

No Family Left Behind: Cash transfers and Investments in Education in Post-war Uganda¹

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Abstract

This paper looks at the secondary effects of a business grant, the Youth Opportunities Program, on household expenditures for education and health in conflict-affected Northern Uganda. I find that education-related expenses increase as a result of the intervention. More specifically, males assigned to receive the grant increased total educational expenses by 21% after two years and by a significant 24% after four years, whereas educational expenditures of females in the treatment group decreased. The results suggest that female grant recipients did not manage to make more substantial investments for their children at least in part because of stronger external influences.

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I. Introduction

Social protection programs that target poor households have become an important component of social policy in several developing countries. They aim to alleviate poverty in the short run by providing cash and, in the longer run, to break 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 enrolment or use of health services. However, their focus on human capital accumulation of the young has led to some criticism because they might miss opportunities to be part of broader programs to foster growth and alter productive activities. For example, 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 for a review).

On the contrary, business grants aim at encouraging business and employment growth. Research finds that these programs are often effective, with capital having high returns both for men in established firms and farms (de Mel, McKenzie, and Woodruff, 2008 and Udry and Anagol, 2006) and for female business entrants (Blattman, Fiala, and Martinez, 2014 and de Mel, McKenzie and Woodruff, 2012). For instance, Blattman, Fiala, and Martinez (2014 - B.F.M., 2014 hereinafter) study the impact of the Youth Opportunities Program (YOP) in the conflict-affected north of Uganda, where groups of youth were invited to submit grant proposals for vocational training and business start-up. The authors show impressive results: after four years, grant recipients had a 50% likelihood to practice a skilled trade and experienced increases in assets, work hours, and earnings.

This paper contributes to this strand of the literature on the impact of cash transfers and expands it by looking at the secondary effects of the YOP business grant on household expenditures in education and health - or investments in child human capital (Cruz and Ziegelhöfer, 2014). It also adds to the evidence on the impact of aid programs in Uganda, a country with one of the youngest populations in the world (49% of its citizens is under the age of 14³). Recent evidence suggests that aid programs targeted to the country's youth were successful in tackling a range of issues from lack of skills to risky health behaviors to underinvestment in education (B.F.M., 2014; Bandiera et al., 2012; and Karlan and Linden, 2013). I show that also the YOP program was effective in increasing education-related expenses, especially for men.

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

Males assigned to receive the grant increased total educational expenses by 21% after two years and by a significant 24% after four years (compared to control males), whereas educational expenditures of females in the treatment group decreased (compared to expenditures of control females). The results suggest that female grant recipients did not manage to make more substantial investments for their children at least in part because of stronger external influences.

II. Context and Experimental Design

II.A. The Context: The History of Violence in Northern Uganda

In the late 1980s, the Ugandan political situation degenerated when the south-based National Resistance Army (NRA) led 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. Blattman and Annan (2010) report that between 60,000 and 80,000 youth were abducted, mostly after 1996 and from one of the Acholi districts of the north. 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 and forced to commit various crimes. 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; diseases and fatalities had high incidence (Bozzoli and Brück, 2010). In 2006, the Ugandan government 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 decision to return was voluntary and shaped by individual war experiences and abilities versus services offered in the camps (Bozzoli, Brück, and Muhumuza, 2012). Although the large-scale movement of IDPs from camps did not gain momentum until 2008, by 2007 about a million of displaced persons had already voluntarily 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 (28-35% lower in 2004 and 20% lower in 2008, two years after returning) and fewer assets (initially a half standard deviation

decrease in value and a one-fifth decrease later) 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, over the duration of the conflict, the risk of violence reduced per capita expenditure in the affected regions by about 70% and national GDP by 4-8%. Bozzoli, Brück, and Muhumuza (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 disproportionately affected the younger generation. In such post-conflict context, the recovery of children and young adults is a critical concern since lost education can take years to regain and psychological effects may be long-lasting.

II.B. The Youth Opportunities Program and The Experimental Design of its 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 US\$ 100 million IDA 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 NUSAF an extra component: the Youth Opportunities Program or YOP.

