# Beyond Productivity Effects: Cash transfers and Investments in Education in Post-war Uganda[1]

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Calderone, Margherita[2]

**Abstract**

This paper looks at the secondary effects of a grant, the Youth Opportunities Program (YOP), on human capital investments in conflict-affected Northern Uganda. The YOP grant was aimed at providing start-up money to groups of underemployed youth, while working in practice as an Unconditional Cash Transfer program. It kept a gender balance by mandating that the groups contained at least a third of female members. 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). Specifically, men assigned to receive the grant increased total educational expenses by about US$ 32 both after two and four years (21-24 percent), whereas total educational expenditures of women in the treatment group did not increase. Female recipients seem to have not made more substantial investments in education, at least in part, because of stronger external influences, such as probable financial requests from other members of their YOP group. These results suggest that the intervention had on average a positive impact on investments in education, but it had a lower effect on women possibly due to its group-structure.

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[2] Applied Economist, World Bank, Education Global Practice, 1818 H Street NW, Washington, DC 20433, U.S.A., mcalderone1@worldbank.org; and Research Affiliate with the Households in Conflict Network (HiCN)

## 1. Introduction

Low levels of investment in productive activities and human capital often constitute a key constraint for households to escape poverty (Macours and Vakis, 2014). 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 (for example, Montenegro and Patrinos, 2014 show that Afghanistan and Iraq have the lowest returns to schooling in the world). Hence, many development projects, especially reconstruction interventions, are directed to support the investments of the lower-income or most vulnerable individuals.

In particular, cash transfers for poor households are aimed at alleviating poverty in the short run by providing money and, in the longer run, at breaking the intergenerational transmission of poverty by inducing investments in child education and health – usually through conditionalities. Evidence from numerous countries suggest that these programs are generally successful in reaching their primary objectives and generating 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 have missed opportunities to be part of broader programs to alter productive activities and sustain growth. Most conditional cash transfers in Latin America have been shown to have little impact on work incentives and adult labor supply (see Asfaw et al., 2012, p. 8, for a review and Alzua et al., 2012, for comparable results from three countries).

On the other hand, in spite of the general success, social programs are seldom effective in conflict-effected countries. For instance, Beath et al. (2012) show that the National Solidarity Program, the largest aid intervention in Afghanistan that included the disbursement of block grants to rural villages, had no lasting economic impact (in spite of positive non-economic effects on subjective wellbeing, attitudes towards the government, and violence).

In this sense, the Youth Opportunities Program (YOP), a grant that provided start-up money to groups of underemployed youth in post-conflict Northern Uganda, represents a remarkable success story. Blattman et al. (Blattman, Fiala, and Martinez, 2014 - B.F.M. hereinafter) 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 this literature showing that the YOP also increased investments in human capital, particularly educational expenditures.

It contributes to the strand of the literature analyzing the consumption impact of cash transfers, either conditional (Angelucci et al., 2012, and Gertler et al., 2012, in Mexico; Attanasio et al., 2011, in Colombia; Cruz and Ziegelhöfer, 2014, in Brazil; Macours et al., 2012, in Nicaragua; Schady and Rosero, 2008, in Ecuador; Shi, 2012 in China on school-fee reductions) or unconditional (Bazzi et al., 2013 and Sumarto and de Silva, 2103, in Indonesia; Haushofer and Shapiro, 2013, in Kenya). It also adds to the evidence on the unintended positive effects of development interventions (the YOP transfer was meant as a business grant). For instance, Ozier (2014) finds that a deworming intervention targeting primary-school pupils in Kenya had large cognitive effects for children who were less than one year old when their older siblings received the treatment. Also, Angelucci and De Giorgi (2009) show that the Mexican cash transfer PROGRESA had indirect effects, through the informal credit market, on expenditures of non-eligible households that were living in the same villages of beneficiaries. Furthermore, this study is linked to the literature on the impact of aid programs in Uganda, a country with one of the youngest populations in the world (49 percent of its citizens are under the age of 14[3]). 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 and underinvestment in education (B.F.M., 2014; Bandiera et al., 2014; Karlan and Linden, 2013). I show that also the YOP program was effective in increasing education-related expenses, especially for men.

