.21)
HH male mem >60					2.381	1.927
					(76.78)	(62.15)
HH female mem >60					318.6***	257.9***
					(70.71)	(57.24)
HH number of orphans					4.514	3.654
					(14.84)	(12.01)

Highest education level					-5.040	-4.080
					(9.734)	(7.880)
Female Headed HH					-118.7***	-96.07***
					(40.12)	(32.48)
Age of HH Head					10.51***	8.510***
					(1.366)	(1.106)
Widow HH Head					-41.55	-33.64
					(39.51)	(31.98)
Elderly (>60) HH Head					-262.4***	-212.5***
					(72.32)	(58.54)
Social Network Index					0.244**	0.198**
					(0.114)	(0.0919)
HH Size					-68.62***	-55.55***
					(25.88)	(20.95)
Land Owned in Acres					9.303	7.531
					(7.721)	(6.250)
TLU					52.19***	42.25***
					(10.16)	(8.221)
Average Education (0-17)					14.67**	11.88**
					(5.942)	(4.810)
Average Education (18-59)					30.74***	24.89***
					(9.676)	(7.833)
Constant	554.5***	448.9***	503.1***	407.3***	-354.8***	-287.2***
	(22.30)	(18.05)	(43.23)	(35.00)	(98.99)	(80.13)
N	4210	4210	4210	4210	3893	3893
R^2	0.036	0.036	0.052	0.052	0.104	0.104

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Table B2. Full Regression Results of Table 6 'Econometric Results from Estimation of Equation (2)'

Parameter Estimates	(1) Nominal Income	(2) Real Income	(3) Nominal Income	(4) Real Income	(5) Nominal Income	(6) Real Income	(7) Nominal Income	(8) Real Income
*T*Y*	87.19	70.58	143.5**	116.1**	120.1*	97.25*	143.5**	116.2**
	(61.93)	(50.13)	(67.49)	(54.63)	(63.10)	(51.08)	(67.46)	(54.61)
*T*Y*E*	218.5***	176.9***	165.6***	134.1***			-177.3	-143.5
	(55.47)	(44.90)	(60.49)	(48.97)			(120.5)	(97.56)
T	-59.94*	-48.52*	-85.70**	-69.38**	-85.38**	-69.12**	-85.46**	-69.18**
	(35.25)	(28.54)	(34.49)	(27.92)	(34.53)	(27.96)	(34.50)	(27.93)
Y	137.4***	111.2***	943.1***	763.4***	940.5***	761.3***	941.2***	761.9***
	(46.09)	(37.31)	(97.38)	(78.83)	(97.49)	(78.92)	(97.40)	(78.85)
Dummy Leribe	12.73	10.31	-0.416	-0.337	-1.095	-0.886	-2.259	-1.829
	(45.85)	(37.12)	(46.44)	(37.59)	(46.69)	(37.80)	(46.53)	(37.67)
Dummy Berea	-162.2***	-131.3***	-138.4***	-112.1***	-139.3***	-112.7***	-139.6***	-113.0***
	(38.30)	(31.00)	(37.46)	(30.33)	(37.61)	(30.45)	(37.58)	(30.42)
Dummy Mafeteng	130.3**	105.5**	73.54	59.53	73.78	59.73	73.28	59.32
	(55.09)	(44.60)	(57.02)	(46.15)	(57.13)	(46.24)	(57.07)	(46.20)
Dummy Qachas Nek	70.50	57.07	18.75	15.17	20.46	16.57	18.82	15.24
	(89.11)	(72.14)	(82.74)	(66.98)	(82.50)	(66.79)	(82.37)	(66.68)
Cluster Eligibility Ratio	48.73	39.45	75.22	60.89	67.56	54.69	60.91	49.31
	(111.4)	(90.21)	(103.7)	(83.93)	(103.5)	(83.81)	(103.3)	(83.65)
Household Weights	74.22***	60.08***	56.81***	45.99***	59.66***	48.30***	56.58***	45.80***
	(14.13)	(11.44)	(13.98)	(11.32)	(13.44)	(10.88)	(13.98)	(11.32)
Baseline Variables								
Asset Index			0.145	0.117	0.139	0.112	0.142	0.115
			(0.143)	(0.116)	(0.144)	(0.116)	(0.144)	(0.116)
HH members 0-5			110.4***	89.36***	113.9***	92.20***	110.8***	89.67***
			(30.37)	(24.58)	(29.94)	(24.24)	(30.33)	(24.56)
HH members 6-12			103.6***	83.90***	107.1***	86.69***	104.0***	84.21***
			(33.05)	(26.76)	(32.48)	(26.29)	(33.01)	(26.72)
HH members 13-17			88.71**	71.81**	92.17**	74.61**	89.19**	72.20**
			(39.10)	(31.66)	(38.85)	(31.45)	(39.07)	(31.62)
HH male mem 18-59			50.87*	41.18*	54.09*	43.79*	51.26*	41.49*
			(28.70)	(23.23)	(28.32)	(22.93)	(28.67)	(23.21)
HH female mem 18-59			158.5***	128.3***	161.7***	130.9***	158.9***	128.6***
			(28.53)	(23.09)	(28.20)	(22.83)	(28.48)	(23.05)
HH male mem >60			15.53	12.57	18.65	15.09	15.91	12.88
			(77.00)	(62.33)	(76.74)	(62.12)	(76.98)	(62.32)
HH female mem >60			329.5***	266.8***	332.3***	269.0***	330.2***	267.3***
			(70.08)	(56.73)	(70.00)	(56.67)	(70.06)	(56.71)

