Are There Different Spillover Effects From Cash Transfers to Men and Women? Impacts on Investments in Education in Post-War Uganda*

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November 2017
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Abstract

This paper looks at the spillover effects of a grant, the Youth Opportunities Program (YOP), on human capital investments in conflict-affected Northern Uganda. The YOP grant was primarily aimed at providing start-up money to groups of underemployed youth, while working in practice similar to an unconditional cash transfer. It kept a gender balance by mandating that groups contained at least a third of women. Overall, the intervention had a significant impact on education-related expenditures, increasing them by 11-15 percent, US$ 17-23, in the shorter and longer term (i.e. after two and four years). However, the educational expenditures of women in the treatment group did not increase. Female recipients seem to have not spent more on education, at least in part, because of redistributive pressures, such as probable financial requests from other members of their YOP group. These findings should be relevant for future designs of group eligibility rules and for targeting of cash transfers.

JEL Codes: D04, I25, J16, O15

*Acknowledgements: I thank Martina Björkman Nyqvist and Siwan Anderson for insightful suggestions, as well as seminar participants at University of Göttingen, University of Turin, University of Namur, UNU-WIDER, and conference participants at the SITES-IDEAS, WinE, PacDev, RES, CSAE, HiCN, ABCA events. I am grateful to Nathan Fiala for sharing the data and providing comments on the first draft. This research received funding from the UNU-WIDER Gender and Development Project and from the European Union's Seventh Framework Programme under grant agreement n. 609402 ('2020 Researchers: Train 2 Move' T2M Project). All errors and opinions are mine.

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1 Introduction

Low levels of investment in productive activities and human capital often constitute a key constraint for households to escape poverty. In conflict-affected and post-conflict countries, investments are limited even more by detrimental economic shocks and by lower expected returns to capital and education. Hence, many development projects, especially reconstruction interventions, are directed to support the investments of the poor.

Cash transfers, in particular, are aimed at alleviating poverty in the short run by providing money and at breaking the intergenerational transmission of poverty by inducing investments in child education and health, usually through conditionalities. Evidence from numerous countries suggests that these programs are generally successful in reaching their primary objectives, i.e. increases in school enrollment or use of health services (see Fizbein et al., 2009 for a review and Saavedra and Garcia, 2012 for a recent meta-analysis). However, their focus on the human capital accumulation of the young has led to some criticism because they might miss opportunities to alter productive activities and have broader effects on graduation from poverty. Most cash transfers have been shown to indeed have little impact on work incentives and adult labor supply (see Alzua et al., 2013, and Banerjee et al., 2015, for comparable results from different countries).

In this sense, the Youth Opportunities Program (YOP), a grant that, with support from the Government of Uganda and the World Bank, provided start-up money to groups of underemployed youth from the post-conflict north represents a remarkable success story. Blattman, Fiala, and Martinez (2014) demonstrate that the program did substantially increase productive assets, along with work hours, earnings, and the probability of practicing a skilled trade. This paper adds to the literature by looking at the spillover effects of the YOP on investments in human capital, particularly educational expenditures.

First, the effect of the YOP on educational outcomes is an empirical question, which is relevant for understanding the overall desirability of such an intervention. For instance, Shah and Steinberg (2015) offer mixed results regarding the impact of NREGS, a large workfare program, on human capital in India: the effect was positive for children aged 2-8 (significantly positive for children aged 2-4), but negative for adolescents. Similarly, Kugler et al. (2015) show that a vocational training program for disadvantaged youth in Colombia increased human capital accumulation -in formal education as well- of training participants and of their family members, but only for male participants.

Second, the spillover effects of the YOP might differ by gender since women in developing countries have to face stronger constraints than men (Cho et al.,

2013) that might limit their capacity of benefiting from the full range of positive externalities of a program.

In this paper, I show that the YOP grant was effective in increasing education-related expenses, which grew by 11-15 percent, US$ 17-23, in the shorter and longer term (i.e. after two and four years). The intervention also increased subjective education-related outcomes by 6 percent overall (by 8 percent for men). Interestingly, the results do not seem to be driven by a direct income effect from the money injection since the size of the grant received influenced only food expenditures. More specifically, men assigned to receive the grant increased total educational expenses by 21-24 percent (US$ 32) both after two and four years, whereas total educational expenditures of female recipients did not increase. The effect for men is driven by a growth in expenses on their own children and family members. On the contrary, women did not change their family spending patterns. They did however increase their expenditures for non-family members by 90-95 percent after two years.

These gender-differentiated effects might be due to different reasons. One hypothesis is that women did not increase investment in human capital because they had already met their optimal level of expenditure on family education. The heterogeneous effects suggest this is not the case since better-educated women did manage to spend significantly more for the education of their own children and family members. Another hypothesis is that women could not invest more on their family's education because after receipt of the grant they were subject to stronger redistributive pressures. The YOP groups were forced to include a third of female members. Hence, the pressures that women suffered might have included financial requests from the other members of their YOP group. The analysis of the heterogeneous effects offers evidence towards this hypothesis. Women who were badly matched to their YOP group, i.e. women who belonged to groups with higher human capital disparity or who, from the time of the baseline, were dissatisfied with their group, did spend substantially more on transfers outside their households.

These results are in line with evidence shown in Blattman et al. (2014) indicating that, in spite of a significant increase in earnings, the social position of women did not improve. On the contrary, in the shorter-term, their anti-social behaviors increased and, in the longer-term, their participation in groups decreased. This suggests that, in the YOP groups, forcing men to team up with women might have impeded fully reaching the stated goal of gender balance.

These findings should be relevant for the design of group eligibility rules and for targeting of cash transfers, and they should pave the way for further research focusing on the role of gender composition in group dynamics. One paper on this

topic by Berge et al. (2016) proposes a laboratory experiment to study the ability to collaborate and make risk-taking decisions among groups of microfinance clients in Tanzania. The authors show that female groups are more able to collaborate and find common solutions to challenges than mixed (or male) groups.

This study also contributes to the recently growing literature on the constraints that women face when making decisions in developing countries. One strand of this literature focuses on gender discrimination and highlights a series of negative social biases women have to bear. Cho et al. (2013) claim that, in participating in training, women in Malawi are constrained by family obligations and penalized by trainers, who are more likely to provide financial help and paid work after training to men rather than to women. Similarly, BenYishay et al. (2016) show that, even though women learn and retain information about new agricultural technologies better, they are not as successful as men at convincing others to adopt because other farmers perceive them as less able communicators. Another strand of the literature investigates the extent of redistributive pressures and, while it does not necessarily have gender as its primary focus, it ends up underlining stark gender differences. In their seminal paper, Jakiela and Ozier (2016) demonstrate that, in rural Kenya, women are significantly burdened by social pressures to share their income with their kin and neighbors. They estimate that women's income is 'taxed' at a rate around four percent or almost eight percent when kin can observe income directly. Focusing on family pressures in Uganda, Quisumbing et al. (2011) show that wives' assets are less insured against shocks than husbands' assets. Along the same line, Fiala (2015a) suggest that women in Uganda feel pressure from husbands to invest their money in other household businesses: women that receive loans do not profit from them, whereas the income of their spouses increases. Boltz et al (2015) offer evidence from urban Senegal that women exhibit high willingness-to-pay to hide money and escape redistributive pressures. Interestingly, for women willingness-to-hide increases with the number and strength of social ties (such as having always been living in the community, while almost the reverse is found for men). In addition, the authors show that lottery winners that manage to transfer less to others (private winners) do spend more on private items and health.

Moreover, this paper is based on the literature analyzing the consumption impacts of cash transfers, either conditional (Attanasio et al., 2011, in Colombia; Cruz and Ziegelhfer, 2014, in Brazil; Gertler et al., 2012, in Mexico; and Macours et al., 2012, in Nicaragua, with focus on food consumption and child nutrition) or unconditional (Bazzi et al., 2015, in Indonesia; Haushofer and Shapiro, 2016, in Kenya; and Schady and Rosero, 2008, in Ecuador). This study is also linked to the literature on the impact of aid programs in Uganda, a country with one

of the youngest populations in the world (48 percent of its citizens are under the age of 15).[1] Recent evidence suggests that development programs targeted to the country's youth were successful in tackling a range of issues from lack of skills to risky health behaviors, child mortality, and underinvestment in education (Blattman et al., 2014; Bandiera et al., 2015; Björkman and Svensson, 2009; and Karlan and Linden, 2014).

The rest of the paper is organized as follows. Section 2 reviews the literature on cash transfers and theoretical models of collective consumption, while highlighting the YOP features that are distinctive with respect to usual cash transfer programs. Section 3 describes the context of the intervention, along with the details of it and of its impact evaluation. Section 4 provides further information on the data and the descriptive statistics. Section 5 explains the identification strategy and shows the results. Finally, Section 6 discusses the findings and offers concluding remarks.

2 Cash Transfers and Intra-household Decision Making: A Literature Review

A number of articles studying intra-household bargaining show that resources under the mother's control have a more beneficial impact on a child's wellbeing. As Duflo (2012) summarizes, various influential papers since the 1990s have suggested that income or assets in the hands of women, instead of in the hands of men, are associated with stronger positive effects on child health (Thomas, 1990) and with increased percentages of household budgets spent on family health and food nutrients (Thomas, 1993). Maluccio and Quisumbing (2003) and Duflo (2003 and 2004) have offerred additional empirical evidence supporting such findings. This literature provided a rationale for entitling cash transfers to mothers.