Males assigned to receive the grant increased total educational expenses by 21-24 percent (US$ 32) both in the shorter and longer run (after two and four years), whereas total educational expenditures of females in the treatment group did not increase. The effect for males seems to be driven by a growth in expenses on their own children and family

[3] Niger is the only country with a higher number (50 percent, World Development Indicators 2012, available on the World Bank Data's webpage). This demographic characteristic of Uganda is the result of lower mortality but still high fertility.

members. On the contrary, women did not change their family spending patterns. They did however increase their educational and health 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 the female recipients did not manage to invest more on the education of their family because after receipt of the grant they were affected by more money requests from others. The YOP groups were forced to include a third of female members. Hence, the external influences 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, from the time of the baseline, were dissatisfied with their YOP group and women belonging to YOP groups with higher human capital disparity did spend more for the education of external individuals. Another hypothesis is that the female recipients did not increase educational investment 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 females did spend more for the education of their children and family members.

In spite of its lower effect on women, the YOP grant had on average a positive impact on investments in education. This finding is supported by evidence that the intervention also increased subjective education-related outcomes by 6 percent overall (by 8 percent for males). Interestingly, the results do not seem to be driven by a pure income effect from the money injection since the size of the grant received influenced only food expenditures.

The rest of the paper is organized as follows. Section 2 describes the context of the intervention, along with the details of it and of its impact evaluation. Section 3 provides further information on the data and the descriptive statistics. Section 4 explains the identification strategy and shows the results. Finally, Section 5 discusses the findings and offers concluding remarks.

## 2. Background and Experimental Design

### 2.1. Background

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 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 these camps health conditions were very poor and diseases and fatalities had high incidence (Bozzoli and Brück, 2010). 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. Although the large-scale movement of IDPs from camps did not gain momentum until 2008, by 2007 about a million displaced persons had already voluntary left the camps.

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 (2012) finds that displaced households had lower consumption (20 percent lower in 2008, two years after returning) and fewer assets (a one-fifth standard deviation decrease in 2008) than non-displaced households. While wealthier households recovered part of their consumption by 2008, poorer households remained trapped in a lower equilibrium. Rockmore (2011) illustrates that not only direct exposure to violence can generate losses, but also the risk of violence can have significant effects (the author estimates that it reduced national GDP by 4-8 percent). Bozzoli et al. (2011) show that recent 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.

### 2.2. 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. 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. It started in 2003 with a US$ 100 million credit from the World Bank. To foster the recovery of the conflict-affected young generation and to boost non-agricultural employment, in 2006, the government added to the NUSAF an extra component: the Youth Opportunities Program or YOP.

The YOP was meant to raise youth employment and earnings and to improve community reconciliation and thus targeted underemployed youth aged between 16 and 35. The YOP component required young adults from the same village to organize into a group of about 20 members and submit a proposal for a cash transfer to pay for technical training, tools, and materials for starting a skilled trade. The eligibility criteria required 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 and spending all funds. 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 B.F.M. (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. Out of the 535 remaining eligible groups (about 12,000 members), 265 received funding and 270 did not. B.F.M. (2014) report that treatment and control groups/ villages were typically very distant from each other and thus spillovers were unlikely.

## 3. Data

### 3.1. 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).

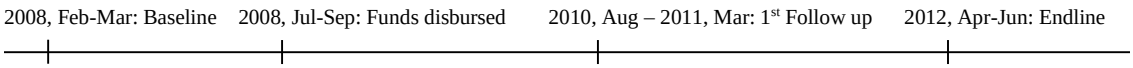


**Figure 1.** Timeline of the events

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). B.F.M. (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 (about the mean annual income) and invested some of it in training, but most of it in tools and materials.

B.F.M. (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.

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.

### 3.2. 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 $8^{th}$ 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 two 30-year old adults and three 5-year old minors, half of which are females. 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.

Column (6) of Table 1 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.

## 4. Methodology and Results

### 4.1. Estimation Method

My estimation is based on the following regression:

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

where T is an indicator for assignment to treatment, X is a set of baseline covariates at the individual and household level, $\phi$ 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, initial level of capital and credit access, employment type and levels, and variables capturing group characteristics (as in B.F.M., 2014 to ensure comparability).[4] This set of covariates corrects for any baseline imbalance

[4] 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

and guarantees similarity between the treatment and the control groups. I use the survey weights, so the observations are weighted by their inverse probability of selection into the endline tracking. The treatment effect is estimated by β and the 2010 and 2012 impacts are evaluated separately.