HH number of orphans			5.623	4.552	5.782	4.680	5.478	4.434
			(14.80)	(11.98)	(14.79)	(11.98)	(14.79)	(11.97)
Highest education level			-5.655	-4.578	-5.833	-4.722	-5.727	-4.636
			(9.711)	(7.861)	(9.712)	(7.862)	(9.709)	(7.859)
Female Headed HH			-117.2***	-94.90***	-117.1***	-94.78***	-117.5***	-95.14***
			(40.14)	(32.49)	(40.14)	(32.49)	(40.16)	(32.51)
Age of HH Head			10.33***	8.362***	10.29***	8.327***	10.32***	8.357***
			(1.364)	(1.104)	(1.364)	(1.104)	(1.364)	(1.104)
Widow HH Head			-39.45	-31.93	-38.92	-31.51	-39.35	-31.85
			(39.55)	(32.02)	(39.55)	(32.02)	(39.58)	(32.04)
Elderly (>60) HH Head			-257.2***	-208.2***	-255.9***	-207.2***	-257.1***	-208.1***
			(72.14)	(58.40)	(72.13)	(58.39)	(72.13)	(58.39)
Social Network Index			0.243**	0.197**	0.243**	0.197**	0.244**	0.198**
			(0.113)	(0.0918)	(0.113)	(0.0918)	(0.114)	(0.0919)
HH Size			-82.23***	-66.57***	-85.45***	-69.17***	-82.60***	-66.87***
			(26.16)	(21.17)	(25.66)	(20.77)	(26.11)	(21.14)
Land Owned in Acres			9.142	7.400	9.090	7.358	9.114	7.378
			(7.789)	(6.305)	(7.807)	(6.319)	(7.795)	(6.310)
TLU			52.00***	42.10***	52.01***	42.11***	52.09***	42.17***
			(10.10)	(8.178)	(10.10)	(8.173)	(10.11)	(8.183)
Average Education (0-17)			15.10**	12.23**	15.16**	12.27**	15.04**	12.18**
			(5.954)	(4.820)	(5.945)	(4.813)	(5.951)	(4.817)
Average Education (18-59)			30.64***	24.80***	30.60***	24.77***	30.61***	24.78***
			(9.693)	(7.847)	(9.698)	(7.851)	(9.694)	(7.847)
*T*Y*E*CGP transfers*					1.225***	0.992***	2.079***	1.683***
					(0.306)	(0.248)	(0.588)	(0.476)
Constant	485.0***	392.6***	-358.4***	-290.1***	-355.8***	-288.0***	-352.6***	-285.5***
	(52.93)	(42.85)	(99.59)	(80.62)	(99.59)	(80.62)	(99.57)	(80.60)
N	4210	4210	3893	3893	3893	3893	3893	3893
R^2	0.057	0.057	0.106	0.106	0.108	0.108	0.109	0.109