More recent papers looking at the effects of Conditional Cash Transfers (CCTs) show that the extra money in the hands of women is indeed well spent. For instance, Rubalcava et al. (2009) study the impact of PROGRESA in Mexico and show that beneficiary women who have resources under their control are more likely to spend them on small livestock, improved nutrition, and child goods (particularly clothing). Similarly, De Brauw et al. (2014) find that, in Brazil's urban areas, the Bolsa Familia CCT had significant impacts on the decision-making power of women, as well as on children's school attendance, children's health expenses, and household purchases of durable goods. This strand of literature generally uses the collective model to explain the mechanisms behind the effects of

[1] Niger is the only country with a higher number(50 percent, World Development Indicators 2014).

targeting cash transfers to women (see, for example, Attanasio and Lechene, 2002 and 2014). In a unitary model, cash transfers would affect household decisions only through the impact they have on total income and the budget constraint. In a collective model, instead, targeted transfers could also change the balance of power within the household and, as a consequence, the allocation of resources.[2]

However, transferring cash to women does not necessarily imply an increase in women's power over household resources. Analyzing the impact of Progresa, Handa et al. (2009) find effects only on women's capacity to spend their own cash, but not on other decision-making spheres. De Brauw et al. (2014) show that the impact of Bolsa Familia on women's bargaining power was substantially heterogeneous and not significant in rural areas. Yoong et al. (2012) review the literature on transfers to men and women and find no consensus on whether CCTs increase women's decision-making power. Almas et al. (2015) argue that previous inconclusive results might be due to measurement issues, while Peterman et al. (2015) test different women's decision-making indicators across three countries and still show mixed evidence.

In this regard, it is important to note that the YOP grant was not targeted to individuals directly, but to groups that had the responsibility to share the cash among male and female members. Hence, it is likely that group structure and matching played a role in influencing the effectiveness of the grant. For instance, Arcand and Fafchamps (2012) find that community-based organizations in West Africa tend to exclude from the group less fortunate members of the society like female-headed households, even when donor-sponsored.

Concerning changes in empowerment, Blattman et al. (2014) show that female YOP beneficiaries did not improve their social position, i.e. their integration in the community, notwithstanding a substantial growth in earnings. This lack of positive results in terms of gender equality is even documented with qualitative evidence in official World Bank reports.[3] More concerning is that women's anti-social behaviors (i.e. self-reported aggression and disputes) increased after two

[2]For a recent review of the literature on intra-household models see, for example, Fiala and He (2016).

[3]For example, the review of the project listed among the key lessons the following: 'Exactly 33 percent of YOP beneficiaries were female. This figure, being YOP's requirement for women participation, suggested that youth groups aimed only to register the very minimum number of women. Moreover, few groups had female leadership. These patterns suggest that women may be marginal players in many groups. It is yet uncertain how to increase female participation and ownership of the projects, but these observations suggest that a more proactive targeting of young women may be necessary to overcome the barriers that limit women's participation in the context of demand-driven, community-based programs such as YOP'. Implementation Completion and Results Report of the Northern Uganda Social Action Fund, ICR1211, p. 40, point 38. http://documents.worldbank.org/curated/en/417791468310732815/pdf/ICR12110P002951IC0disclosed03111101.pdf

years from receipt of the grant.[4] Blattman et al. (2012) hypothesize that women's increase in disputes, quarrels, and threats was a consequence of greater market engagement/ interaction outside the home and, therefore, opportunities for aggression. However, after four years, this effect dissipates while women's market engagement and profits grow even more. On the contrary, their number of group memberships decreases (Blattman et al., 2014, p. 738, Table VIII).

Studying the role of networks, a number of articles -on the effects of cash transfers or on the lives of entrepreneurs in developing countries- document the existence of social redistributive pressures and (forced) risk-sharing arrangements, with cash being transferred from those that have additional resources in their hands to other close peers (Angelucci and De Giorgi, 2009, and Grimm et al., 2016). For example, Angelucci and De Giorgi (2009) indicate that, after two years from the start of Progresa, ineligible households in treatment villages increased their likelihood of receiving monetary transfers by roughly 50 percent. This phenomenon has been analyzed in a more rigorous way within the context of extended families. While some authors (e.g. LaFave and Thomas, 2017) highlight that, under such solidarity arrangements, a collective model can still be used to describe household decision-making and efficiency can still be ensured[5]; other scholars (e.g. Baland et al., 2016, and Kazianga and Wahhaj, 2016) find evidence of large disincentive effects and inefficiencies.

Also, the YOP worked, in practice, as an Unconditional Cash Transfer (UCT) and was entitled to groups, in theory, as start-up money to encourage business generation. So it is plausible that it affected women's capacity to invest on their own business, without improving their overall decision-making power. The literature on UCTs indicates that these transfers are usually successful in improving the wellbeing of the younger generation (Baird et al., 2011, showing decreased teenage pregnancies and Haushofer and Shapiro, 2016, showing improved food security), but less so when there are 'marginal' individuals involved - such as females and lower ability children. In particular, Akresh et al. (2013 and 2016) find that, while conditionality does not play a big role in improving school participation for boys, it is crucial in increasing participation for girls.

Finally, on the one hand, targeting cash transfers to females has the advantage

[4]Blattman et al. (2012) paper offers more details about these short-term results. 'Treated females are twice as likely as control females to report a physical fight, bringing them to roughly the same level of physical fights as males. [...] Males also report significantly lower disputes with leaders and police, or physical fights, among their peers. Females do the opposite. [...] The final four dependent variables look at self-reported hostile behaviors; the largest male decline, and female increase, are seen for quarrelsomeness and threatening others, two of the more serious forms of hostile behavior we measure'. Blattman et al., 2012, pp. 36-38.

[5]Families can coordinate allocation decisions in such a way as to make no family member better off without another member being worse off.

of promoting gender equality; on the other hand, though, the evidence on the importance of targeting women versus men for the improvement of child wellbeing is still non conclusive. Recent findings by Akresh et al. (2016), Benhassine et al. (2015), Haushofer and Shapiro (2016), and Tommasi (2016) show that randomizing the gender of the cash recipient does not make much of a difference and, hence, the claim that mothers have a larger marginal willingness to pay for children than fathers might not be as strong as supposed.

Against this background, it is useful to recognize one important limitation of this study. I cannot directly analyze why the YOP program increased the educational expenses of men, but not those of women. I discuss how I believe that the lack of effect for women might be due to their weak decision power among strong redistributive pressures, but this discussion is entirely speculative. Since I do not have information about spouses, I cannot test the predictions of the collective model on this data. Thus, the question of whether these findings are due to differences in bargaining power or in preferences of women versus men remains unanswered. Lastly, it should be noticed that men from Northern Uganda are not necessarily representative of men from Uganda or from other developing countries more generally. As I explain in the next session, the war in the North of the country disproportionally affected boys and it is possible that such a violent shock affected the preferences of men in unique ways. For instance, Zhou (2015) argues that early life adversities can have significant effects on the education outcomes of one's children.[6]

3 Background and Experimental Design

3.1 Northern Uganda Context

In the late 1980s, the Ugandan political situation degenerated when the south-based National Resistance Army (NRA) lead by Museveni overtook power with a military coup. In response, a civilian resistance movement was formed and, at the end of 1987, the rebel leader Joseph Kony established a new north-based guerilla group, the Lord's Resistance Army (LRA). To maintain supplies and forces, the LRA started to attack the local population raiding homes and kidnapping youth. Between 60,000 and 80,000 youth were abducted, mostly after 1996 and from one of the Acholi districts of the north (Blattman and Annan, 2010). Adolescent males were disproportionately targeted since they were more malleable recruits (Beber and Blattman, 2013) and those who failed to escape were trained as fighters. In

[6]The author shows that, in China, urban youth that were forced to move to rural areas to carry out hard manual labor invest more on their children's education in their adult life.

1996, the government created the so-called "protected camps" and, in 2002, systematic displacement increased during military operations against the LRA bases in southern Sudan. By 2006, 1.8 million people lived in more than 200 Internally Displaced Person (IDP) camps in Northern Uganda. In 2006, the Government of Uganda and the LRA signed a truce. From the ceasefire onwards, IDPs were allowed to leave the camps and encouraged to return to their area of origin.

The conflict had a series of negative consequences on human capital, household wealth, and individual expectations for the northern population. Blattman and Annan (2010) show that, among abducted youth, schooling fell by nearly a year, literacy rate and skilled employment halved, and earnings dropped by a third. Fiala (2015b) finds that displaced households had lower consumption and fewer assets than non-displaced households. While wealthier households recovered part of their consumption by 2008, poorer households remained trapped in a lower equilibrium. Bozzoli et al. (2011) show that exposure to conflict caused pessimism about future economic wellbeing and that young individuals were more affected than people in their 30s. They posit that the latter result is due to the cohort effects of the war, during which the youth grew up in camps and lost education and networking opportunities.

These findings suggest that the war left many scars and it disproportionally affected the younger generation. In such a post-conflict context, the recovery of children and young adults is a critical concern since lost education can take years to regain and the psychological effects may be long-lasting.

3.2 The Youth Opportunities Program and its Impact Evaluation

Historically, the government's development strategy for the north was embodied in the Northern Uganda Reconstruction Program (NURP-I). NURP-I ran from 1992 to 1997 with limited success and it was re-launched as NURP-II in 1999 with a new decentralized approach. The most significant initiative under NURP-II was the Northern Uganda Social Action Fund or the NUSAF project, which started in 2003 with funding from the World Bank. NUSAF was based on a community-driven development design and it was aimed at helping the rural poor of the north cope with the effects of the prolonged LRA insurgency. In 2006, the government added to the NUSAF an extra component, the Youth Opportunities Program or YOP, in order to foster the recovery of the conflict-affected young generation and boost non-agricultural employment.

