GENERATING SKILLED SELF-EMPLOYMENT IN DEVELOPING COUNTRIES: EXPERIMENTAL EVIDENCE FROM UGANDA*

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We study a government program in Uganda designed to help the poor and unemployed become self-employed artisans, increase incomes, and thus promote social stability. Young adults in Uganda's conflict-affected north were invited to form groups and submit grant proposals for vocational training and business start-up. Funding was randomly assigned among screened and eligible groups. Treatment groups received unsupervised grants of $382 per member. Grant recipients invest some in skills training but most in tools and materials. After four years, half practice a skilled trade. Relative to the control group, the program increases business assets by 57%, work hours by 17%, and earnings by 38%. Many also formalize their enterprises and hire labor. We see no effect, however, on social cohesion, antisocial behavior, or protest. Effects are similar by gender but are qualitatively different for women because they begin poorer (meaning the impact is larger relative to their starting point) and because women's work and earnings stagnate without the program but take off with it. The patterns we observe are consistent with credit constraints. *JEL Codes:* J24, O12, D13, C93.

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I. Introduction

A third of the world's population is aged 16 to 35 and lives in a less developed country.[1] A large number are unemployed or, more often, underemployed in that they have fewer hours of work than they would like at prevailing wages (Behrman 1999; World Bank 2012). Besides the obvious effects on poverty, the conventional wisdom holds that large young and unemployed populations increase rates of crime and social instability.[2] As a result, tackling unemployment is among the highest priorities in developing countries (World Bank 2012).

This article evaluates the Youth Opportunities Program (YOP), a government program in northern Uganda designed to help poor and unemployed adults become self-employed artisans. The government invited young adults to form groups and prepare proposals for how they would use a grant to train in and start independent trades. Funding was randomly assigned among 535 screened, eligible applicant groups. Successful proposals received one-time unsupervised grants worth $7,500 on average—about $382 per group member, roughly their average annual income.

YOP's 17 eligible districts were recovering from two decades of civil strife. The government's aims were to expand skilled employment, lower poverty, and reduce the risk of social unrest (Government of Uganda 2007). Applicants were young, rural farmers who on average had reached eighth grade, earned less than $1 a day, and worked less than 12 hours a week.

Cash is a controversial intervention, in part because of concerns that the poor misuse it. Banerjee (2007) laments, "it is an item of faith in the development community that no one should be giving away money." One reasonably worries that giving $7,500 to a group of inexperienced and low-skilled 25-year-olds will come to naught. At the same time, young people have their lives ahead of them and the most to gain from investment. What they will do is uncertain.

1. Authors' calculations using U.S. Census Bureau 2012 international population data for United Nations-designated less developed countries: http://www.census.gov/ipc/www/idb/worldpop.php.

2. Although the evidence is limited, a large literature assumes that poor, unemployed young men weaken social bonds, reduce civic engagement, and heighten the risk of unrest (e.g., Becker 1968; Collier and Hoeffler 1998; Goldstone 2002; World Bank 2007, 2010, 2012; Blattman and Miguel 2010).

The effects of YOP are impressively large, however. The program led to substantial and persistent increases in investment, work, and income. We surveyed the treatment and control groups two and four years after disbursement. Groups invest grants in skills training but most of all in tools and materials. After four years, groups assigned to grants were more than twice as likely to practice a skilled trade—typically a self-employed artisan in carpentry, metalworking, tailoring, or hairstyling. After four years the treatment group had 57% greater capital stocks, 38% higher earnings, and 17% more hours of work than did the control group. Treatment group members also became more "firm-like" in that they were 40–50% more likely to keep records, register their business, and pay taxes. They also used significantly more unpaid family labor in agriculture and, for every four people treated, a part-time employee was hired and paid.

A third of the applicants were women and the program had large and sustained effects on them as well. After four years, incomes of treatment women were 73% greater than control women, compared to a 29% gain for men. Over the four years, control men kept pace or caught up with treatment men. Women stagnated without the program but took off when funded. Previous studies from South Asia have shown limited effects of cash on unemployed women (Field, Jayachandran, and Pande 2010; de Mel, McKenzie, and Woodruff 2012). It is possible that in some places social constraints limit the efficient scale of female entrepreneurs. Our study suggests there are some places these social constraints do not bind so firmly.

In spite of large economic gains, however, we see little noneconomic impact at the individual level. There was little to no effect on our measures of individual community integration, local and national collective action, antisocial behavior, or violent protest. Blattman, Emeriau, and Fiala (2013) examine political impacts and find little change in support for the government.

These results complement research that finds high returns to capital in established firms and farms, especially among men (de Mel, McKenzie, and Woodruff 2008; Fafchamps et al. 2011; Udry and Anagol 2006). These experiments estimate the returns to capital on the intensive margin. This article adds to our understanding of employment growth on the extensive margin, particularly the transition from agriculture to cottage industry.