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. Many applicants were functionally illiterate and so 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, with no subsequent government monitoring. Full details of the program 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 villages were typically very distant from each other and thus spillovers were unlikely.

III. Data

III.A. 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 February-March 2008, a follow-up happened between August 2010 and March 2011, and the endline survey was collected in April-June 2012, while the government disbursed funds in July-September 2008. Attrition was minimized with a two-step tracking strategy that allowed to reach satisfactory effective response rates (85% in 2010 and 82% 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 imbalance or potentially selective attrition.

The sample is mostly composed of young rural farmers with low earnings (less than a dollar 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% relative to control men, while treatment women increased their stocks by more than 100% relative to control women. Treatment also increased total hours worked

per week by 17%, 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% and women's earnings by 73% and it increased both durable assets and non-durable consumption by 0.18 standard deviations. Finally, the program improved subjective wellbeing by about 13%, 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.

III.B. 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 a 8th grade education, which corresponds to a completed primary education level. On average, they experienced at least one war-related event (mostly 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 that completed primary school. Households also include about three 5-year old minors, half of which are females. Minors represent indeed the majority of household members and almost every household (93%) has at least a minor in the composition. These minors are mainly the biological children of the respondent. However, the presence of other minors is also frequent enough and 41% 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% level. Therefore, the sample is suitable also for an analysis at the household level.

IV. Methods and Results

IV.A. Identification Strategy

My estimation is based on the following regression:

$$(1) \quad Y_{h \text{ POST}} = C + \beta T_h + \delta X_h + \phi + \epsilon_{h \text{ POST}}$$

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, 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 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 reasons⁵, 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 (IV) 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. Thus, I cap all currency-denominated variables at the 99th 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

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 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).

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.

IV.B. Impacts on Household Expenditures

Table 2 displays the intent-to-treat estimates of the cash grant on monthly household consumption. After four years, the program significantly increased household per capita consumption by more than UGX 3,000 or about \$ 2 (a 12% 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% compared to control females, while assigned males increased it by 11% 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 more than UGX 23,000 (\$ 13 or again 12%). The result is confirmed also when using the log variable in place of the level indicator.

Similarly, food consumption in the treatment group significantly rose by 10% (UGX 14,660 or about \$ 8) and non-food consumption relevantly grew by 18% (UGX 8,400 or \$ 5) compared to the control group. The decomposing of consumption in food and non-food expenditures shows interesting gender differences. While women assigned to treatment spent about \$ 10 more (a 13% relative increase) on food consumption, they only spend \$ 3-4 more on non-food consumption. On the contrary, men in the treatment group increased food consumption by 8% and non-food consumption by 20% 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 by 11-15% (UGX 29,000-40,000 or \$ 17-23) in either 2010 or 2012 (Table 3). The intervention

⁷ The results do not depend on this choice though.

also caused a significant growth in medium-term health expenditures by 23% (about UGX 7,000 or \$ 4), but the effect is close to zero after four years. The results are confirmed by the illustration of the relative log distributions (Figure 1). 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.

Passing to the gender-differentiated impacts, in 2010, males assigned to receive the grant increased total educational expenses by 21% relative to control males (the effect is significant when looking at the log results). In 2012, their educational expenditures increased even more by a significant 24%, 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 \$ 32 both in the medium and long run and health expenditures raised only temporarily by about \$ 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 both in the medium and longer horizon (Table 4). After four years, males assigned to receive the intervention also increased their educational expenses for non-family members by about \$ 6 as compared to control males. This finding might represent the fact that as their income rises relative to the community average (see treatment effect on monthly earnings reported in B.F.M., 2014) they receive more requests for concrete help from neighbors or other village members and they are 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 \$ 6 more for the education of non-family members, they still spent \$ 20 more for the education of their children and family members (relative to control males).

On the other hand, in 2010, educational and health expenditures for non-family members increased by 90-95% among females in the treatment group relative to females in the control group. This result suggests that females, especially short after receiving the grant, were more affected by money requests by external individuals. Rather than different gender differences in spending this result might 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 which were assigned to receive the cash 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 for their children at least in part because of stronger external influences and possibly because of tighter constraints on their spending capacity.