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Table B3. Full Regression Results of Table 7 'DD Impact of CGP on Eligible and Ineligible Households from Equation (2), by Income Source with Real Income as Dependent Variable'

Parameter Estimates	(1) Income from Livestock	(2) Income from Wage Work	(3) Income from Only Crop and Self-employment
*T*Y*	48.63***	18.73	-121.6
	(17.64)	(57.38)	(116.4)
*T*Y*E*	-49.91**	-90.63*	0.750
	(24.56)	(52.85)	(186.1)
*T*Y*E*CGP Transfers*	0.00587	0.523*	1.947**
	(0.0966)	(0.301)	(0.906)
T	-12.74**	-40.99	-12.83
	(5.494)	(46.72)	(29.62)
Y	36.82**	61.01	542.5***
	(15.15)	(126.4)	(107.5)
Dummy Leribe	3.156	-50.63	-94.21**
	(8.943)	(46.71)	(47.21)
Dummy Berea	2.389	-85.75**	-85.57**
	(7.613)	(41.39)	(42.59)
Dummy Mafeteng	-2.871	20.52	42.46
	(8.011)	(70.46)	(54.97)
Dummy Qachas Nek	-11.85	-22.76	73.97*
	(10.03)	(128.7)	(41.31)
Cluster Eligibility Ratio	43.07	-71.30	-168.9
	(26.14)	(59.32)	(103.2)
Household Weights	-1.754	-9.370	62.80**
	(2.873)	(9.620)	(25.44)
Baseline Variables			
Asset Index	0.0328	-0.0220	-0.172
	(0.0299)	(0.179)	(0.186)
HH members 0-5	-4.876	80.25	138.3***
	(10.05)	(61.22)	(44.95)
HH members 6-12	-2.791	50.18	99.79*
	(10.85)	(56.48)	(51.17)
HH members 13-17	-6.066	-17.56	98.62*
	(12.07)	(65.18)	(52.72)
HH male mem 18-59	-4.914	8.058	86.51**
	(10.02)	(55.05)	(40.73)
HH female mem 18-59	-5.357	55.60	114.6***
	(10.62)	(64.75)	(41.65)
HH male mem >60	10.72		202.7**
	(8.578)		(83.79)
HH female mem >60		17.39	198.7**
		(66.58)	(90.63)
HH number of orphans	0.785	9.673	-4.757
	(2.620)	(21.26)	(15.28)
Highest education level	0.470	-8.344	-15.25
	(1.938)	(12.15)	(11.28)
Female Headed HH	14.52*	35.30	-11.89
	(7.498)	(53.73)	(42.10)
Age of HH Head	-0.314	2.219	4.555**
	(0.334)	(1.703)	(1.754)
Widow HH Head	-9.380	-55.72	42.30
	(5.978)	(61.92)	(33.10)

Elderly (>60) HH Head	-5.047	13.98	-143.2
	(14.67)	(71.28)	(96.19)
Social Network Index	0.0113	-0.0770	0.327***
	(0.0173)	(0.146)	(0.112)
HH Size	3.169	-30.28	-91.60**
	(10.16)	(54.71)	(43.88)
Land Owned in Acres	1.033	-1.509	21.89*
	(1.165)	(5.685)	(11.48)
TLU	16.67***	8.056	
	(2.803)	(6.918)	
Average Education (0-17)	0.638	18.37*	11.52
	(1.234)	(10.37)	(7.273)
Average Education (18-59)	0.292	0.137	24.41**
	(1.580)	(11.80)	(11.30)
Constant	-3.834	176.1	-89.52
	(17.46)	(127.2)	(101.4)
N	2487	1430	882
R^2	0.075	0.029	0.163