There are some caveats. First, despite randomization the control group began slightly wealthier than the treatment group. Second, unemployed rural youth are mobile, and 18% of the sample could not be found after four years, despite extensive tracking. Attrition is higher in the control group. Treatment effects, however, are generally robust to the inclusion or exclusion of baseline covariates, to difference-in-difference estimates, and to conservative missing data scenarios.

A third limitation is that we are unable to evaluate the non-cash components of YOP separately. We speculate that the group and business plan may have been important as initial commitment devices, though the sustained investment and earnings growth we see over four years suggests that such restrictions were not vital to long-term success. Alternatively, these restrictions may have helped screen out applicants uninterested in becoming artisanal entrepreneurs.

To help understand why YOP had such large economic effects (and to assess generalizability), we consider a simple model of investment. If financial markets function well, people should produce at their efficient scale and will consume and save a grant. A "restricted" grant that compels investment will be divested over time and, in the meantime, returns will be below market interest rates. To expect sustained investment and high returns, the program must relieve some constraint keeping people below efficient scale. One possibility is a social or behavioral constraint that limits new business start-up or expansion but does not lead people to divest afterward. A more standard explanation is credit constraints—if unable to borrow, people who are poor and able will be below efficient scale. Either way, a grant will be invested and earn returns higher than market interest rates. Labor supply can also increase among the underemployed.

Our evidence is consistent with imperfect credit being a key constraint on the young and unemployed: our sample begins highly constrained; returns to the grant are high (especially among the most credit constrained), investment is sustained, and the control group saves and accumulates capital in enterprise rapidly, but only in sectors with low fixed costs. Nonstandard social and behavioral constraints could augment the effect of credit constraints.

The results from this YOP evaluation contrast with other efforts to create jobs in developing countries. Evaluations of vocational training and internship program report positive results, but seldom for men.[3] One difference is that YOP provides funding for business assets and start-up in an environment where there are few firms. Governments also invest large sums trying to create jobs and raise earning capacities through microfinance, "ultra-poor" asset transfers, and conditional cash transfers (CCTs). Although designed to help the poor cope with shocks or pay for education and health, it is also hoped that these programs will stimulate new enterprise (Fizbein, Schady, and Ferreira 2009; Karlan and Morduch 2009; IPA 2013). These programs have successfully reduced risk and poverty, but so far show little effect on employment or earning capacities.[4]

There are exceptions, and our evidence is consistent with three program evaluations in Asia and Latin America: Gertler, Martinez and Rubio (2012) and Bianchi and Bobba (2013) show that a Mexican CCT program stimulates self-employment; Macours, Premand, and Vakis (2012) show that a grant raises nonfarm earnings in Nicaragua; and Bandiera et al. (2013) show that livestock transfers in Bangladesh shift occupations from farm labor to rearing one's own livestock. Our study contributes to this evidence by its size and length, by providing some of the first evidence from Africa, by a focus on the shift from agriculture into skilled artisanal work, the attention to formalization and multiplier effects on employment, and the downstream impacts on stability after conflict.

More generally, our results are consistent with a body of observational micro-level evidence that suggests that financial market imperfections are widespread and can account for the

3. In Colombia there was no effect on men, but women's work and wages rose (Attanasio, Kugler, and Meghir 2011). In the Dominican Republic there was no effect on men (Card et al. 2007). In Malawi there were small effects on men but none on women (Cho et al. 2013). In Uganda, girls' self-employment rose but earnings did not (Bandiera et al. 2012). In India there were modest impacts on women's work and earnings (Maitra and Mani 2012).

4. Several experiments show microfinance raises farm investment but has little effect on new enterprise or earnings (Attanasio et al. 2011; Crépon et al. 2011; Angelucci, Karlan, and Zinman 2012; Augsburg et al. 2012; Banerjee et al. 2013). Ultra-poor programs that provide allowances, livestock, and training appear to raise consumption and food security but not employment and incomes (Banerjee et al. 2010; IPA 2013). One exception is a Bangladeshi study by Bandiera et al. (2013). Studies of CCT programs often ignore enterprise growth (Fizbein, Schady, and Ferreira 2009), but two Nicaraguan experiments find no effect on earnings and nonfarm production (Maluccio 2010; Macours, Premand, and Vakis 2012).

fact that many of the poor have high returns to capital (Banerjee and Duflo 2011). They also echo classic macro-level theories of development that emphasize how credit constraints hold back long-run growth and structural change (Banerjee and Newman 1993; Galor and Zeira 1993; King and Levine 1993; Aghion and Bolton 1997; Piketty 1997). We also see results consistent with canonical models of surplus labor, in that increasing nonagricultural production and labor supply does not diminish output or inputs into agriculture (Lewis 1954; Ranis and Fei 1961)

Overall this evidence increases confidence that cash can be used to reduce unemployment and poverty. The evidence and conceptual framework guide where cash could have the largest effect on new employment in future: by targeting poor young adults with ability and initiative, especially where local economies are below steady state, credit is scarce, and social norms do not stifle new enterprise. There are limits on generalizability and scale, but the YOP model merits more experimentation. It will be important to explore the mechanisms that make the YOP model successful, especially the costs and benefits of labeling, framing, group commitment, and other restrictions. This remains the most important gap in existing evidence.