Table 5 reproduces the treatment impact using treatment-on-the-treated estimations. As expected, the results are similar to the ITT estimates with the only difference that the magnitudes of the effects are about 2% 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), 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% compared to control males, as well as they increased expenses in clothes and shoes for male minors by 14% (while the 5% increase on female minors is not significant). At the same time, their expenses on educational material grew by 22%, whereas their medical expenses raise by merely 4%.

On the other hand, assigned females increased their expenditures for adults' clothes and shoes by 31-35% compared to control females, but they did not substantially increase expenditures for minors (the treatment effect is positive and equal to an 11% increase for clothes and shoes for females and to a 5% increase for clothes and shoes for males, but it is not statistically significant). Contemporarily, their medical expenses relevantly increased by 23%, while their expenses on educational materials increased by only 10%. 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.

IV.C. Impact Heterogeneity

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

$$(2) \quad Y_{h \text{ POST}} = C + \beta T_h + \gamma T_h \times \text{TRAIT}_h + \eta \text{TRAIT}_h + \delta X_h + \phi + \epsilon_{h \text{ POST}}$$

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.

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 outcome variable, I focus only on educational expenditures since they show a more consistent post-intervention increase than health expenditures and they might offer more useful insights into the differentiated effects of the program.

Table 7.A. illustrates the heterogeneous results for the whole sample. As expected, individuals in the treatment group with higher baseline wealth and with more foster children in the household had higher total educational expenses (by about \$ 35 and \$ 44 respectively after four years, while the effect for the full sample was of only \$ 17). Interestingly, also people that witnessed violence raised educational expenses (by about \$ 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 spend more on non-family members and less on their-selves. Surprisingly, there is no heterogeneity based on the proportion of female household minors, whereas the treatment impact is heterogeneous based on the average age of the minors. However, the effect of age is unclear because 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) only 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 7.B. shows the relative heterogeneous effects of the program on females.

Females in the treatment group with higher education (at least secondary) spent significantly more for education, especially in the shorter run and for their children and family members (in 2010 they spent about \$ 68 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 \$ 55 less in 2010 for their children and family members and \$ 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 (\$ 6 after two years – versus \$

4 for the full female sample – and \$ 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 influences. In 2010, they spent about \$ 20 for educational expenses of non-family members, while they spent significantly less on their own educational expenses.

IV.C. 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 was not designed to reply to such question and 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). This measure is based on the self-reported answers to the questions 1) “how many children of school age do you have?” and 2) “how many of these children are attending school?” that were asked in the 2010 questionnaire. This indicator might not be a particularly meaningful outcome in such a context since Uganda abolished primary school fees in 1997, enrollment rates are almost universal, and attendance rates are already high enough. In fact, in spite of the higher educational expenses, the cash 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 parent’s education. This result is not surprising and it is in line with previous literature on the subject. For example, Karlan and Linden (2013) study the effect of a school-based commitment savings device for educational expenses in Uganda. More specifically, they compare 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 does not, however, affect attendance nor enrollment. Nonetheless, it does indeed translate in better educational outcomes for children as it increases scores on an exam covering language and math skills by 0.14 standard deviations.

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 probably 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% 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. Individuals that were under 21 in 2008 and were selected into treatment returned to school 54% more frequently than control individuals in the same age group.

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 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% higher than the control group. In particular, assigned males placed their children 8% higher than control males. Contrarily, there appears to be no effect on self-assessed children health. Referring to a 1 to 9 scale where on the bottom stand the people who have the least access to basic services (such as health and education) in the community, individuals assigned to receive the grant placed their families 11% higher relative to the control. These findings seem to suggest that the intervention increased not only educational expenditures, but also subjective education-related outcomes.

V. Discussion and Conclusions

There are different reasons that could explain the effectiveness of the YOP program in fostering household investments in education. Educational expenses might be a luxury item that people can afford only when released from credit constraints. Otherwise, it might 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, Garrido, and Prina (2012) show that the Mexican antipoverty program PROGRESA raised the educational aspirations of beneficiary parents for their children of a third of a school year through exposure to educated professionals. Specifically, educational aspirations for children from high-exposure households were almost half of a school year higher, six months after the start of the program. It might also be that the facilitator actively helped in boosting educational expenditures by suggesting a wise investment strategy.