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Table B4. Full Regression Results of Table 8 'Quantile Treatment Effects on the Treated Eligible and Ineligible Households'

Dependent Variable: Real Income	*Quantile = 0.25*		*Quantile = 0.50*		*Quantile = 0.75*	
*T*Y*	-15.11	-16.83	77.39*	76.10*	159.4**	159.6**
	(24.16)	(22.72)	(43.45)	(45.84)	(68.80)	(65.52)
*T*Y*E*	156.8***	316.0***	-213.8**	168.1***	-324.2**	140.6**
	(48.32)	(20.61)	(86.92)	(41.58)	(137.6)	(59.43)
*T*Y*E*CGP Transfers*	0.998***		2.317***		2.481***	
	(0.261)		(0.470)		(0.744)	
T	-8.452	-8.030	-27.25	-25.62	-74.41*	-74.62**
	(13.87)	(13.05)	(24.95)	(26.32)	(39.51)	(37.62)
Y	136.9***	137.8***	561.1***	565.8***	930.6***	930.5***
	(45.79)	(43.06)	(82.36)	(86.88)	(130.4)	(124.2)
Dummy Leribe	-11.48	-11.11	-3.194	5.015	-56.33	-56.43
	(22.93)	(21.56)	(41.25)	(43.51)	(65.31)	(62.19)
Dummy Berea	-53.19**	-52.46***	-98.62***	-92.88**	-181.9***	-181.5***
	(21.00)	(19.75)	(37.77)	(39.84)	(59.80)	(56.94)
Dummy Mafeteng	-14.14	-14.41	9.293	14.34	1.474	1.472
	(21.18)	(19.91)	(38.09)	(40.18)	(60.31)	(57.43)
Dummy Qachas Nek	29.72	29.82	53.73	54.47	7.716	7.639
	(34.99)	(32.91)	(62.94)	(66.40)	(99.66)	(94.90)
Cluster Eligibility Ratio	-1.775	3.708	18.47	38.78	73.01	72.85
	(37.83)	(35.54)	(68.04)	(71.70)	(107.7)	(102.5)
Household Weights	4.461	4.479	32.51***	32.95***	45.56***	45.50***
	(5.210)	(4.899)	(9.371)	(9.886)	(14.84)	(14.13)
Baseline Variables						
Asset Index	-0.0218	-0.0193	-0.0835	-0.0790	0.240	0.239
	(0.0696)	(0.0654)	(0.125)	(0.132)	(0.198)	(0.189)
HH members 0-5	-27.96	-27.70	43.20	41.70	143.1	142.9
	(153.6)	(144.5)	(276.3)	(291.5)	(437.5)	(416.7)
HH members 6-12	-22.55	-21.75	34.33	32.62	92.53	92.32