II. Description of the Intervention and Experiment

II.A. Setting: Northern Uganda

Uganda is a small, poor, growing country in East Africa. Shortly before the program, in 2007, it had a population of about 30 million and gross domestic product (GDP) per capita of roughly \$330. The economy has been stable and growing, with real GDP at market prices rising 6.5% a year from 1990 to 2007, inflation under 5%, and falling rates of poverty (Government of Uganda 2007). This growth puts Uganda's GDP per capita slightly above the sub-Saharan average.

This growth, however, was concentrated in south-central Uganda. Subsistence agriculture, cattle herding, and some commercial agriculture have historically dominated the north, home to a third of the population. The north is more distant from trade routes and, as a bed of opposition support, received less public investment from the 1980s onward, especially for power and roads. Growth and structural change in the north were also held back by insecurity. From 1987 to 2006 a low-level insurgency destabilized north-central Uganda, and wars in Sudan and Democratic Republic of Congo fostered mild insecurity in the northwest. Cattle rustling and armed banditry were common in the northeast.

As a result, in 2006 the government estimated that nearly two-thirds of northern people were unable to meet basic needs, just over half were literate, and most were underemployed in subsistence agriculture (Government of Uganda 2007). Also, like much of rural Africa, the average person has almost no access to formal finance. Formal insurance was unknown, and almost no formal lenders were present in the north at the outset of this study in 2008. Although village savings and loan groups are common, loan terms seldom extend beyond three months, with annual interest rates of 100% to 200%. Because of high fees, real interest rates on savings are negative.

By 2008, however, the north's economy was growing. In 2003, peace came to Uganda's neighbors and Uganda's government increased efforts to pacify, control, and develop the north. By 2006, the military pushed the rebels out of the country and began to disarm cattle-raiders. The government also began to improve northern infrastructure. Neighboring countries, especially south Sudan, began to grow rapidly. With this political uncertainty resolved, and growth in linked markets, the northern economy began to catch up.

From 2003 to 2010, the centerpiece of the government's northern development and security strategy was a decentralized development program, the Northern Uganda Social Action Fund, or NUSAF (Government of Uganda 2007). NUSAF was Uganda's second-largest development program. Starting in 2003, communities and groups could apply for cash grants for either community infrastructure construction or livestock for the "ultra-poor". The government wanted to do more to boost nonagricultural employment. To do so, in 2006 it announced a third NUSAF component: the YOP.

II.B. Intervention: The YOP

YOP invited groups of young adults, aged roughly 16 to 35, to apply for cash grants to start a skilled trade, such as carpentry or tailoring. The program had four key elements. First, people had to apply as a group. One reason was administrative convenience: it was easier to verify and disburse to a few hundred groups than to thousands of people. Another reason is that in the absence of formal monitoring, officials hoped groups would be more likely to implement proposals. Groups in our sample ranged from 10 to 40 people, averaging 22. They are mostly from the same village and typically represent less than 1% of the local population. Half the groups existed already, often for several years, as farm cooperatives, or sports, drama, or microfinance clubs. New groups formed specifically for YOP were often initiated by a respected community member (e.g., teachers, local leaders, or existing tradespersons) and sought members through social networks. In our sample, 5% of groups are all female and 12% are all male, but most groups are mixed—about one-third female on average.

Second, groups had to submit a written proposal stating how they would use the grant for nonagricultural skills training and enterprise start-up costs. They could request up to about \$10,000. The proposal specified member names, a management committee of five, the proposed trade(s), and the assets to purchase. Decisions were made by member vote, and nearly all members report they had a voice in decisions.[5] Most groups proposed a single trade for all, but a third of the groups proposed that different members would train two to three different trades. Women and mixed groups often chose trades common to both genders, such as tailoring or hairstyling. Men and a small number of women often chose trades such as carpentry or welding.

In preparing the proposal, groups selected their own trainers, typically a local artisan or small institute. These are common in Uganda (as in much of Africa), and there is a tradition of artisans taking on paying students as apprentices. Most of these artisans and institutes had been in existence more than five years, and most took students previously. In our sample, few were located in the village but the median artisan or institute was within 8 km. Groups would travel to be closer to trainers or paid transport and upkeep for trainers to come to them. Thus groups were seldom constrained in their choice of vocation by local trainers. This group-based training generally produced bulk discounts and enabled a wider choice of vocations.

5. According to our qualitative interviews, groups often acted on advice of experienced advisers, especially if that person was a group organizer. They were most influenced by the marketability and profitability of a trade.

Many applicants were functionally illiterate, so YOP also required facilitators—usually a local government employee, teacher, or community leader—to meet with the group several times before proposal submission, advise them on program rules, and help prepare the written proposals. Groups chose their own facilitators, and facilitators received 2% of funded proposals (up to $200).