For various reasons[5], 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.

My main outcomes of interest are household consumption and educational and health expenditures. Dealing with monetary variables, the treatment effect might be biased by extreme values, so I cap all currency-denominated variables at the 99$^{th}$ percentile. For comparability, I also deflate all values to the 2008 correspondent.[6]

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 computations coming from 135 different questions.

### 4.2. Impacts on Household Expenditures

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

leadership position in group (dummy); group chair or vice-chair (dummy). All indicators refer to the baseline values.

[5] See B.F.M. (2014) for an explanation.

[6] In particular, I deflate by 1.22 in 2010 and by 1.61 in 2012 as in B.F.M. (2014).

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 compared to control males. 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 interesting gender differences. While women assigned to receive the grant spent about US$ 10 more on food consumption, they only spent US$ 3-4 more on non-food consumption. On the contrary, men in the treatment group increased food consumption by 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.

I focus on total expenditures for education and health made in the 12 months before the survey. Aggregate educational and health expenditures refer to own expenses, expenses for children and family members, and expenses for other (not better specified) non-family members. Since total expenditures might go beyond expenditures for household members, in Table 3 I consider aggregate measures instead of per capita indicators, while controlling for the number of household members, the number of household minors, and the number of biological children.[7]

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

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

US$ 4), but the effect is close to zero after four years. The results are confirmed by the illustration of the relative log distributions (Figure A1).[8]

Passing 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. On the contrary, there is no significant gender heterogeneity on health expenditures (even if the 2010 treatment effect is slightly higher for males). 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 disaggregating total expenses in expenditures for children or young family members, non-family members, and own expenses, the treatment effect for males seems to be driven mostly by a statistically significant growth in expenses on children and family members (Table 4). After four years, males assigned to receive the intervention also 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 treatment effect on monthly earnings reported in B.F.M., 2014) they received more requests for informal credit and they were more likely to transfer money outside of the household. However, for males, family seems to come first and 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.

On the other hand, in 2010, educational and health expenditures for non-family members increased by 90-95 percent among females in the treatment group. This result suggests 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

[8] 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.

reflect poor female decision power. B.F.M. (2014) show that the cash transfer substantially increased economic outcomes for females, but it did not improve their social position nor their empowerment level. 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 at least in part because of stronger external influences.

Table 5 reproduces the treatment impact using the ToT estimations. As expected, the results are similar to the ITT estimates with the only difference that the magnitudes of the effects are about 2 percent higher since they now refer to those that did indeed receive the money.

Finally, Table 6 shows the program impact on different types of household expenditures measured in more detail in the consumption module of the final endline survey. To check whether my findings are consistent with the results on other expenditure dimensions, I look at expenditures for clothes, shoes, and other material for adults versus minors (separated in males and females),[9] expenditures for educational materials (i.e. books, stationary, and school uniforms), and expenditures for medical treatments and medicines. Table 6 offers a picture that confirms previous findings.

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.[10] Contemporarily, their medical expenses relevantly increased by 23 percent, while their expenses on educational materials increased by only 10 percent.

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[9] For instance, Schluter and Wahba (2010) use this outcome indicator (expenditures on clothing of children versus adults) to test whether parents in Mexico are altruistic and care about their children.

[10] 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.

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.

### 4.3. Heterogeneous Impacts

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

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

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 $\beta$ and $\gamma$ and if $\gamma$ is significantly different from zero then there is evidence of heterogeneity in the treatment effect for that trait.

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. As an outcome variable, I focus only on educational expenditures since they show a more consistent post-intervention increase than health expenditures and might offer more useful insights into the differentiated effects of the program. Table 7 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 behavior towards their neighbors (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 relevant especially for females: 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 8 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 external influences 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.

### 4.4. 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 designed to reply to such question and, given the lack of suitable outcome indicators, it is not possible to give a clear answer. Table 9 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.[11] 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, 2013). 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 (2013) from rural and peri-urban Uganda.[12] 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., 2011).

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 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

[11] 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.

[12] 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.

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 exploring self-assessed outcomes related to education and access to basic services (Table 10).

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.

Overall, these findings suggest that the intervention increased not only educational expenditures, but also subjective education-related outcomes.

## 5. Discussion and Conclusion

There are different reasons that could explain the effectiveness of the YOP program in fostering household investments in education.