	(153.5)	(144.4)	(276.2)	(291.3)	(437.2)	(416.4)
HH members 13-17	-40.42	-38.86	38.43	37.03	123.5	123.2
	(153.9)	(144.7)	(276.8)	(292.0)	(438.3)	(417.4)
HH male mem 18-59	-48.34	-47.39	-8.325	-8.167	76.20	76.11
	(153.3)	(144.1)	(275.6)	(290.8)	(436.4)	(415.6)
HH female mem 18-59	-7.102	-6.904	70.46	70.40	162.1	162.0
	(153.3)	(144.1)	(275.7)	(290.8)	(436.5)	(415.6)
HH male mem >60	-23.16	-23.09	65.22	59.48	102.8	103.1
	(156.4)	(147.0)	(281.3)	(296.7)	(445.3)	(424.1)
HH female mem >60	71.63	72.59	219.6	224.6	329.7	329.8
	(155.7)	(146.5)	(280.1)	(295.5)	(443.5)	(422.4)
HH number of orphans	-1.997	-2.265	1.679	2.481	10.37	10.39
	(6.978)	(6.562)	(12.55)	(13.24)	(19.87)	(18.92)
Highest education level	1.264	1.360	0.525	-0.0897	-7.465	-7.436
	(5.247)	(4.935)	(9.438)	(9.956)	(14.94)	(14.23)
Female Headed HH	13.84	13.51	-13.75	-12.51	-11.68	-11.53
	(25.29)	(23.78)	(45.49)	(47.99)	(72.02)	(68.58)
Age of HH Head	2.043**	2.059***	6.282***	6.289***	10.25***	10.25***
	(0.830)	(0.780)	(1.492)	(1.574)	(2.363)	(2.250)
Widow HH Head	-11.67	-11.43	-23.62	-25.03	-82.46	-82.47
	(24.13)	(22.70)	(43.41)	(45.79)	(68.73)	(65.45)
Elderly (>60) HH Head	-73.82**	-74.47**	-138.5**	-138.4**	-263.2**	-263.3***
	(35.91)	(33.77)	(64.59)	(68.13)	(102.3)	(97.38)
Social Network Index	0.114**	0.114**	0.148	0.131	0.205	0.207
	(0.0537)	(0.0505)	(0.0966)	(0.102)	(0.153)	(0.146)
HH Size	30.99	30.25	-26.19	-25.13	-91.45	-91.30
	(153.3)	(144.1)	(275.7)	(290.8)	(436.5)	(415.7)
Land Owned in Acres	2.685	2.703	6.904	6.653	7.643	7.684
	(3.529)	(3.318)	(6.347)	(6.695)	(10.05)	(9.569)
TLU	22.06***	22.11***	40.02***	39.81***	62.26***	62.19***
	(3.337)	(3.138)	(6.002)	(6.332)	(9.503)	(9.050)
Average Education (0-17)	5.349	5.091	2.564	2.425	9.087	9.094
	(3.547)	(3.335)	(6.379)	(6.730)	(10.10)	(9.618)
Average Education (18-59)	1.827	1.811	14.71*	15.12	23.37*	23.35*
	(4.850)	(4.561)	(8.724)	(9.203)	(13.81)	(13.15)
Constant	-73.39	-75.61*	-232.3***	-243.9***	-262.0**	-261.8**
	(46.34)	(43.57)	(83.34)	(87.91)	(132.0)	(125.6)
N	3893	3893	3893	3893	3893	3893
R^2	-	-	-	-	-	-

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Table B5. Full Regression Results of Table 10 'Alternative Specifications--Intensity of Treatment on Treated Eligible Households and Non-linear Effects of CGP'