A third feature of the program is government screening. Villages typically submitted one application, and that privilege may have gone to the groups with the most initiative, need, or connections. Village officials passed applications up to districts, which verified the minimum technical criteria (such as group size and a complete proposal) and were supposed to visit projects they planned to fund. Districts said they prioritized early applications and disqualified incomplete ones, but unobserved quality and political calculation could have played a role.

Finally, successful proposals received a large lump-sum cash transfer to a bank account in the names of the management committee, with no government monitoring thereafter. Our impression is that the absence of formal government monitoring was generally understood. In our sample, the average grant was 12.9 million Ugandan shillings (UGX) per group, or $7,497 (all figures in the paper are quoted in 2008 UGX and U.S. dollars). Per capita grant size varied across groups due to variation in group size and amounts requested. Eighty percent of grants were between $200 and $600 per capita, averaging $382.[6] Figure I reports group size and per capita grant distributions.

II.C. Experimental Design

YOP was oversubscribed, and we worked with the government to randomize funding among screened and eligible proposals. Thousands of groups submitted proposals in 2006 and the government funded hundreds in 2006–7, prior to our study. By 2008, 14 NUSAF-eligible districts had funds remaining.

6. This figure divides funds received by estimated 2008 group size. Funds received can be lower than funds requested because a small number of groups: (i) did not receive a transfer for administrative reasons, or (ii) had funds diverted before arrival (see Section VI). Group size differs from the proposal because composition changed between application in 2006 and the baseline in 2008. We calculate group size using the 2008 group roster, adjusted by endline reports of baseline members excluded from the grant.

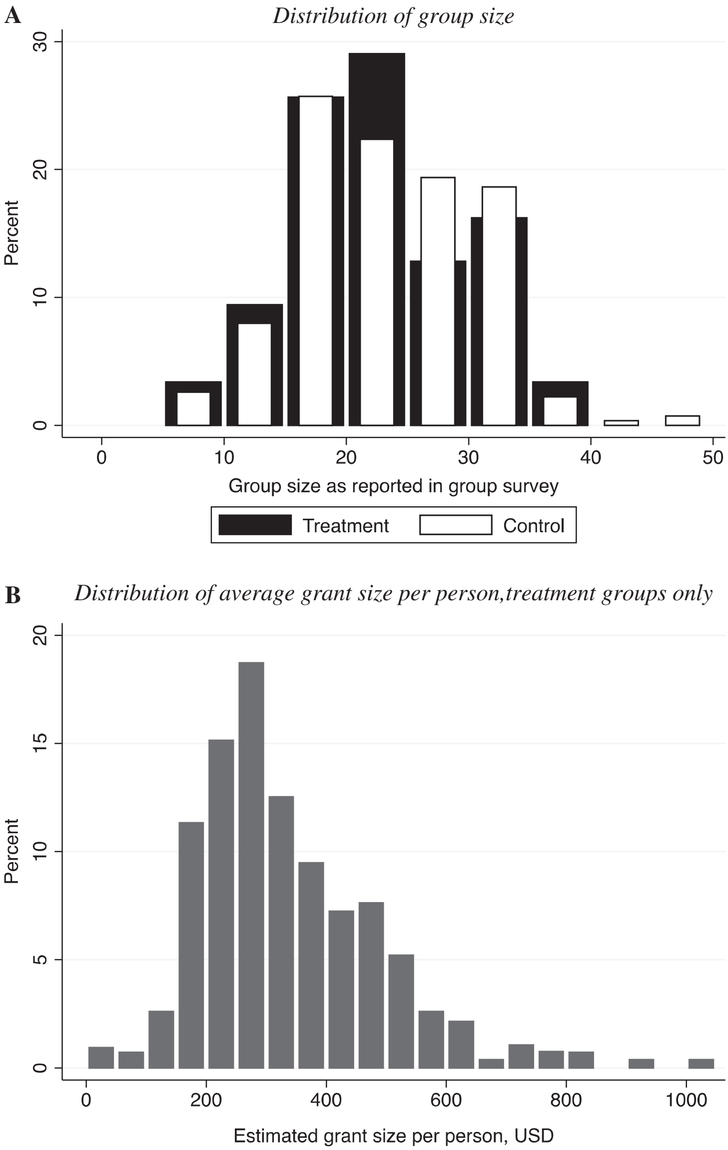

FIGURE I

Group and Grant Size

A. Distribution of group size

B. Distribution of average grant size per person, treatment groups only

Estimated grant size does not include funds reported as not transferred or diverted by district officials. UGX-denominated grants are converted to dollars at the 2008 market exchange rate of 1,915 UGX per USD. The bin widths are (A) five years and (B) $50.

Figure II maps these study districts.[7] None of the most war-affected districts (Gulu, Kitgum, and Pader) had the funds to participate in the final round.