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, in Mexico, 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; it also

affected households' attitudes towards the future and increased aspirations by providing role models. 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 11 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., 2013). 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, 2014). 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., 2014; Mani et al., 2013). Besides, the group feature of the intervention might have stimulated peer effects among members, either through social norms or information sharing (Bobba and Gignoux, 2014).

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 external financial requests on women. Receiving more requests from external individuals 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).

Nonetheless, looking at the average impact, the intervention had a positive effect on education-related expenses. This suggests that the YOP program supported investments not only in productive assets, but also in human capital.

**Table 1. Sample characteristics and balance test**

| | Treatment | | Control | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Covariate in 2008 (baseline) | Mean | SD | Mean | SD | Difference | p-value |
| Age | 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 adult members | 1.8 | 1.51 | 1.77 | 1.5 | 0.03 | 0.60 |
| Mean age of adults | 30.54 | 10.11 | 30 | 10.18 | 0.54 | 0.55 |
| Mean education of adults | 2.62 | 1.18 | 2.65 | 1.13 | -0.03 | 0.99 |
| 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 |

Notes: 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.

**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 B.F.M. (2014). The models from (2) to (7) include also the number of household members as a control. As in B.F.M. (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 99$^{th}$ 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 B.F.M. (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 99$^{th}$ 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 disaggregated in expenses for children and family members, expenses for non-family members, and own expenses**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Educational expenses for children and family members | | Educational expenses for non-family members | | Own educational expenses | | Health expenses for children and family members | | Health expenses for non-family members | | Own health expenses | |
| | 2010 | 2012 | 2010 | 2012 | 2010 | 2012 | 2010 | 2012 | 2010 | 2012 | 2010 | 2012 |
| Full sample ITT | 29.71 | 23.46 | 5.14 | 4.7 | 7.23 | 4.72 | 4.45** | -0.29 | 0.47 | 0.51* | 1.47 | 1.19 |
| SE | (18.26) | (17.99) | (3.7) | (4.258) | (12.18) | (6.498) | (1.851) | (1.178) | (0.292) | (0.27) | (1.098) | (1.35) |
| Control mean | 193.39 | 199.59 | 18.05 | 17.54 | 39.3 | 21.17 | 17.57 | 14.22 | 1.35 | 1.34 | 9.11 | 10.85 |
| Male ITT | 53.14** | 34.13* | 3.28 | 10.63** | 3.5 | 6.55 | 5.32** | -0.56 | 0.27 | 0.43 | 2.0 | 1.09 |
| SE | (23.07) | (19.72) | (4.809) | (5.032) | (14.97) | (8.393) | (2.387) | (1.49) | (0.363) | (0.335) | (1.389) | (1.685) |
| Control mean | 175.54 | 171.56 | 22.57 | 18.5 | 47.74 | 23.5 | 18.55 | 15.51 | 1.6 | 1.53 | 8.39 | 10.58 |
| Female ITT | -15.5 | 3.59 | 8.74* | -6.34 | 14.44 | 1.26 | 2.78 | 0.22 | 0.86* | 0.66 | 0.45 | 1.39 |
| SE | (30.74) | (31.45) | (4.61) | (6.983) | (15.09) | (10.52) | (2.296) | (2.041) | (0.466) | (0.497) | (1.565) | (2.143) |
| Control mean | 225.76 | 249.36 | 9.84 | 15.83 | 23.97 | 17.03 | 15.8 | 11.94 | 0.9 | 0.99 | 10.4 | 11.31 |
| Female - Male ITT | -68.64* | -30.54 | 5.46 | -16.97** | 10.93 | -5.29 | -2.54 | 0.78 | 0.59 | 0.23 | -1.56 | 0.3 |
| SE | (38.77) | (34.7) | (6.244) | (8.235) | (18.51) | (13.65) | (3.08) | (2.579) | (0.58) | (0.622) | (2.002) | (2.677) |
| Observations | 2,000 | 1,860 | 1,999 | 1,860 | 1,999 | 1,807 | 2,000 | 1,860 | 2,000 | 1,860 | 1,999 | 1,860 |
| R-squared | 0.212 | 0.251 | 0.071 | 0.084 | 0.113 | 0.106 | 0.117 | 0.077 | 0.043 | 0.071 | 0.062 | 0.123 |