Dependent Variable: Real Income	(a) OLS Regressions			(b) Quantile Regressions		
		Only Eligible Households			*All Households*	
*T*Y*	38.18			-12.86	77.99*	157.5**
	(93.44)			(23.30)	(43.12)	(68.87)
*T*Y*E*CGP Transfers*	1.696***	1.870***	1.944**	2.864***	-0.479	-1.703
	(0.474)	(0.233)	(0.762)	(0.370)	(0.685)	(1.095)
*T*Y*E*(CGP Transfers)^2*			-0.000333	-0.00516***	0.00842***	0.0124**
			(0.00322)	(0.00166)	(0.00306)	(0.00489)
T	-44.82	-40.14	-41.26	-8.171	-27.66	-72.67*
	(28.65)	(26.43)	(28.69)	(13.40)	(24.80)	(39.61)
Y	513.9***	518.4***	517.3***	133.5***	559.8***	926.6***
	(86.81)	(85.46)	(86.79)	(44.24)	(81.86)	(130.7)
Dummy Leribe	-104.3***	-104.7***	-104.6***	-12.92	-3.340	-58.94
	(38.79)	(38.99)	(38.91)	(22.15)	(40.99)	(65.47)
Dummy Berea	-148.9***	-148.9***	-148.9***	-55.22***	-99.23***	-179.7***
	(34.17)	(34.24)	(34.24)	(20.29)	(37.54)	(59.96)
Dummy Mafeteng	7.370	7.303	7.319	-16.05	8.682	2.476
	(51.41)	(51.50)	(51.50)	(20.46)	(37.86)	(60.46)
Dummy Qachas Nek	-15.24	-14.70	-14.83	27.62	52.96	7.428
	(66.94)	(66.57)	(66.77)	(33.81)	(62.56)	(99.92)
Cluster Eligibility Ratio	-3.653	-5.846	-5.373	-11.56	19.20	61.67
	(102.7)	(102.8)	(102.9)	(36.54)	(67.61)	(108.0)
Household Weights	-26.66	-25.98	-26.15	3.905	32.28***	45.25***
	(54.01)	(53.69)	(54.02)	(5.029)	(9.306)	(14.86)
Baseline Variables						
Asset Index	-0.0532	-0.0539	-0.0538	-0.0227	-0.0836	0.238
	(0.149)	(0.149)	(0.149)	(0.0672)	(0.124)	(0.199)
HH members 0-5	-43.68	-43.78	-43.75	-29.28	42.28	144.1
	(87.90)	(87.88)	(87.92)	(148.4)	(274.7)	(438.7)
HH members 6-12	-57.76	-57.71	-57.73	-24.52	33.19	92.54
	(84.30)	(84.30)	(84.31)	(148.3)	(274.5)	(438.4)
HH members 13-17	-66.12	-66.08	-66.09	-43.98	37.52	122.7
	(82.66)	(82.66)	(82.67)	(148.7)	(275.1)	(439.4)
HH male mem 18-59	-80.96	-80.94	-80.95	-49.93	-8.858	77.34
	(84.20)	(84.19)	(84.21)	(148.1)	(274.0)	(437.6)
HH female mem 18-59	-20.15	-20.27	-20.24	-8.990	69.75	162.8
	(84.49)	(84.45)	(84.50)	(148.1)	(274.0)	(437.6)
HH male mem >60	0	0	0	-20.88	63.70	105.9
	(.)	(.)	(.)	(151.1)	(279.5)	(446.5)
HH female mem >60	116.7**	116.6**	116.6**	70.10	218.8	331.9
	(52.38)	(52.36)	(52.40)	(150.5)	(278.4)	(444.7)
HH number of orphans	14.00	13.97	13.98	-1.410	1.788	10.48
	(11.07)	(11.09)	(11.08)	(6.741)	(12.47)	(19.92)
Highest education level	-15.62*	-15.62*	-15.62*	1.280	0.449	-7.829
	(9.152)	(9.158)	(9.158)	(5.069)	(9.381)	(14.98)
Female Headed HH	-9.798	-9.800	-9.801	14.94	-13.47	-8.742
	(30.46)	(30.46)	(30.46)	(24.43)	(45.21)	(72.21)
Age of HH Head	6.948***	6.945***	6.946***	2.001**	6.276***	10.15***
	(1.305)	(1.304)	(1.305)	(0.801)	(1.483)	(2.369)
Widow HH Head	-11.49	-11.25	-11.31	-11.76	-23.73	-85.08
	(32.94)	(32.87)	(32.96)	(23.32)	(43.14)	(68.91)
Elderly (>60) HH Head	-190.8***	-190.5***	-190.6***	-75.36**	-137.6**	-263.9**