In 2007 the central government asked district governments to nominate 2.5 times the number of groups they could fund. The districts submitted roughly 625 proposals to a central government office that reviewed them for completeness and validity. To minimize chances of corruption, the central government also sent out audit teams to visit and verify each group. The

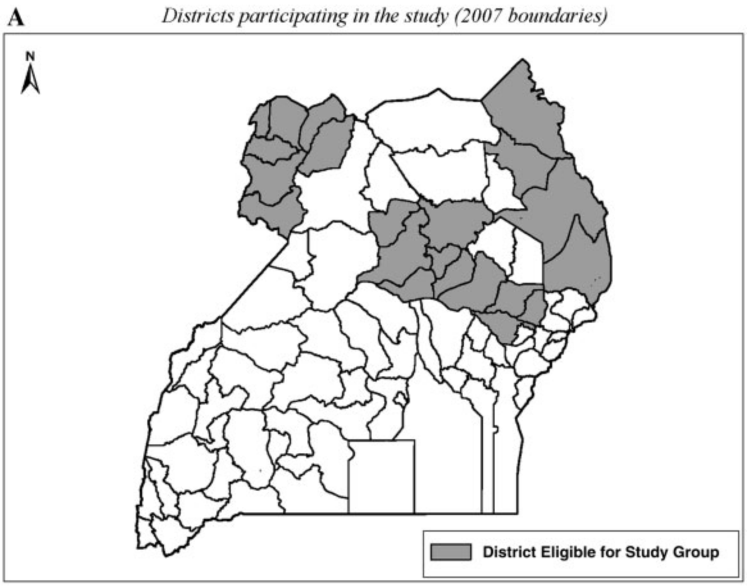

FIGURE II

Location of Study Communities

A. Districts participating in the study (2007 boundaries)

B. Number of study communities (treatment and control) per parish

Panel A displays 2006 political boundaries (subdivided since 2003), with further subdivisions after 2006 marked by a white border line. In Panel B, gaps in administrative data mean that 20 villages are linked to a district but not a parish. Of the 26 parishes with three or more applicant groups per parish, just 6 parishes have 4+ groups.

7. By 2008, a national program of decentralization had subdivided these 14 districts into 22, as depicted in the map, but YOP was organized, disbursed, and randomized using the original 14 districts from 2003.

B *Number of study communities (treatment and control) per parish*

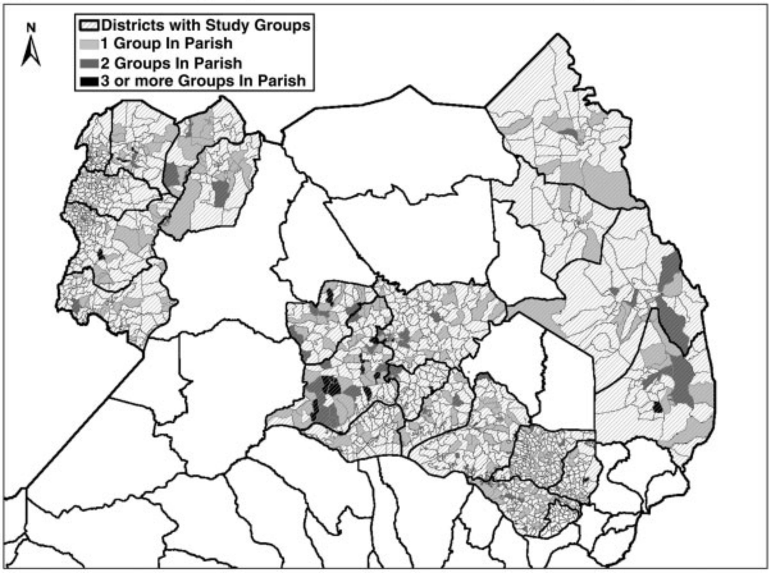

FIGURE II

Continued

government disqualified about 70 applications, mainly for incomplete information or ineligibility (e.g., many group members over age 35, or a group size more than 40). The government also asked that 22 groups of underserved people (Muslims and orphans) be funded automatically.

In January 2008 the government provided us with a list of 535 remaining groups eligible for randomization, along with district budgets. We randomly assigned 265 of the 535 groups (5,460 individuals) to treatment and 270 groups (5,828 individuals) to control, stratified by district.[8] Spillovers between study villages are unlikely as the 535 groups were spread across 454 communities in a population of more than 5 million, and control groups are typically very distant from treatment villages. See Figure II for a map of groups per parish.

8. Control groups were not formally waitlisted for the program, though officials privately expressed an interest in funding them in the future. During the baseline survey, before treatment status was known, groups were told they had a 50% chance of funding and that there were no plans to extend the YOP program in the future.

III. Data and Description of the Sample

III.A. Data and Randomization Balance

The 535 groups contained nearly 12,000 members. We survey 5 people per group three times over four years—a panel of 2,677 (7 were inadvertently surveyed in one group). We also conducted informal qualitative interviews in 2007 with 10 YOP groups funded previously, plus formal interviews in 2010 with 30 people from 10 randomly selected groups in three districts.[9] Table I reports survey response rates and sample size at each round.