I explore the role of these different group-specific program characteristics to try to shed light on the possible channels of the treatment impact on consumption. Table 10 suggests that the results are not

driven by an income effect because the size of 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.

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Table 1. Sample characteristics and balance test

Covariate in 2008 (baseline)	Treatment		Control		(5) Difference	(6) p-value
	(1) Mean	(2) SD	(3) Mean	(4) SD		
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 an OLS regression of baseline characteristics on an 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)	(2)	(3)	(4)	(5)	(6)	(7)
	HH consump- tion per capita	HH consump- tion	Ln(HH consump- tion)	HH food consump- tion	Ln(HH food consump- tion)	HH non- food consump- tion	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 intent-to-treat 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). 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 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

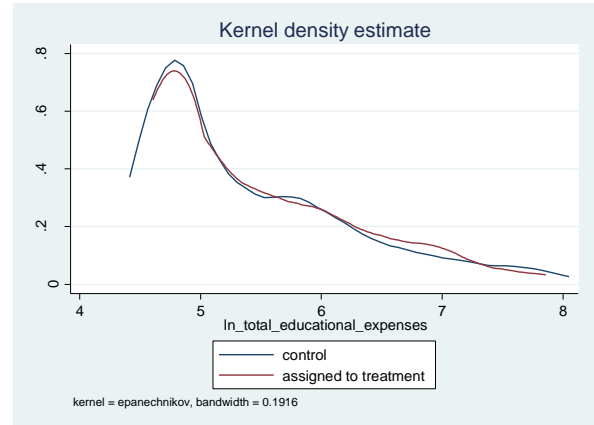
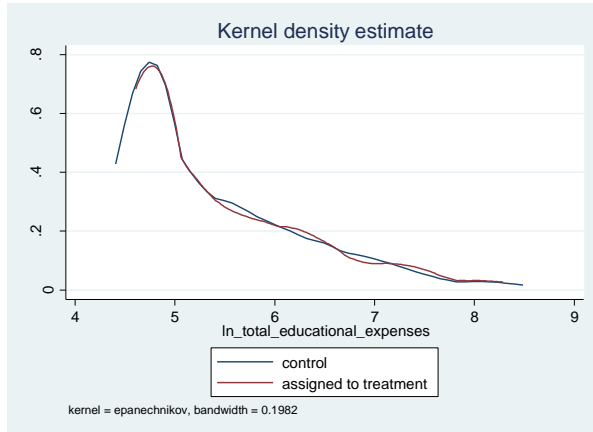
Table 3. Intent-to-treat estimates of program impact on household educational and health expenditures

	(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 intent-to-treat 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, 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 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

Figure 1. Kernel densities of educational and health expenditures

a. Educational expenditures



b. Health expenditures

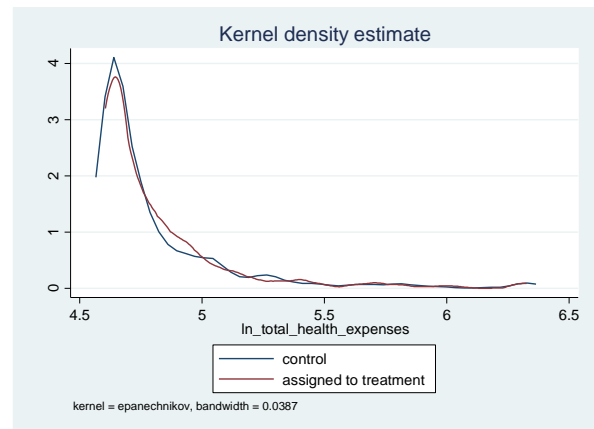
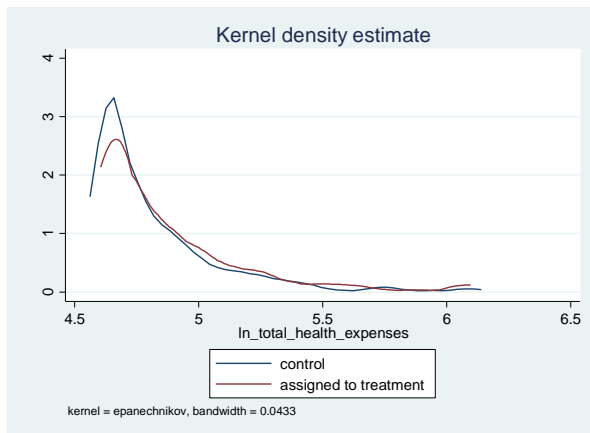