Notes: Columns (1) to (12) 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 B.F.M. (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 99$^{th}$ 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. 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 99$^{th}$ 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 6. 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 B.F.M. (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 99$^{th}$ 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

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

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 B.F.M. (2014). Robust standard errors are in brackets, clustered by group. All consumption variables were top-censored at the 99$^{th}$ 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 expenditures for females

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

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 B.F.M. (2014). Robust standard errors are in brackets, clustered by group. All consumption variables were top-censored at the 99$^{th}$ 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 9. 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 B.F.M. (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 10. 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 B.F.M. (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 11. 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 B.F.M. (2014). All consumption variables were top-censored at the 99$^{th}$ percentile to contain outliers and deflated to 2008 values. *** p<0.01, ** p<0.05, * p<0.1

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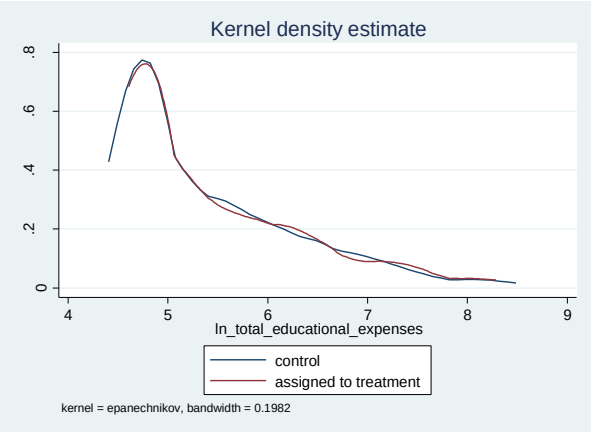
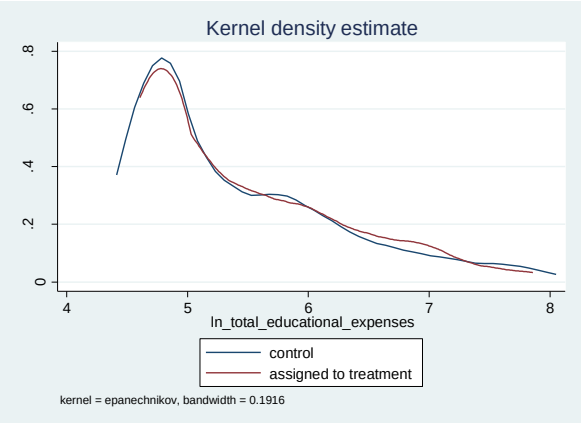
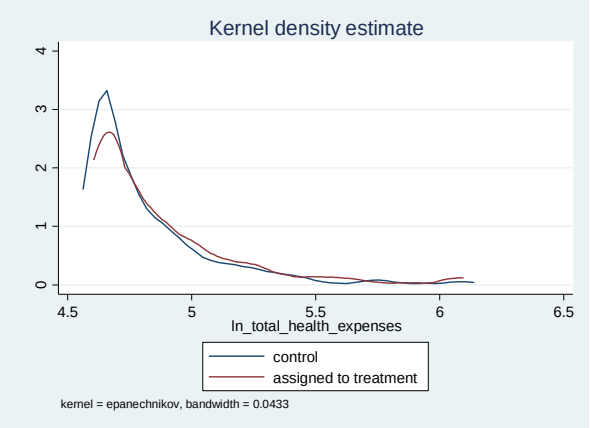
# Appendix

a. Educational expenditures




b. Health expenditures



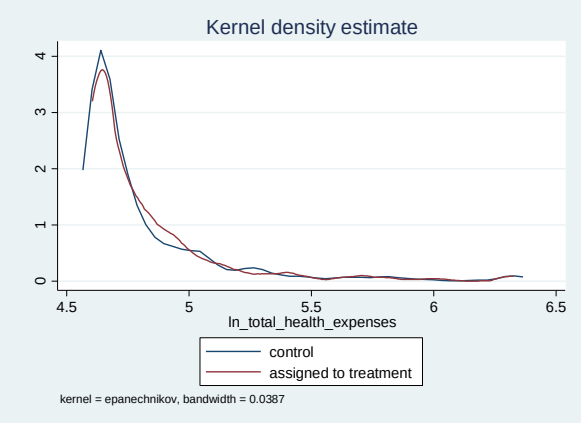




**Figure A1.** Kernel densities of educational (a) and health expenditures (b)