We ran a baseline survey in February and March 2008, prior to funding treatment groups. Enumerators and local officials mobilized group members to complete a survey of demographic data on all members as well as group characteristics. Virtually all members were mobilized, and we randomly selected five of the members present to be individually surveyed and tracked.[10] Enumerators could not locate 13 groups (3% of the sample). Unusually, after the survey it was discovered that all 13 were assigned to the control group. We investigated the matter and found no motive for or evidence of foul play. District officials, enumerators, and the groups themselves did not know the treatment status of the groups they were mobilizing. We were only able to find 1 of the 13 at endline.

The government disbursed funds between July and September 2008. We conducted the first two-year endline survey between August 2010 and March 2011, 24–30 months after disbursement, and a four-year survey between April and June 2012, 44–47 months after disbursement.

YOP applicants are a young, mobile population. Nearly 40% had moved or were away temporarily at each endline survey. To minimize attrition we used a two-phase tracking approach (Thomas, Frankenberg, and Smith 2001). Table I summarizes.

9. The districts were Teso, Lango, and West Nile. Three local qualitative interviewers were recruited and trained on how to conduct individual interviews and focus group discussions using a question guide, in the local language, and were audio-recorded, translated to English, and transcribed. Transcripts were read and informally analyzed by the authors plus the local project coordinator who trained and supervised the qualitative interviewers.

10. Members were mixed up then lined up, and enumerators selected every $N/5$ person to survey (where N is the total number present). Four percent of the groups had missing members, and these missing members were not included in the baseline survey sampling.

TABLE I

SURVEY RESPONSE RATES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Selection and tracking, by survey phase					Effective response rates				
Survey	Total sought	Found, phase 1	Selected, phase 2	Found, phase 2	Final # of observations	All	Control	Treatment	Difference	p-value
2008 baseline	2,677	97.0%	—	—	2,598	97.0%	94.4%	99.8%	5.3%	<.001
2010 endline	2,677	63.4%	53.0%	74.7%	2,005	85.4%	85.6%	85.3%	−0.8%	.717
2012 endline	2,677	61.0%	38.5%	58.6%	1,868	82.1%	79.1%	85.5%	7.1%	.004

Notes. Column (1) reports the full study sample sought in each round—in general, five people per group over 535 groups, save for 1 group where baseline data on seven individuals was accidentally collected. Column (2) reports the percentage of these found in a first survey phase, where each respondent was sought at least once in the town they lived at baseline. Each endline had a second survey phase that tracked a random sample of migrants and other unfound individuals, and column (3) reports average percentage randomly selected. This percentage varied exogenously by stratum according to the proportion missing and expense of tracking in that district. Column (4) reports the percentage of those sought in phase 2 successfully surveyed. Column (5) reports the final number of observations by survey round. Columns (6)–(9) report the corresponding response rates overall, by treatment status, and the treatment-control difference (calculated via regression, controlling for baseline district). Columns (6)–(9) are weighted by the inverse probability of selection in phase 2 of the survey (which varies by strata, with weights ranging from 1 to 4), and are referred to as “effective” response rates. Unfound respondents randomly dropped in phase 2 receive zero weight. Column (10) reports p-value on the difference term, using robust standard errors clustered at the group level.

In Phase 1, we attempted to interview all 2,677 people in their last known location. Thirty-seven percent were not found in 2010 and 39% in 2012, almost all migrants. In Phase 2, we selected a random sample of the unfound—53% in 2010 and 38.5% in 2012, stratifying by district and by the proportion unfound in the group. We made three attempts to find this subset in their new locations. We found 75% in 2010 and 59% in 2012. Those found in Phase 1 receive unit weight, those selected in Phase 2 are weighted by the inverse of their selection probability, and those not selected in Phase 2 are dropped. We have no reports of survey refusal, and no reward was offered for survey completion.

Our response rate was 97% at baseline, and effective response rates at endline (weighted for selection into endline tracking) were 85% after two years and 82% after four (see Table I). Overall our attrition levels are similar to other panels of young adults in rural Africa (e.g., Baird et al. 2011; Friedman et al. 2011), though higher than some panels of existing entrepreneurs, who are typically urban, less mobile, and in some cases screened for attrition before the experiment (Udry and Anagol 2006; Fafchamps et al. 2011; de Mel, McKenzie, and Woodruff 2012).

Of greater concern is correlation between attrition and treatment, as in Table I. The treatment group was 5 percentage points more likely to be found at baseline in 2008. There is no treatment-control imbalance in 2010, although controls are more likely to have been lost in 2008 and the treatment group in 2010. In 2012, controls were 7 percentage points less likely to be found. If unfound controls are particularly successful, we could overstate the impact of the intervention. Such bias is conceivable: baseline covariates are significantly correlated with attrition and the unfound tend to be younger, poorer, less literate farmers from larger communities (Online Appendix B.1). Our conceptual framework that follows suggests that impacts could be high in this group.