Specifically, the YOP targeted underemployed youth aged between 16 and 35. It required young adults from the same village to organize into a group, of about

20 members on average, and submit a proposal for a cash transfer to pay for technical training, tools, and materials for starting a skilled trade.[7] The eligibility criteria made compulsory to include at least a third of female members and, thus, forced male groups to team up with women.

Many applicants were functionally illiterate and so the YOP required 'facilitators', usually a local government employee, teacher, or community leader, to meet with the group and help prepare the proposal. Groups were responsible for selecting their facilitator and management-committee, for choosing the skills and schools, and for allocating funds among members. Successful proposals received a lump sum transfer of up to US$ 10,000 to a bank account in the names of the management-committee's members. The transfer was not subsequently monitored by the government and therefore, in practice, it was similar to an unconditional cash transfer even though the eligibility required the submission of a business plan. Full details of the intervention are explained in Blattman et al. (2014).

Thousands of groups sent their application and hundreds received funding from 2006 to 2008. In 2008, few funds were left and the remaining eligible groups were randomized into treatment and control by the research group that designed the program evaluation (Blattman et al., 2014)[8]. Out of the 535 remaining eligible groups (about 12,000 members), 265 received funding and 270 did not. Blattman et al. (2014) report that treatment and control groups/ villages were typically very distant from each other and thus spillovers were unlikely.

4 Data

4.1 The Sample

For each of the 535 remaining groups, five members were randomly selected to be interviewed for a total of 2,677 observations spread over 17 districts in Northern Uganda. The baseline survey was collected in early 2008, the government disbursed funds during the summer of 2008, and two follow-ups were collected two years later between 2010 and 2011 and four years later in 2012 (Figure 1).

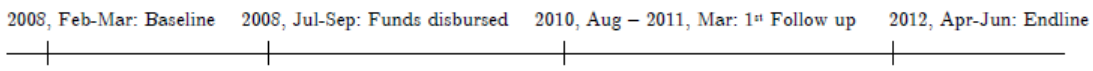

Figure 1. Timeline of the events

[7] As Blattman et al. (2014) explains, people had to apply as a group because of administrative convenience and villages typically submitted only one group application each.

[8] The data is publicly available at: Blattman, C., Fiala, N., Martinez, S. (2014), Northern Uganda Social Action Fund - Youth Opportunities Program, Harvard Dataverse, V1, http://dx.doi.org/10.7910/DVN/27898

Attrition was minimized with a two-step tracking strategy that allowed to reach satisfactory effective response rates (85 percent in 2010 and 82 percent in 2012). The randomization attained balance over an ample array of measures (with few exceptions). Blattman et al. (2014) show in their sensitivity analysis that the results are robust to concerns arising from potentially selective attrition or imbalance.

The sample is mostly composed of young rural farmers with low earnings (less than one US$ a day). Given that the three most conflict-affected districts were not included into the YOP evaluation and that members had to have a minimum capacity to benefit from training, applicants were not from the most vulnerable or poorest population groups. Nonetheless, the program did not have specific educational requirements and many uneducated and unemployed people applied. Beneficiaries received on average US$ 382 each[9] (about the mean annual income) and invested some of it in training, but most of it in tools and materials.

Blattman et al. (2014) give a detailed picture of the impacts of the cash transfer over a wide range of individual indicators. As expected, assignment to receive the YOP grant positively affected training hours and capital stocks. Beneficiaries reported 340 more hours of vocational training than controls. By 2012, treatment men increased their stocks by 50 percent relative to control men, while treatment women increased their stocks by more than 100 percent relative to control women. Treatment also increased total hours worked per week by 17 percent, mostly dedicated to skilled trades. However, it did not influence hours in other activities nor migration decisions. In addition, the program increased business formalization and hired labor (mainly in agriculture), as well as earnings, assets, and consumption. By 2012, the grant raised men's earnings by 29 percent and women's earnings by 73 percent, but in absolute terms men's earnings remained substantially higher than women's earnings. It also increased both durable assets and non-durable consumption by 0.18 standard deviations. Finally, the program improved subjective wellbeing by about 13 percent, but had no impact on socio-political attitudes and behaviors (Blattman et al., 2016, explore in more details the political effects of the program).

I employ the same dataset to focus on the spillovers of the program on children and adolescents. I look at household-level outcomes and, in particular, at household expenditures on education and health.

[9]Blattman and co-authors obtain this figure by dividing the group funds received by the estimated 2008 group size. So per capita grant size varies across groups due to variation in group size and amount requested.

4.2 Descriptive Statistics

Table 1 presents descriptive statistics about individual and household level pre-intervention characteristics of the sample.

Individuals are on average 25 years old and they have almost an eighth grade education, which corresponds to a completed primary education level. On average, they have experienced at least one war-related event (most have witnessed violence). In spite of the young age, they already have a mean of 2.5 children. Households comprise about five members, with on average three minors -half of which are females. These minors are on average 5-years old in 2008, meaning that they are of school age at the time of the follow-ups. Minors represent indeed the majority of household members and almost every household (93 percent) has at least a minor in the composition. The minors are mainly the biological children of the respondent, but the presence of other minors is also frequent (41 percent of the households comprise at least one). These other young family members are mostly nieces or nephews or young brothers/ sisters. Households are close enough to primary education facilities -with primary schools being generally not further than 2 km-, whereas secondary schools are on average 5 km away.

Table 1, Panel A., Column (6) shows the p-value of the balance test on the above-mentioned baseline covariates. Household characteristics seem to be well-balanced since none of the differences between treatment and control groups is significant at a 95 percent level. Therefore, the sample is suitable also for an analysis at the household level. Panel B. confirms that the women-only sample is well balanced as well, while Panel C. shows how men and women differed at the time of the baseline. To minimize this selection bias, all the statistically significant covariates are included in the regression framework -as explained in the next session.

5 Methodology and Results

5.1 Estimation Method

My estimation is based on the following regression:

$$(1) Y_{h,POST} = c + \beta T_h + \delta X_h + \Phi + \epsilon_{h,POST}$$

where T is an indicator for assignment to treatment, X is a set of baseline covariates at the individual and household level, Φ represents district fixed effects, and standard errors are adjusted for clustering at the group level. More specifically, X comprises a female dummy, age, education and human capital levels, ini-

tial level of capital and credit access, employment type and levels, and variables capturing group characteristics (as in Blattman et al., 2014 to ensure comparability)[10]. This set of covariates corrects for any baseline imbalance and guarantees similarity between the treatment and the control groups. The treatment effect is estimated by β and the 2010 and 2012 impacts are evaluated separately. The survey weights are used, so the observations are weighted by their inverse probability of selection into the endline tracking. My main outcomes of interest are household consumption and educational and health expenditures. Since I employ mostly household-aggregated measures instead of per capita indicators, I also control for the number of household members, the number of household minors, and the number of biological children. Moreover, dealing with monetary variables, I cap all currency-denominated variables at the 99th percentile to avoid biases driven by extreme values. For comparability, I deflate all values to the 2008 correspondent.

For various reasons[11], out of the 265 treatment groups 29 did not receive the grant. Thus, regression (1) represents an Intention-to-Treat (ITT) estimation. To take into account imperfect compliance, I also employ Instrumental Variable estimations that use the initial assignment (the ITT) as an instrument for actual treatment in order to assess the Treatment-effect-on-the-Treated (ToT). In showing the results, I focus on the ITT estimates while I present the ToT parameters as a robustness check.

Finally, since outcomes are self-reported, the treatment effect might be affected by over-reporting in the treatment group due to the social desirability bias (i.e. the tendency to answer questions in a manner that can be favorably viewed) and under-reporting in the control group due to its desire to be included in future aid programs. I try to overcome this issue comparing the results for educational and health expenditures that should be equally affected by the social desirability bias and by looking also at household food and non-food consumption indicators that are less likely to be significantly biased since they are based on aggregate

[10]The full list of variables included is: female (dummy); age (plus quadratic and cubic); located in a urban area (dummy); being unfound at baseline (dummy); risk aversion index; being enrolled in school (dummy); highest grade reached at school; distance in km to educational facilities; able to read and write, even minimally (dummy); received prior vocational training (dummy); digit recall test score; index of physical disability; z-score of durable assets (z-score); savings in past 6 months; monthly gross cash earnings; can obtain 100,000 UGX loan (dummy); can obtain 1,000,000 UGX loan (dummy); average of weekly hours spent on: all non-agricultural work, casual low-skill labor, skilled trades, high-skill wage labor, other low-skill petty business, other non-agricultural work, household chores; zero employment hours in past month (dummy); main occupation is non-agricultural (dummy); engaged in a skilled trade (dummy); grant amount applied for in USD; group size; grant amount per member in USD; group existed before application (dummy); group age in years; z-score of within-group heterogeneity; z-score of quality of group dynamic; any leadership position in group (dummy); group chair or vice-chair (dummy). All indicators refer to the baseline values.

[11]See Blattman et al. (2014) for an explanation.

computations coming from 135 different questions.