	(69.43)	(69.40)	(69.41)	(34.69)	(64.20)	(102.5)
Social Network Index	0.212**	0.213**	0.213**	0.116**	0.148	0.214
	(0.0902)	(0.0899)	(0.0901)	(0.0519)	(0.0960)	(0.153)
HH Size	70.02	69.98	69.99	32.69	-25.40	-91.96
	(81.86)	(81.87)	(81.88)	(148.1)	(274.0)	(437.7)
Land Owned in Acres	12.99**	12.94**	12.95**	2.704	6.728	7.776
	(5.903)	(5.909)	(5.905)	(3.409)	(6.308)	(10.08)
TLU	15.24**	15.19**	15.21**	22.25***	40.15***	62.06***
	(7.516)	(7.525)	(7.519)	(3.224)	(5.966)	(9.528)
Average Education (0-17)	3.005	2.975	2.982	5.602	2.527	9.594
	(6.439)	(6.447)	(6.445)	(3.426)	(6.340)	(10.13)
Average Education (18-59)	20.35**	20.39**	20.38**	1.626	14.77*	23.70*
	(8.067)	(8.058)	(8.061)	(4.686)	(8.671)	(13.85)
Constant	-72.47	-74.20	-73.76	-67.68	-231.2***	-256.3*
	(84.37)	(83.83)	(84.25)	(44.76)	(82.83)	(132.3)
N	2485	2485	2485	3893	3893	3893
R^2	0.163	0.162	0.162			

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Appendix C: Rank Similarity Test Regression Results

Table C1. Rank Similarity Test Results for Eligible Households Sample

Dependent Variable: Sample Ranks on Real Income	OLS Regression (1) Mean Regression	Quantile Regressions (2) q = 0.25	(3) q = 0.5	(4) q = 0.75
T	-0.0101	-0.0184	-0.0177	-0.00702
	(0.0216)	(0.0187)	(0.0324)	(0.0362)
Baseline Variables				
Asset Index	0.000295***	0.000132	0.000266*	0.000249
	(0.0000980)	(0.0000850)	(0.000147)	(0.000164)
Social Network Index	-0.0000276	0.00000569	0.00000267	-0.0000361
	(0.0000662)	(0.0000574)	(0.0000994)	(0.000111)
Household Size	-0.0147***	-0.00699*	-0.0134**	-0.0171**
	(0.00426)	(0.00369)	(0.00639)	(0.00713)
Land Owned in Acres	0.00274	0.00162	0.00344	0.00270
	(0.00430)	(0.00373)	(0.00645)	(0.00720)
TLU	-0.00152	-0.000679	-0.000468	-0.00208
	(0.00692)	(0.00600)	(0.0104)	(0.0116)
Average Education (0-17)	0.00190	0.00102	-0.000316	-0.000267
	(0.00405)	(0.00352)	(0.00609)	(0.00679)
Average Education (18-59)	-0.0257***	-0.0101***	-0.0231***	-0.0348***
	(0.00332)	(0.00288)	(0.00498)	(0.00555)
Female Headed Household	0.00893	0.0113	0.00263	-0.0291
	(0.0191)	(0.0166)	(0.0287)	(0.0320)
Age of Household Head	-0.00454***	-0.00212***	-0.00366***	-0.00488***
	(0.000585)	(0.000508)	(0.000878)	(0.000980)
Treatment*Baseline Variables				
*T*Asset Index*	-0.0000565	-0.00000703	-0.0000910	-0.000125

	(0.000132)	(0.000114)	(0.000198)	(0.000221)
*T*Social Network Index*	0.0000458	0.0000128	0.0000371	0.0000317
	(0.0000923)	(0.0000800)	(0.000139)	(0.000155)
*T*Household Size*	0.00141	0.00219	0.00257	0.00260
	(0.00564)	(0.00489)	(0.00847)	(0.00945)
*T*Land Owned in Acres*	-0.00284	-0.00136	-0.00322	-0.00203
	(0.00677)	(0.00587)	(0.0102)	(0.0113)
*T*TLU*	0.00320	0.000726	0.00301	0.00353
	(0.00863)	(0.00748)	(0.0130)	(0.0144)
*T*Average Education (0-17)*	0.00282	0.00230	0.00279	0.00518
	(0.00581)	(0.00504)	(0.00872)	(0.00973)
*T*Average Education (18-59)*	0.00369	-0.000172	0.00375	0.0124
	(0.00461)	(0.00399)	(0.00691)	(0.00771)
*T*Female Headed Household*	-0.0153	-0.00916	-0.0145	0.0132
	(0.0261)	(0.0226)	(0.0392)	(0.0437)
*T*Age of Household Head*	-0.000301	0.000151	-0.000233	-0.00165
	(0.000842)	(0.000730)	(0.00126)	(0.00141)
Constant	0.446***	0.160***	0.383***	0.660***
	(0.0160)	(0.0139)	(0.0240)	(0.0268)
N	2485	2485	2485	2485
R^2	0.550			