Table II displays summary statistics and tests of balance for 38 baseline covariates. There is balance across a wide range of measures, but a handful show imbalance—the treatment group report 2 percentage points more vocational training, 0.07 standard deviations greater wealth, 56% greater savings (though only in the linear, not in log form), and 5 percentage points more access to small loans. This imbalance may be chance. The missing 13 control groups, however, could also cause the imbalance. We estimate that if the missing controls had baseline values 0.1 to 0.2

TABLE II
PRE-INTERVENTION DESCRIPTIVE STATISTICS AND TEST OF BALANCE

	(1) Control	(2)	(3) Treatment	(4)	(5) Regression difference	(6)
Covariate in 2008 (baseline)	Mean	Std. dev.	Mean	Std. dev.	Mean	p-value
Grant amount applied for, USD	7,497	2,220	7,275	2,025	143.82	.290
Group size	22.5	6.8	21.2	7.2	0.03	.960
Grant amount per member, USD	363.1	159.4	381.7	170.9	14.09	.250
Group existed before application	0.45	0.50	0.49	0.50	0.03	.420
Group age, in years	3.8	2.0	3.8	1.9	−0.05	.800
Within-group heterogeneity (z-score)	−0.03	0.92	0.03	1.06	−0.02	.800
Quality of group dynamic (z-score)	−0.02	1.02	0.02	0.99	0.05	.530
Distance to educational facilities (km)	6.84	6.50	7.26	5.71	0.48	.350
Individual unfound at baseline	0.06	0.23	0.00	0.05	−0.05	.000
Age at baseline	24.8	5.2	25.1	5.3	0.17	.550
Female	0.35	0.48	0.32	0.47	−0.02	.380
Large town/urban area	0.23	0.42	0.20	0.40	−0.02	.610
Risk aversion index (z-score)	−0.02	1.00	−0.03	1.01	−0.01	.750
Any leadership position in group	0.28	0.45	0.29	0.45	0.00	.880
Group chair or vice-chair	0.11	0.31	0.12	0.32	0.01	.330
Weekly employment, hours	10.7	15.8	11.4	15.5	0.55	.490
All nonagricultural work	6.0	12.5	5.7	11.4	−0.45	.440
Casual labor, low skill	1.0	5.2	1.1	5.0	−0.11	.630
Petty business, low skill	2.2	7.0	2.4	6.8	0.21	.520
Skilled trades	1.8	8.4	1.5	7.8	−0.33	.400
High-skill wage labor	0.0	0.6	0.1	1.0	0.08	.020
Other nonagricultural work	0.9	4.8	0.6	3.8	−0.29	.100
All agricultural work	4.7	10.1	5.6	10.5	1.02	.040

TABLE II
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		Treatment		Regression difference	
Covariate in 2008 (baseline)	Mean	Std. dev.	Mean	Std. dev.	Mean	*p*-value
Weekly household chores, hours	9.0	17.6	8.7	16.1	0.30	.730
Zero employment hours in past month	0.48	0.50	0.42	0.49	−0.04	.180
Main occupation is nonagricultural	0.26	0.44	0.28	0.45	0.00	.920
Engaged in a skilled trade	0.08	0.27	0.08	0.28	0.00	.810
Currently in school	0.04	0.21	0.04	0.19	−0.01	.450
Highest grade reached at school	8.0	2.9	7.8	3.0	−0.07	.620
Able to read and write minimally	0.75	0.43	0.71	0.45	−0.03	.170
Received prior vocational training	0.07	0.26	0.08	0.28	0.02	.050
Digit recall test score	4.2	2.0	4.0	2.0	−0.04	.640
Index of physical disability	8.7	2.5	8.6	2.2	−0.14	.290
Durable assets (z-score)	−0.16	0.96	−0.07	1.05	0.07	.120
Savings in past 6 mos. (000s 2008 UGX)	19.3	98.2	32.9	137.1	10.89	.020
Monthly gross cash earnings (000s 2008 UGX)	62.2	129.0	67.7	135.2	6.9	.300
Can obtain 100,000 UGX ($58) loan	0.33	0.47	0.40	0.49	0.05	.010
Can obtain 1,000,000 UGX ($580) loan	0.10	0.30	0.12	0.32	0.01	.460

Notes. The grant amount applied for comes from program administrative data, available for all 535 groups. Group size, prior existence, age, and heterogeneity measures come come from a collective survey of group members at baseline. All other variables come from an indidivual-level survey administered to the full sample at baseline. Group survey data are missing for 13 control groups not found at baseline. Individual data are missing for the 65 members of these missing control groups plus 16 additional people who refused consent or otherwise failed to complete the survey. Missing group size data are taken from program administrative data, and otherwise no missing observations are imputed in this table. All USD- and Ugandan shilling (UGX)-denominated variables and all hours worked variables were top-censored at the 99th percentile to contain outliers. Columns (5) and (6) report the mean difference between the treatment and control groups, calculated using an ordinary least squares regression of baseline characteristics on an indicator for random program assignment plus fixed effects for randomization strata (districts). The standard errors in column (6) are heteroskedastic-robust and clustered at the group level.

standard deviations above the control mean, it would account for the full imbalance (Online Appendix B.2). If so, the observed control group may be poorer than the treatment group and will overstate true program impacts.