5.2 Impacts on Household Expenditures

5.2.1 Impacts on consumption

Table 2 displays the ITT estimates of the cash grant on monthly household consumption on non-durables. After four years, the program significantly increased monthly consumption per capita by more than UGX 3,000 or about US$ 2 (a 12 percent increase relative to the control). The impact seems to be slightly more substantial for assigned females since they increased their consumption per capita by 15 percent compared to control females, while assigned males increased it by 11 percent. The same finding holds when looking at total household consumption controlling for the number of household members. Considering that in 2012 there were on average eight members, the magnitude of the effect is similar to the per capita correspondent with an increase of US$ 13, or again 12 percent. The result is confirmed also when using the log variable in place of the level indicator.

Food consumption in the treatment group significantly rose by 10 percent (UGX 14,660 or about US$ 8) and non-food consumption relevantly grew by 18 percent (UGX 8,400 or US$ 5). The decomposition in food and non-food expenditures shows some gender differences. Women assigned to receive the grant spent 13 percent more on food consumption and non-food consumption; wheareas men in the treatment group increased food consumption by only 8 percent and non-food consumption by 20 percent relative to men in the control group. This spending preference of males could be either positive or negative for household welfare depending on the types of non-food expenses privileged.

5.2.2 Impacts on education and health expenditures

I focus on total expenditures for education and health made in the 12 months before the survey. In Table 3, I consider household-aggregated measures[12] while controlling for the number of household members, the number of household minors, and the number of biological children. The program impact on educational expenditures is statistically significant only in logs, but corresponds to a quite substantial relative increase of 11-15 percent (UGX 29,000-40,000 or US$ 17-23) in 2010 and 2012 (Table 3). The intervention also caused a significant growth in shorter-term health expenditures by 23 percent (about UGX 7,000 or US$ 4), but the effect is close to zero after four years. The results are confirmed by the

[12]The results do not depend on this choice though.

illustration of the relative log distributions (Figure A1)[13].

Passing on to the gender-differentiated impacts, in 2010, males assigned to receive the grant increased total educational expenses by 21 percent (the effect is significant when looking at the log results). In 2012, their educational expenditures increased even more by a statistically significant 24 percent, whereas educational expenditures of females in the treatment group decreased. There is no significant gender heterogeneity on health expenditures. In economic terms, among males in the treatment group, educational expenditures increased by US$ 32 both in the shorter and long run and health expenditures increased only temporarily by about US$ 5.

When expenditures are separated in one category for children or young family members and in another category for oneself, Table 4 shows the program generated a statistically significant growth of 20-30 percent in expenses of the first category only for men (except for health expenditure after four years).

Instead, when expenditures for (not-better-specified) non-family members are considered, Table 5 illustrates that the program caused a statistically significant increase of 89-96 percent only for women in 2010. In 2012, also males assigned to receive the intervention increased their educational expenses for non-family members by about US$ 6. This might be because as their incomes rose relative to the community average (see Blattman et al., 2014) they received more requests for informal credit and were more likely to transfer money outside of the household. In any case, while in 2012 males in the treatment group might have spent US$ 6 more for the education of non-family members, they still spent US$ 20 more for the education of their children and family members.

These findings suggest that females, especially shortly after receiving the grant, were more affected by requests by external individuals. Rather than different gender differences in spending, this result might reflect poor female decision power. The treatment effects on household consumption suggest that females who were assigned to receive the grant tried to provide more for their families in those aspects that were under their control like every day food expenses. However, they did not manage to make more substantial investments in the education of their children. I hypothesize this is due, at least in part, to stronger redistributive pressures exerted on women.

Finally, Table A1 reproduces the treatment impact using the ToT estimations. As expected, the results are similar to the ITT estimates with only the difference that the magnitudes of the effects are about 2 percent higher since they now refer

[13]In 2012, the kernel density of educational expenses in the treatment group is more pronouncedly above the control group than it was in 2010. The reverse is true in the case of health expenses. In the shorter-run, the cash grant decreased the proportion of households not (or almost not) spending for health, while the effect dissipated in 2012.

to those that did indeed receive the money.

5.2.3 Impacts on other types of expenditures for children

To check whether my findings are consistent with the results on other expenditure dimensions, I look in more detail at the consumption module of the final endline survey and assess the treatment impacts on expenditures for clothes, shoes, and other material for adults versus minors (separated in males and females), expenditures for educational materials (i.e. books, stationary, and school uniforms), and expenditures for medical treatments and medicines. Table A2 offers a picture that confirms previous results.

On the one hand, males assigned to treatment increased their expenses in clothes and shoes for adults by 16-18 percent, as well as they increased expenses in clothes and shoes for male minors by 14 percent (while the 5 percent increase on female minors is not significant). At the same time, their expenses on educational material grew by 22 percent, whereas their medical expenses rose by merely 4 percent. On the other hand, assigned females increased their expenditures for adults' clothes and shoes by 31-35 percent, but they did not substantially increase expenditures for minors[14]. At the same time, their medical expenses relevantly increased by 23 percent, while their expenses on educational materials increased by only 10 percent. This suggests that males assigned to receive the grant consumed more on education-related items and provided for adults as well as for minors, especially males. Females, on the other side, apparently spent more on (were in better charge of) food consumption and health-related expenses.

5.3 Heterogeneous Impacts

To test for heterogeneity in the treatment effect based on observable characteristics, I run the following set of regressions:

$$(2) Y_{h,POST} = c + \beta T_h + \gamma T_h \cdot TRAIT_h + \eta TRAIT_h + \delta X_h + \phi + \epsilon_{h,POST}$$

where TRAIT is the vector of background characteristics along which theory would predict heterogeneity in the program impacts. The effect of the intervention for the subgroup of people with a given trait is given by the sum of the coefficients β and γ and if γ is significantly different from zero then there is evidence of heterogeneity in the treatment effect for that trait. As an outcome variable, I

[14]The treatment effect is positive and equal to an 11 percent increase for clothes and shoes for females and to a 5 percent increase for clothes and shoes for males, but it is not statistically significant.

focus only on educational expenditures since they offer more useful insights into the differentiated effects of the program.

In particular, I estimate equation (2) for the following baseline characteristics: wealth, having witnessed violence at baseline, number of foster children in the household, proportion of female household minors, and mean age of household minors. Table 6 illustrates the heterogeneous results for the whole sample. Individuals in the treatment group with higher baseline wealth and with more foster children in the household had higher total educational expenses (by about US$ 35 and US$ 44 respectively after four years, whereas the effect for the full sample was of only US$ 17). While the effect of foster children is as expected, the magnifying role of wealth signals that the grant did not necessarily work as an anti-poverty intervention as it did not strongly support investments by the poorest. Interestingly, also people that witnessed violence raised educational expenses (by about US$ 30). This might be due to a difference in social preferences since it has been shown that individuals exposed to violence display more altruistic behaviors (Voors et al., 2012). Their higher altruism might explain why they seem to have spent more on non-family members and less on their-selves. Surprisingly, there is no heterogeneity based on the proportion of female minors in the household, whereas the treatment impact is heterogeneous based on the average age of the minors. The effect of age is unclear though, it is negative in 2010 and positive in 2012.

In order to explore which constraints influenced the educational expenses of females, I also estimate equation (2) on the smaller sample of female respondents. I identified the following baseline characteristics that could be especially relevant for women: baseline education, being married, number of groups one belongs to, dissatisfaction with the YOP group, and the standard deviation of human capital within the YOP group.

Table 7 shows the relative heterogeneous effects of the program on females. Better educated females (who completed at least secondary school) assigned to receive the grant spent significantly more for education, especially in the shorter run and for their children and family members (in 2010 they spent about US$ 68 more for them). Being married does not seem to affect their educational expenditures, while belonging to more groups is detrimental. Females that were assigned to receive the grant and belonged to more groups spent US$ 55 less in 2010 for their children and family members and US$ 62 less in total. Similarly, the results suggest that women that were dissatisfied with their YOP group suffered stronger redistributive pressures and spent significantly more on non-family members (US$ 6 after two years, versus US$ 4 for the full female sample, and US$ 9 after four years). In particular, it seems that women belonging to YOP groups with higher

human capital heterogeneity (higher standard deviation) were substantially more affected by external requests. In 2010, they spent about US$ 20 more for educational expenses of non-family members, while they spent significantly less on their own educational expenses.

5.4 Impacts on Educational Outcomes

Did the increase in educational expenses translated also into better educational outcomes for children and adolescents in Northern Uganda? The evaluation of the YOP intervention and the relative questionnaires were not specifically designed to reply to such a question and, given the lack of suitable outcome indicators, it is not possible to give a clear answer. Table 8 suggests that the response might be yes and shows the results on the only two education-related outcome measures that are available in the dataset.

First, I consider the attendance rate, i.e. the ratio of children attending school over children of school age[15]. However, this indicator might not represent a particularly meaningful outcome because, since Uganda abolished primary school fees in 1997, enrollment rates are almost universal and attendance rates are already high enough (Deininger, 2003; Karlan and Linden, 2014). In fact, in spite of the higher educational expenses, the grant does not appear to have significantly influenced the attendance rate. The sign is even negative, but it turns positive after taking into account the important heterogeneity based on the adult's education. This result is not surprising and it is in line with the results from Karlan and Linden (2014) from rural and peri-urban Uganda[16]. It suggests that the increase in investments in human capital is on the intensive margin, and not on the extensive margin. This finding (no effect on attendance, but a positive effect on educational expenses) could also signal that some minors in the treatment group have switched from low-quality public schools to private schools as found in previous studies (Banerjee and Duflo, 2007; Bold et al., 2015).

Second, I look at the probability of returning to school. This indicator is complementary to the analysis of educational expenditures and outcomes for children, since it is mostly related to own educational expenses and it refers more

[15]This measure is based on the self-reported answers to the following questions: 'How many children of school age do you have?' and 'How many of these children are attending school?' that were asked in the 2010 questionnaire.