Table 4. Intent-to-treat estimates of program impact on educational and health expenditures disaggregated in expenses for children and family members, expenses for non-family members, and own expenses

	(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 intent-to-treat 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, 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 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. 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 consumption	HH non food consumption	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)
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)
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)
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 6. Intent-to-treat estimates of program impact on other types of household expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
	Clothes/ shoes expenses for males over 16	Clothes/ shoes expenses for females over 16	Clothes/ shoes expenses for male minors under 16	Clothes/ shoes expenses for female minors under 16	Expenses for educational material	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 intent-to-treat 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, 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 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.A. 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								
Assigned to treatment (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								
Assigned to treatment (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								
Assigned to treatment (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								
Assigned to treatment (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								
Assigned to treatment (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 (4) report coefficients from a weighted least squares regression of the dependent variable on the listed independent variables plus the listed interaction variable, 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 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.B. 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)								
Assigned to treatment (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								
Assigned to treatment (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								
Assigned to treatment (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								
Assigned to treatment (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								
Assigned to treatment (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 (4) report coefficients from a weighted least squares regression of the dependent variable on the listed independent variables plus the listed interaction variable, 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 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 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 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 intent-to-treat 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 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 consumption	HH non food consumption	Total educational expenses	Educational expenses for children and family members	Total health expenses	Health expenses for children and family members
Treatment group only	2012	2012	2010, 2012	2010, 2012	2010, 2012	2010, 2012
Grant size per person (z-score)	0.084* (0.051)	-0.019 (0.042)	0.053 (0.044)	-0.03 (0.034)	0.024 (0.045)	0.051 (0.046)
Observations	810	810	1,676	1,676	1,676	1,676
Facilitator/M&E advisor provided additional support (z-score)	0.018 (0.05)	0.081 (0.057)	0.105*** (0.039)	0.054 (0.033)	-0.009 (0.035)	-0.022 (0.04)
Observations	554	554	1,276	1,276	1,276	1,276
Facilitator monitored group performance (z-score)	0.041 (0.048)	0.106* (0.058)	0.064 (0.043)	0.022 (0.035)	0.024 (0.034)	-0.001 (0.04)
Observations	550	550	1,268	1,268	1,268	1,268
Facilitator provided business advice (z-score)	0.007 (0.049)	0.066 (0.057)	0.125*** (0.042)	0.05 (0.033)	-0.023 (0.03)	-0.008 (0.038)
Observations	550	550	1,268	1,268	1,268	1,268
Facilitator provided advice on profit sharing/ spending (z-score)	0.049 (0.047)	0.098* (0.058)	0.083** (0.04)	0.053* (0.031)	0.006 (0.033)	-0.012 (0.036)
Observations	550	550	1,268	1,268	1,268	1,268
Numbers of months during which the facilitator supported the group (z-score)	-0.028 (0.038)	0.009 (0.029)	0.042** (0.02)	0.042** (0.021)	0.025 (0.035)	0.008 (0.031)
Observations	551	551	1,267	1,267	1,267	1,267
Performance of the facilitator (z-score)	0.023 (0.049)	0.106* (0.059)	0.085** (0.037)	0.042 (0.031)	0.018 (0.034)	0.011 (0.038)
Observations	549	549	1,264	1,264	1,264	1,264
Facilitator provided additional support and/or continued to work with group (z-score)	0.043 (0.047)	0.092* (0.051)	0.055* (0.032)	0.04 (0.031)	0.052* (0.031)	0.038 (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 99th percentile to contain outliers and deflated to 2008 values. *** p<0.01, * p<0.1