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010

Table C2. Rank Similarity Test Results for Ineligible Households Sample

	OLS Regression	Quantile Regressions		
Dependent Variable: Sample Ranks on Real Income	(1) Mean Regression	(2) q = 0.25	(3) q = 0.5	(4) q = 0.75
T	-0.463***	-0.160***	-0.395***	-0.691***
	(0.0192)	(0.0121)	(0.0186)	(0.0255)
Baseline Variables				
Asset Index	0.000133*	0.0000512	0.0000866	0.000169*
	(0.0000696)	(0.0000440)	(0.0000674)	(0.0000925)
Social Network Index	-0.0000414	-0.0000359	-0.0000413	-0.0000907
	(0.0000613)	(0.0000387)	(0.0000594)	(0.0000815)
Household Size	-0.0131***	-0.00423*	-0.0126***	-0.0182***
	(0.00352)	(0.00222)	(0.00341)	(0.00468)
Land Owned in Acres	-0.00630	-0.00173	-0.00332	-0.00303
	(0.00484)	(0.00306)	(0.00469)	(0.00644)
TLU	0.00283	0.00202	0.00338	-0.00112
	(0.00338)	(0.00213)	(0.00327)	(0.00449)
Average Education (0-17)	0.000988	-0.00153	0.00169	0.00219
	(0.00286)	(0.00181)	(0.00277)	(0.00381)
Average Education (18-59)	-0.0206***	-0.00803***	-0.0170***	-0.0281***
	(0.00273)	(0.00173)	(0.00265)	(0.00363)
Female Headed Household	0.0140	0.00746	0.0205	-0.00992
	(0.0196)	(0.0124)	(0.0190)	(0.0261)
Age of Household Head	-0.00488***	-0.00203***	-0.00419***	-0.00582***
	(0.000454)	(0.000287)	(0.000440)	(0.000603)
Treatment*Baseline Variables				
*T*Asset Index*	-0.000133	-0.0000512	-0.0000866	-0.000169

	(0.0000995)	(0.0000629)	(0.0000964)	(0.000132)
*T*Social Network Index*	0.0000414	0.0000359	0.0000413	0.0000907
	(0.0000867)	(0.0000548)	(0.0000840)	(0.000115)
*T*Household Size*	0.0131***	0.00423	0.0126***	0.0182***
	(0.00496)	(0.00314)	(0.00481)	(0.00660)
*T*Land Owned in Acres*	0.00630	0.00173	0.00332	0.00303
	(0.00582)	(0.00368)	(0.00564)	(0.00774)
*T*TLU*	-0.00283	-0.00202	-0.00338	0.00112
	(0.00430)	(0.00272)	(0.00417)	(0.00572)
*T*Average Education (0-17)*	-0.000988	0.00153	-0.00169	-0.00219
	(0.00403)	(0.00254)	(0.00390)	(0.00536)
*T*Average Education (18-59)*	0.0206***	0.00803***	0.0170***	0.0281***
	(0.00416)	(0.00263)	(0.00403)	(0.00553)
*T*Female Headed Household*	-0.0140	-0.00746	-0.0205	0.00992
	(0.0288)	(0.0182)	(0.0279)	(0.0383)
*T*Age of Household Head*	0.00488***	0.00203***	0.00419***	0.00582***
	(0.000679)	(0.000429)	(0.000658)	(0.000903)
Constant	0.463***	0.160***	0.395***	0.691***
	(0.0135)	(0.00854)	(0.0131)	(0.0180)
N	1408	1408	1408	1408
R^2	0.642			

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010