[16]The authors study the effect of savings devices for educational expenses by comparing an account fully-committed to educational expenses to an account in which savings are available for cash withdrawal, but intended for educational expenses. They show that the weaker commitment device generates increased savings and, when combined with a parent outreach program, even higher expenditures on educational supplies. It did not affect attendance nor enrollment though. Nonetheless, it did translate in better educational outcomes for children as it increased scores on an exam covering language and math skills by 0.14 standard deviations.

appropriately to younger grant recipients that do not represent the majority of the parents. Nevertheless, it offers interesting insights into the education-related impact of the program. In 2010, individuals assigned to receive the grant were 26 percent more likely to have returned to school relative to the control counterparts. The intervention was even more effective among the younger cohorts since there is significant heterogeneity based on age. In 2010, individuals in the treatment group that were under 21 in 2008 were 54 percent more likely to have returned to school.

Finally, I shed more light into the educational effects of the grant by exploring self-assessed outcomes related to education and access to basic services (Table 9). Using a 9-step ladder where on the bottom stand the least educated children in the class and on the highest step stand the most educated ones, parents in the treatment group placed their children 6 percent higher than the control group -while assigned males place their children 8 percent higher than control males. Similarly, referring to a 1 to 9 scale where on the bottom stand the people in the community who have the least access to basic services (such as health and education), individuals assigned to receive the grant place their families 11 percent higher relative to the control. On the contrary, there appears to be no effect on self-assessed children health. These findings suggest that the intervention increased not only educational expenditures, but also subjective education-related outcomes.

6 Discussion and Conclusion

On average, the YOP program had a positive effect on education-related expenses. This suggests that the intervention supported investments not only in productive assets, but also in human capital. Different reasons could explain such an effect.

Educational expenses might be an expenditure item that people can afford only when released from credit constraints. Otherwise, it could be that the connection with a community facilitator (generally a local government employee, teacher, or community leader, presumably with higher than average education) increased the educational aspirations of the YOP group members and helped in shifting their attention towards the importance of the education of their children or younger family members. For example, Chiapa et al. (2012) show that Progresa raised the educational aspirations of beneficiary parents of a third of a school year through exposure to educated professionals. Similarly, Macours and Vakis (2014) show that in Nicaragua interaction with local leaders (who are generally more motivated and successful) augmented the impacts of a social program on households' investments in education and nutrition. Besides, it might be that the facilitator actively helped in boosting educational expenditures by suggesting a wise investment strategy.

I explore the role of these two group-specific program characteristics in order to shed light on the possible channels of the treatment impact on consumption. Table 10 suggests that the results are not driven by a pure income effect since the size of the grant received influences only food expenditures. On the contrary, the active presence of the facilitator is positively correlated with non-food expenditures and, in particular, educational expenditures.

However, it is not possible to test the actual mechanisms at work or to further disentangle all the channels that might have affected the findings. In fact, there are other factors that might have played a role in influencing the results on educational expenditures. Labelling the cash transfer for expenses in business tools and skills training might have had an endorsement effect that highlighted the importance of training/ schooling (Benhassine et al., 2015). Also, after four years, individuals assigned to receive the grant were more likely to practice a skilled trade and this better employment opportunity might have increased the perceived returns to education (Heath and Mobarak, 2015). In addition, it could be that the greater economic stability achieved by treated individuals through their higher earnings enabled them to have more cognitive resources available for their everyday life and parenting activities (Chiapa et al., 2016). Besides, the group feature of the intervention increased social interactions among members and hence it might have stimulated positive changes in parents' behaviors either through diffusion of social norms or information (Bobba and Gignoux, 2016).

While this group feature might have contributed to the success of the program and including a reserved quota for females might have helped in reducing certain gender gaps, forcing groups to comprise a third of female members might have also played a role in increasing redistributive pressures on women. Receiving more financial requests from non-family members might explain why, after four years, women assigned to receive the grant did not increase educational expenditures, whereas men assigned to receive the grant increased educational expenses by 24 percent (US$ 32).

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Tables

Table 1. Sample characteristics and balance test

Covariate in 2008 (baseline)	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) Difference	(6) p-value
Panel A. Treatment and Control - Full sample	Treatment		Control			
Age at baseline	25.14	5.31	24.76	5.22	0.38	0.55
Highest grade reached at school	7.82	3.03	7.95	2.92	-0.13	0.62
Number of war-related experiences	1.41	1.86	1.34	1.96	0.07	0.54
Number of biological children	2.66	1.83	2.45	1.6	0.21	0.14
Number of household members	5.14	2.67	5.12	2.74	0.02	0.64
Number of household minors	3.09	2.04	3.08	2.05	0.01	0.88
Mean age of minors	5.29	3.03	5.34	3.15	-0.05	0.85
Proportion of female minors	0.51	0.34	0.47	0.35	0.03	0.06
Distance to primary school (km)	1.89	2.6	1.87	3.16	0.03	0.44
Distance to secondary school (km)	5.29	6.92	5.09	7.61	0.21	0.52
Panel B. Treatment and Control - Females	Treatment		Control			
Age at baseline	24.10	5.05	23.90	5.62	0.20	0.35
Highest grade reached at school	6.97	3.12	7.16	3.06	-0.19	0.64
Number of war-related experiences	1.14	1.59	1.22	1.86	-0.08	0.20
Number of biological children	2.59	1.86	2.26	1.41	0.32	0.53
Number of household members	5.38	2.89	5.20	2.87	0.18	0.48
Number of household minors	3.43	2.20	3.23	2.02	0.19	0.45
Mean age of minors	5.59	3.16	5.23	3.11	0.36	0.22
Proportion of female minors	0.53	0.34	0.49	0.36	0.04	0.19
Distance to primary school (km)	1.93	3.16	1.77	1.81	0.16	0.93
Distance to secondary school (km)	5.22	7.99	4.35	6.06	0.87	0.87
Panel C. Females and Males	Females		Males			
Age at baseline	23.99	5.35	25.43	5.17	-1.44	0.00
Highest grade reached at school	7.07	3.09	8.29	2.83	-1.23	0.00
Number of war-related experiences	1.18	1.73	1.47	1.99	-0.29	0.00
Number of biological children	2.42	1.66	2.65	1.75	-0.22	0.04
Number of household members	5.29	2.88	5.05	2.60	0.24	0.02
Number of household minors	3.33	2.11	2.96	2.00	0.37	0.00
Mean age of minors	5.41	3.14	5.25	3.06	0.16	0.11
Proportion of female minors	0.51	0.35	0.48	0.34	0.03	0.16
Distance to primary school (km)	1.85	2.56	1.90	3.04	-0.05	0.68
Distance to secondary school (km)	4.78	7.07	5.40	7.35	-0.63	0.23

Notes: Panel A. and B., Column (6) reports the p-value of the OLS regression of the listed baseline characteristics on the indicator for random program assignment plus district fixed effects, with robust standard errors clustered at the group level. In Panel C., the listed baseline characteristics are regressed on the female dummy indicator.

Table 2. Intent-to-treat estimates of program impact on household consumption

	(1) HH consump-tion per capita	(2) HH consump-tion	(3) Ln(HH consump-tion)	(4) HH food consump-tion	(5) Ln(HH food consump-tion)	(6) HH non-food consump-tion	(7) Ln(HH non-food consump-tion)
	2012	2012	2012	2012	2012	2012	2012
Full sample ITT	3.5**	23.17***	0.072***	14.66***	0.057***	8.4***	0.047***
SE	(1.414)	(6.983)	(0.02)	(4.961)	(0.017)	(3.11)	(0.015)
Control mean	29.33	199.75	5.61	149.74	5.45	47.77	4.94
Male ITT	3.21*	22.42***	0.071***	12.77**	0.052**	9.25***	0.053***
SE	(1.837)	(8.657)	(0.024)	(6.186)	(0.021)	(3.548)	(0.018)
Control mean	30.53	204.97	5.63	156.29	5.48	46.19	4.93
Female ITT	4.04**	24.57**	0.074**	18.16**	0.066**	6.82	0.036
SE	(1.79)	(11.23)	(0.031)	(7.799)	(0.027)	(5.432)	(0.026)
Control mean	27.2	190.48	5.58	138.11	5.41	50.56	4.96
Female - Male ITT	0.83	2.15	0.004	5.39	0.014	-2.43	-0.018
SE	(2.405)	(13.92)	(0.038)	(9.73)	(0.032)	(6.228)	(0.03)
Observations	1,866	1,866	1,866	1,866	1,866	1,865	1,865
R-squared	0.142	0.240	0.251	0.211	0.211	0.196	0.225

Notes: Columns (1) to (7) report the ITT estimates of program impact for the full sample, males only, and females only. Robust standard errors are in brackets, clustered by group. The mean level of the dependent variable in the control group is reported below the standard error. Each ITT is calculated via a weighted least squares regression of the dependent variable on the program assignment indicator, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). The models from (2) to (7) include also the number of household members as a control. As in Blattman et al. (2014), the male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between program assignment and the female dummy; thus the female ITT is the sum of the coefficients on program assignment and this interaction. All consumption variables were top-censored at the 99th percentile to contain outliers and deflated to 2008 values. Columns (1), (2), (4), and (6) report values in 000s of Ugandan shillings. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Intent-to-treat estimates of program impact on household educational and health expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total educational expenses		Ln(Total educational expenses)		Total health expenses		Ln(Total health expenses)	
	2010	2012	2010	2012	2010	2012	2010	2012
Full sample ITT	40.11	28.8	0.067*	0.077*	6.79**	0.26	0.038***	0.008
SE	(28.72)	(21.67)	(0.04)	(0.042)	(2.725)	(2.792)	(0.014)	(0.014)
Control mean	272.06	250.63	5.4	5.41	29.14	29.61	4.82	4.8
Male ITT	56.51	54.78**	0.121**	0.122**	7.65**	-0.11	0.043**	0.004
SE	(36.06)	(24.52)	(0.05)	(0.048)	(3.472)	(3.445)	(0.018)	(0.017)
Control mean	270.6	225.47	5.39	5.37	29.82	31.34	4.82	4.81
Female ITT	8.49	-19.58	-0.037	-0.006	5.14	0.94	0.028	0.016
SE	(42.38)	(38.56)	(0.063)	(0.07)	(3.626)	(4.878)	(0.02)	(0.023)
Control mean	274.72	295.31	5.42	5.49	27.91	26.54	4.81	4.79
Female - Male ITT	-48.02	-74.36*	-0.158**	-0.128	-2.518	1.045	-0.014	0.012
SE	(53.52)	(43.89)	(0.078)	(0.08)	(4.718)	(6.022)	(0.026)	(0.028)
Observations	2,000	1,860	2,000	1,860	2,000	1,860	2,000	1,860
R-squared	0.159	0.214	0.249	0.252	0.109	0.133	0.121	0.122

Notes: Columns (1) to (8) report the ITT estimates of program impact for the full sample, males only, and females only. Robust standard errors are in brackets, clustered by group. The mean level of the dependent variable in the control group is reported below the standard error. Each ITT is calculated via a weighted least squares regression of the dependent variable on: the program assignment indicator, the number of household members, the number of household minors, the number of biological children, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). As in that article, the male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between program assignment and the female dummy; thus the female ITT is the sum of the coefficients on program assignment and this interaction. All consumption variables were top-censored at the 99th percentile to contain outliers and deflated to 2008 values. Columns (1), (2), (5), and (6) report values in 000s of Ugandan shillings. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Intent-to-treat estimates of program impact on educational and health expenditures for children or other young family members and for oneself

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Educational expenses for children and family members		Own educational expenses		Health expenses for children and family members		Own health expenses	
	2010	2012	2010	2012	2010	2012	2010	2012
Full sample ITT	29.71	23.46	7.23	4.72	4.45**	-0.29	1.47	1.19
SE	(18.26)	(17.99)	(12.18)	(6.498)	(1.851)	(1.178)	(1.098)	(1.35)
Control mean	193.39	199.59	39.3	21.17	17.57	14.22	9.11	10.85
Male ITT	53.14**	34.13*	3.5	6.55	5.32**	-0.56	2.0	1.09
SE	(23.07)	(19.72)	(14.97)	(8.393)	(2.387)	(1.49)	(1.389)	(1.685)
Control mean	175.54	171.56	47.74	23.5	18.55	15.51	8.39	10.58
Female ITT	-15.5	3.59	14.44	1.26	2.78	0.22	0.45	1.39
SE	(30.74)	(31.45)	(15.09)	(10.52)	(2.296)	(2.041)	(1.565)	(2.143)
Control mean	225.76	249.36	23.97	17.03	15.8	11.94	10.4	11.31
Female - Male ITT	-68.64*	-30.54	10.93	-5.29	-2.54	0.78	-1.56	0.3
SE	(38.77)	(34.7)	(18.51)	(13.65)	(3.08)	(2.579)	(2.002)	(2.677)
Observations	2,000	1,860	1,999	1,807	2,000	1,860	1,999	1,860
R-squared	0.212	0.251	0.113	0.106	0.117	0.077	0.062	0.123

Notes: Columns (1) to (8) report the ITT estimates of program impact for the full sample, males only, and females only. Robust standard errors are in brackets, clustered by group. The mean level of the dependent variable in the control group is reported below the standard error. Each ITT is calculated via a weighted least squares regression of the dependent variable on: the program assignment indicator, the number of household members, the number of household minors, the number of biological children, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). As in that article, the male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between program assignment and the female dummy; thus the female ITT is the sum of the coefficients on program assignment and this interaction. All consumption variables were top-censored at the 99th percentile to contain outliers, deflated to 2008 values, and refer to values in 000s of Ugandan shillings. ** $p<0.05$, * $p<0.1$

Table 5. Intent-to-treat estimates of program impact on educational and health expenditures for non-family members

	(1)	(2)	(3)	(4)
	Educational expenses for non-family members		Health expenses for non-family members	
	2010	2012	2010	2012
Full sample ITT	5.14	4.7	0.47	0.51*
SE	(3.7)	(4.258)	(0.292)	(0.27)
Control mean	18.05	17.54	1.35	1.34
Male ITT	3.28	10.63**	0.27	0.43
SE	(4.809)	(5.032)	(0.363)	(0.335)
Control mean	22.57	18.5	1.6	1.53
Female ITT	8.74*	-6.34	0.86*	0.66
SE	(4.61)	(6.983)	(0.466)	(0.497)
Control mean	9.84	15.83	0.9	0.99
Female - Male ITT	5.46	-16.97**	0.59	0.23
SE	(6.244)	(8.235)	(0.58)	(0.622)
Observations	1,999	1,860	2,000	1,860
R-squared	0.071	0.084	0.043	0.071

Notes: Columns (1) to (4) report the ITT estimates of program impact for the full sample, males only, and females only. Robust standard errors are in brackets, clustered by group. The mean level of the dependent variable in the control group is reported below the standard error. Each ITT is calculated via a weighted least squares regression of the dependent variable on: the program assignment indicator, the number of household members, the number of household minors, the number of biological children, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). As in that article, the male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between program assignment and the female dummy; thus the female ITT is the sum of the coefficients on program assignment and this interaction. All consumption variables were top-censored at the 99th percentile to contain outliers, deflated to 2008 values, and refer to values in 000s of Ugandan shillings. ** $p<0.05$, * $p<0.1$

Table 6. Heterogeneity of program impact on educational expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total educational expenses		Educ. expenses for children and family members		Own educational expenses		Educational expenses for non-family members	
	2010	2012	2010	2012	2010	2012	2010	2012
Panel 1. Heterogeneity for baseline wealth								
Assigned to treatment (ITT)	24.34	20.42	22.85	11.96	3.23	2.93	1.74	6.13
SE	(25.72)	(19.61)	(17.58)	(15.87)	(8.91)	(5.68)	(3.2)	(3.81)
ITT x Wealth index	37.28	40.98**	27.44	22.19	6.46	7.61	-2.49	4.02
SE	(25.04)	(19.17)	(17.12)	(15.51)	(8.67)	(5.53)	(3.11)	(3.72)
Observations	2,000	1,860	2,000	1,860	1,999	1,807	1,999	1,860
R-squared	0.153	0.207	0.198	0.236	0.108	0.093	0.071	0.078
Panel 2. Heterogeneity for having witnessed violence at baseline								
Assigned to treatment (ITT)	17.61	-0.06	7.93	-0.43	13.63	1.39	2.42	1.72
SE	(28.08)	(21.38)	(19.18)	(17.30)	(9.7)	(6.19)	(3.49)	(4.15)
ITT x Violence witnessed	9.24	52.0*	43.62	32.74	-41.06***	1.58	-1.5	13.48**
SE	(42.69)	(31.47)	(29.16)	(25.46)	(14.75)	(9.09)	(5.3)	(6.1)
Observations	2,000	1,860	2,000	1,860	1,999	1,807	1,999	1,860
R-squared	0.152	0.206	0.198	0.236	0.111	0.092	0.071	0.081
Panel 3. Heterogeneity for number of foster minors in the HH								
Assigned to treatment (ITT)	2.91	-8.98	12.52	-4.1	0.48	1.25	-0.56	0.25
SE	(26.96)	(20.55)	(18.42)	(16.65)	(9.29)	(5.98)	(3.36)	(3.95)
ITT x Numb. foster minors	64.83**	85.67***	27.76	47.13**	9.66	0.5	8.2**	19.81***
SE	(28.83)	(22.64)	(19.70)	(18.35)	(9.93)	(6.63)	(3.59)	(4.35)
Observations	1,966	1,828	1,966	1,828	1,965	1,775	1,965	1,828
R-squared	0.154	0.212	0.199	0.239	0.108	0.093	0.074	0.092
Panel 4. Heterogeneity for proportion of female minors in the HH								
Assigned to treatment (ITT)	35.04	28.65	27.36	30.36	9.02	-2.59	-7.18	3.81
SE	(52.89)	(41.24)	(37.44)	(34.38)	(15.71)	(9.76)	(6.85)	(7.52)
ITT x Prop. female minors	4.51	-35.76	17.73	-29.55	-21.12	8.07	14.34	-7.81
SE	(86.79)	(67.47)	(61.43)	(56.25)	(25.79)	(15.96)	(11.24)	(12.3)
Observations	1,338	1,257	1,338	1,257	1,337	1,224	1,337	1,257
R-squared	0.166	0.219	0.211	0.242	0.092	0.065	0.098	0.093
Panel 5. Heterogeneity for mean age of minors in the HH								
Assigned to treatment (ITT)	-42.14	53.67	-31.72	77.28*	6.69	0.41	-4.46	-10.29
SE	(61.54)	(47.3)	(43.56)	(39.43)	(18.05)	(11.21)	(8.07)	(8.6)
ITT x Minors' mean age	15.88	-8.82	13.57*	-12.49*	-1.69	0.25	0.92	2.0
SE	(10.17)	(7.71)	(7.2)	(6.43)	(2.98)	(1.82)	(1.33)	(1.4)
Observations	1,350	1,272	1,350	1,272	1,349	1,239	1,349	1,272
R-squared	0.178	0.222	0.223	0.243	0.089	0.068	0.097	0.095

Notes: Columns (1) to (8) report coefficients from a weighted least squares regression of the dependent variable on the listed interaction and independent variables, the number of household members, household minors, and biological children, district fixed effects, and a vector of control variables listed in the text. Robust standard errors are in brackets, clustered by group. All consumption variables were top-censored at the 99th percentile to contain outliers, deflated to 2008 values, and refer to values in 000s of Ugandan shillings. . *** p<0.01, ** p<0.05, * p<0.1

Table 7. Heterogeneity of program impact on educational expenditures for females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total educational expenses		Educational expenses for children and family members		Own educational expenses		Educational expenses for non-family members	
	2010	2012	2010	2012	2010	2012	2010	2012
Panel 1. Heterogeneity for baseline education (at least secondary)								
Assigned to treatment (ITT)	-51.88	-31.04	-59.78*	-20.04	2.85	-1.93	5.38	-0.88
SE	(46.38)	(41.79)	(33.86)	(35.79)	(13.42)	(9.0)	(5.59)	(6.76)
ITT x Education	174.6	10.73	176.8**	31.03	10.79	-14.34	14.93	-1.52
SE	(107.6)	(99.6)	(78.55)	(85.29)	(31.12)	(21.08)	(12.97)	(16.12)
Observations	667	627	667	627	666	607	666	627
R-squared	0.273	0.247	0.302	0.276	0.186	0.168	0.124	0.110
Panel 2. Heterogeneity for being married								
Assigned to treatment (ITT)	-40.66	-49.7	-55.94	-38.07	13.0	-7.51	7.55	0.95
SE	(57.83)	(51.25)	(42.14)	(43.96)	(16.7)	(11.08)	(6.94)	(8.28)
ITT x Married	52.44	44.33	67.62	50.44	-15.55	7.12	0.62	-4.91
SE	(81.7)	(73.7)	(59.52)	(63.2)	(23.61)	(15.91)	(9.81)	(11.9)
Observations	666	626	666	626	665	606	665	626
R-squared	0.263	0.250	0.290	0.276	0.182	0.162	0.127	0.117
Panel 3. Heterogeneity for number of groups one belongs to								
Assigned to treatment (ITT)	-211.5**	1.54	-174.3***	7.08	1.52	-6.11	4.8	-1.91
SE	(84.46)	(73.51)	(61.78)	(63.1)	(24.48)	(15.83)	(10.2)	(11.9)
ITT x Numb. groups	103.0***	-17.57	78.83***	-12.42	1.99	0.88	1.79	0.15
SE	(39.13)	(32.91)	(28.63)	(28.25)	(11.34)	(7.13)	(4.72)	(5.33)
Observations	667	627	667	627	666	607	666	627
R-squared	0.273	0.255	0.299	0.279	0.181	0.162	0.120	0.117
Panel 4. Heterogeneity for dissatisfaction with YOP group								
Assigned to treatment (ITT)	-18.68	-30.45	-26.35	-15.77	6.71	-4.25	7.86	-2.04
SE	(42.9)	(38.63)	(31.37)	(33.07)	(12.33)	(8.23)	(5.14)	(6.21)
ITT x Dissatisfaction YOP	9.35	31.54	5.13	27.18	-13.75	-11.06	1.71	17.55**
SE	(53.5)	(51.26)	(39.13)	(43.88)	(15.38)	(10.83)	(6.41)	(8.23)
Observations	667	627	667	627	666	607	666	627
R-squared	0.262	0.245	0.289	0.274	0.183	0.178	0.122	0.120
Panel 5. Heterogeneity for standard deviation of human capital within YOP group								
Assigned to treatment (ITT)	21.57	97.26	-10.03	25.81	43.23	32.05*	-17.35	0.42
SE	(97.97)	(88.64)	(71.61)	(75.95)	(28.19)	(18.98)	(11.77)	(14.32)
ITT x SD human capital YOP	-64.44	-253.5	-23.65	-79.09	-72.11	-72.44**	51.25**	-4.43
SE	(175.2)	(160.1)	(128.1)	(137.2)	(50.46)	(34.15)	(21.06)	(25.87)
Observations	660	620	660	620	659	600	659	620
R-squared	0.272	0.248	0.299	0.276	0.195	0.166	0.131	0.115

Notes: Columns (1) to (8) report coefficients from a weighted least squares regression of the dependent variable on the listed interaction and independent variables, the number of household members, the number of household minors, the number of biological children, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). Robust standard errors are in brackets, clustered by group. All consumption variables were top-censored at the 99th percentile to contain outliers, deflated to 2008 values, and refer to values in 000s of Ugandan shillings. *** p<0.01, ** p<0.05,* p<0.1

Table 8. Heterogeneity of program impact on educational outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Ratio of children attending school over children of school age			Returned to school		
		2010			2010	
Assigned to treatment (ITT)	-0.027	-0.035	0.021	0.026*	0.025	0.009
SE	(0.019)	(0.024)	(0.045)	(0.015)	(0.018)	(0.014)
ITT x Female		0.021			0.003	
SE		(0.036)			(0.029)	
Female		0.035			-0.059***	
SE		(0.027)			(0.023)	
ITT x Education			-0.006			
SE			(0.005)			
Education			0.009**			
SE			(0.004)			
ITT x Age under 21						0.077*
SE						(0.044)
Age under 21						0.008
SE						(0.04)
Control mean	0.89	0.88	0.9	0.1	0.07	0.16
Observations	1,067	1,067	1,067	2,005	2,005	2,005
R-squared	0.121	0.122	0.122	0.128	0.128	0.132

Notes: Columns (1) to (6) report coefficients from a weighted least squares regression of the dependent variable on the listed independent variables plus a program assignment indicator, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). The models (1), (2), and (3) include also the number of biological children as a control. Models (4) to (6) are estimated through a linear probability model to ease interpretation of the program impacts, but the results are the same as the marginal effects of a probit model. Robust standard errors are in brackets, clustered by group. The mean level of the dependent variable in the control group is reported in the last row. *** p<0.01, ** p<0.05, * p<0.1

Table 9. Intent-to-treat estimates of program impact on self-assessed educational and health measures

	(1)	(2)	(3)
	Self-assessed children education, on a scale from 1 to 9	Self-assessed children health, on a scale from 1 to 9	Self-assessed access to basic services such as education and health, on a scale from 1 to 9
	2010	2010	2010
Full sample ITT	0.21**	0.13	0.41***
SE	(0.107)	(0.107)	(0.097)
Control mean	3.47	4.78	3.68
Male ITT	0.29**	0.15	0.42***
SE	(0.129)	(0.127)	(0.115)
Control mean	3.46	4.71	3.74
Female ITT	0.07	0.08	0.38**
SE	(0.171)	(0.188)	(0.17)
Control mean	3.47	4.9	3.56
Female - Male ITT	-0.22	-0.07	-0.04
SE	(0.206)	(0.224)	(0.202)
Observations	1,728	1,783	1,983
R-squared	0.119	0.087	0.101

Notes: Columns (1) to (3) report the ITT estimates of program impact for the full sample, males only, and females only. Robust standard errors are in brackets, clustered by group. The mean level of the dependent variable in the control group is reported below the standard error. Each ITT is calculated via a weighted least squares regression of the dependent variable on a program assignment indicator, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). As in that article, the male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between program assignment and the female dummy; thus the female ITT is the sum of the coefficients on program assignment and this interaction. *** $p<0.01$, ** $p<0.05$

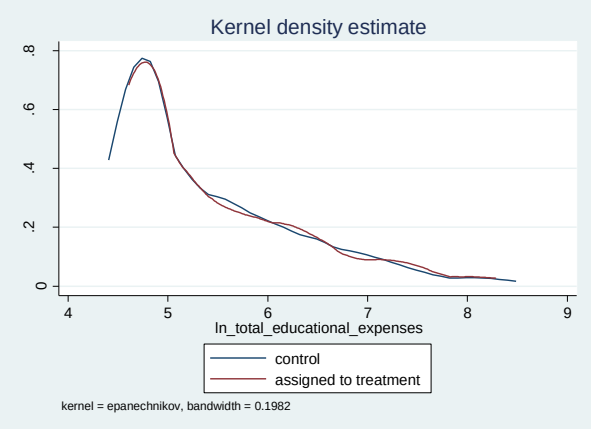
Table 10. Association between household expenditures and group-specific program characteristics

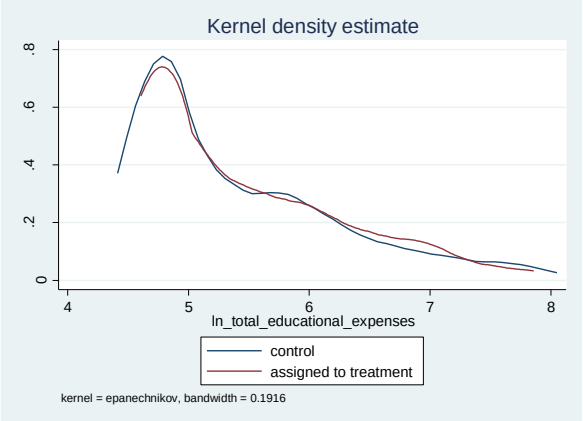
	Dependent variables (standardized z-score), pooled endline surveys					
	(1)	(2)	(3)	(4)	(5)	(6)
	HH food consump-tion	HH non food consump-tion	Total educational expenses	Educational expenses for children and family members	Total health expenses	Health expenses for children and family members
	2012	2012	2010, 2012	2010, 2012	2010, 2012	2010, 2012
Grant size per person (z-score)	0.084*	-0.019	0.053	-0.03	0.024	0.051
	(0.051)	(0.042)	(0.044)	(0.034)	(0.045)	(0.046)
Observations	810	810	1,676	1,676	1,676	1,676
Facilitator/M&E advisor provided further support (z-score)	0.018	0.081	0.105***	0.054	-0.009	-0.022
	(0.05)	(0.057)	(0.039)	(0.033)	(0.035)	(0.04)
Observations	554	554	1,276	1,276	1,276	1,276
Facilitator monitored group performance (z-score)	0.041	0.106*	0.064	0.022	0.024	-0.001
	(0.048)	(0.058)	(0.043)	(0.035)	(0.034)	(0.04)
Observations	550	550	1,268	1,268	1,268	1,268
Facilitator provided business advice (z-score)	0.007	0.066	0.125***	0.05	-0.023	-0.008
	(0.049)	(0.057)	(0.042)	(0.033)	(0.03)	(0.038)
Observations	550	550	1,268	1,268	1,268	1,268
Facilitator provided advice on profit sharing/ spending (z-score)	0.049	0.098*	0.083**	0.053*	0.006	-0.012
	(0.047)	(0.058)	(0.04)	(0.031)	(0.033)	(0.036)
Observations	550	550	1,268	1,268	1,268	1,268
Months during which the facilitator supported the group (z-score)	-0.028	0.009	0.042**	0.042**	0.025	0.008
	(0.038)	(0.029)	(0.02)	(0.021)	(0.035)	(0.031)
Observations	551	551	1,267	1,267	1,267	1,267
Performance of the facilitator (z-score)	0.023	0.106*	0.085**	0.042	0.018	0.011
	(0.049)	(0.059)	(0.037)	(0.031)	(0.034)	(0.038)
Observations	549	549	1,264	1,264	1,264	1,264
Facilitator provided further support or continued to work with group (z-score)	0.043	0.092*	0.055*	0.04	0.052*	0.038
	(0.047)	(0.051)	(0.032)	(0.031)	(0.031)	(0.038)
Observations	571	571	1,315	1,315	1,315	1,315

Notes: Columns (1) to (6) report coefficients from a weighted least squares regression of the dependent variable on the listed independent variables plus an indicator for the 2012 survey, the number of household members, the number of household minors, the number of biological children, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). All consumption variables were top-censored at the 99th percentile to contain outliers and deflated to 2008 values. *** p<0.01, ** p<0.05, * p<0.1

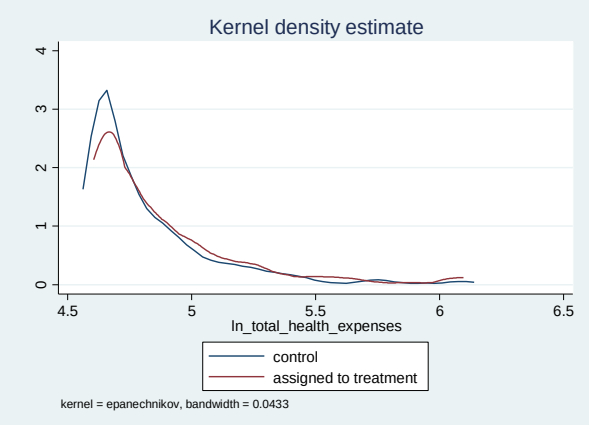
Appendix

a. Educational expenditures, 2010 and 2012





b. Health expenditures, 2010 and 2012



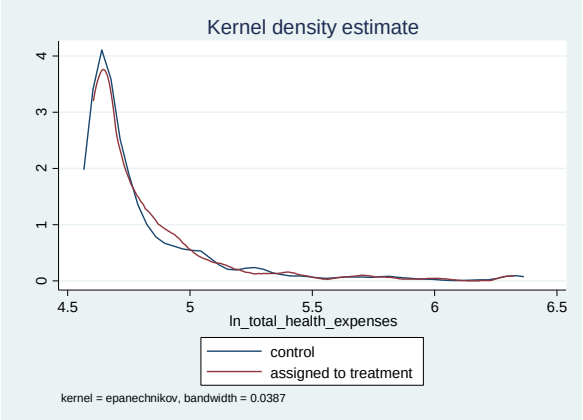


Figure A1. Kernel densities of educational (a) and health expenditures (b)

Table A1. Sensitivity analysis of intent-to-treat consumption estimates to the use of an instrumental-variable model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	HH food consump-tion	HH non food consump-tion	Total educational expenses		Educational expenses for children and family members		Total health expenses		Health expenses for children and family members	
	2012	2012	2010	2012	2010	2012	2010	2012	2010	2012
Full sample ToT	16.67***	9.55***	45.7	32.72	33.84	26.65	7.74**	0.3	5.07**	-0.33
SE	-5.635	(3.569)	(32.62)	(24.7)	(20.73)	(20.53)	(3.093)	(3.171)	(2.099)	(1.339)
Control mean	149.74	47.77	272.06	250.63	193.39	199.59	29.14	29.61	17.57	14.22
Male ToT	14.38**	10.36***	62.82	60.9**	58.94**	38.07*	8.53**	-0.11	5.92**	-0.62
SE	(6.894)	(3.97)	(39.99)	(27.39)	(25.55)	(22.08)	(3.851)	(3.839)	(2.647)	(1.661)
Control mean	156.29	46.19	270.6	225.47	175.54	171.56	29.82	31.34	18.55	15.51
Female ToT	21.13**	7.97	10.77	-22.2	-17.35	4.41	6.12	1.09	3.33	0.25
SE	(9.006)	(6.343)	(49.6)	(44.52)	(36.04)	(36.39)	(4.248)	(5.637)	(2.685)	(2.358)
Control mean	138.11	50.56	274.72	295.31	225.76	249.36	27.91	26.54	15.8	11.94
Female - Male ToT	6.75	-2.387	-52.06	-83.1*	-76.29*	-33.66	-2.41	1.2	-2.59	0.87
SE	(11.01)	(7.113)	(60.79)	(49.92)	(44.23)	(39.51)	(5.35)	(6.83)	(3.48)	(2.923)
Observations	1,866	1,865	2,000	1,860	2,000	1,860	2,000	1,860	2,000	1,860
R-squared	0.212	0.193	0.159	0.214	0.212	0.250	0.109	0.133	0.118	0.076

Notes: Columns (1) to (10) report the Treatment-on-the-Treated estimates of program impact for the full sample, males only, and females only. Robust standard errors are in brackets, clustered by group. ToT estimates are calculated via two-stage least squares, where assignment to treatment is used as an instrument for having received the grant. Weights and controls used are identical to the ITT counterparts. All consumption variables were top-censored at the 99th percentile to contain outliers, deflated to 2008 values, and refer to values in 000s of Ugandan shillings. *** p<0.01, ** p<0.05, * p<0.1

Table A2. Intent-to-treat estimates of program impact on other types of household expenditures

	(1) Clothes/ shoes expenses for males over 16	(2) Clothes/ shoes expenses for females over 16	(3) Clothes/ shoes expenses for male minors under 16	(4) Clothes/ shoes expenses for female minors under 16	(5) Expenses for educational material	(6) Expenses for medical treatments and medicines
	2012	2012	2012	2012	2012	2012
Full sample ITT	7.97***	7.07***	2.86*	1.69	8.24**	6.38
SE	(2.497)	(2.391)	(1.534)	(1.421)	(3.449)	(4.717)
Control mean	34.69	34.27	25.87	24.45	48.37	63.02
Male ITT	7.11**	6.07**	3.77*	1.16	9.94**	2.57
SE	(3.224)	(3.053)	(1.996)	(1.589)	(4.179)	(6.012)
Control mean	38.82	37.4	26.24	24.75	45.79	65.0
Female ITT	9.58**	8.93**	1.16	2.67	5.07	13.49*
SE	(4.155)	(3.672)	(2.295)	(2.739)	(5.858)	(7.372)
Control mean	27.19	28.67	25.19	23.9	52.99	59.46
Female - Male ITT	2.48	2.86	-2.61	1.52	-4.88	10.92
SE	(5.375)	(4.722)	(3.029)	(3.141)	(7.097)	(9.434)
Observations	1,848	1,853	1,850	1,851	1,855	1,852
R-squared	0.106	0.119	0.137	0.119	0.177	0.072

Notes: Columns (1) to (6) report the ITT estimates of program impact for the full sample, males only, and females only. Robust standard errors are in brackets, clustered by group. The mean level of the dependent variable in the control group is reported below the standard error. Each ITT is calculated via a weighted least squares regression of the dependent variable on: the program assignment indicator, the number of household members, the number of household minors, the number of biological children, district fixed effects, and a vector of control variables listed in the text and described in Blattman et al. (2014). As in that article, the male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between program assignment and the female dummy; thus the female ITT is the sum of the coefficients on program assignment and this interaction. All consumption variables were top-censored at the 99th percentile to contain outliers, deflated to 2008 values, and refer to values in 000s of Ugandan shillings. *** p<0.01, ** p<0.05, * p<0.1