Our empirical strategy in Section V and sensitivity analysis in Section VI explicitly address the concerns that arise from imbalance and potentially selective attrition.

III.B. Participants

From Table II, we see that members of the 535 eligible groups were generally young, rural, poor, credit constrained, and underemployed. In 2008 they were 25 years on average, mainly aged 16 to 35. Less than a quarter lived in a town, and most lived in villages of 100 to 2,000 households. A quarter did not finish primary school; on average they reached eighth grade.

In 2008 the sample reported 11 hours of work a week. Half these hours were low-skill labor or petty business, and the other half was in agriculture—rudimentary subsistence and cash cropping on small rain-fed plots with little equipment or inputs. Almost half of our sample reported no employment in the past month, and only 6% were engaged in a skilled trade. Cash earnings in the past month averaged \$1 a day. Savings were \$15 on average. Only 11% reported savings. Thirty-three percent held loans, but these were small: under \$7 at the median among those who had any loans, mainly from friends and family. About 10% reported they could obtain a large loan of UGX 1,000,000 (about \$580).

Although poor by any measure, these applicants were slightly wealthier and more educated than their peers. If we compare our sample to their age group and gender of a 2008 population-based household survey, our sample has 1.7 years more education, 0.15 standard deviations more wealth, is 7.5 percentage points more urban, 5.4 percentage points more likely to be married, and has 1.6 fewer household members (see Online Appendix B.3). Given that the three most war-affected districts did not participate in the YOP evaluation, only 3% were involved in an armed group in any fashion.

In some ways this is a selective intervention and sample, in that the poorest and least educated people may have been less likely to apply and more likely to be screened out. Nonetheless, there was no educational requirement for the program, and a large number of uneducated, impoverished, and unemployed young people were eligible and applied. Based on qualitative interviews, people applied to the program not because they thought it would turn into their main occupation but to have a side profession that would raise cash to meet their household's direct needs. Agriculture—mainly subsistence but some cash cropping—was expected to remain the main activity. Initiative and affinity for skilled work was clearly important, but people were keen to apply even if they were poorly qualified or had limited interest in a vocation. Most had no other government program to apply to. As a result, the sample has wide variation in wealth, education, and experience, not terribly dissimilar from the general population.

IV. Conceptual framework

Under what conditions do we expect people to invest cash windfalls and start new, profitable enterprises? This section presents an intuitive framework, drawing on a Ramsey model of investment with occupational choice and heterogeneous individuals in Online Appendix A.

In standard models of investment, unrestricted windfalls will not be used to start or expand enterprises when financial markets function well. To see this, consider the case in which there are two sectors: traditional labor-intensive work (such as subsistence agriculture) and capital-intensive small enterprise. Both use labor as an input, and production depends on a person's innate, sector-specific abilities. The enterprise sector also uses capital (physical and human), however, and may have a fixed cost of start-up in the form of a minimum capital requirement. People vary in their initial wealth and can either consume, save, or invest their current earnings and wealth. They can also borrow and save at the market interest rate, r. In this benchmark case, people with an affinity for enterprise (whom we call "high ability") will already operate enterprises at efficient scale, borrowing to meet capital needs until marginal returns equal r. Such people will consume and save an unrestricted windfall. As a result, occupational choice only depends on innate abilities, not initial wealth.

Antipoverty programs could restrict the use of windfalls by distributing in-kind capital or making formal conditions. YOP is restrictive in the sense that framing, planning, and group decision making may force initial investments in human and physical capital. In this case, low-ability types will start inefficient enterprises and high-ability types will expand beyond efficient scale. Earnings and entrepreneurial labor will rise, but returns will be low in the sense that they are less than r. Both types will want to divest capital, slowed only by irreversibility or a "flypaper effect"—market or psychological conditions that make capital investments "sticky."

To expect investment and high returns from a windfall, it must help overcome some constraint. We focus on imperfect financial markets, but also consider time-inconsistent preferences. We discuss other nonstandard possibilities in Section VII.

First consider savings and credit constraints. Both are consistent with sustained investment of a windfall, but of the two, only credit constraints are consistent with returns that are high in the sense that they exceed r. To see this, consider the simple case where people cannot save but can borrow at some moderate r. Enterprises are the only means of savings, and so more people will invest. But these enterprises will be inefficient in the sense that the marginal returns are always less than or equal to r. The returns to cash windfalls will also be low.

Under a credit constraint, however, the poor will generally be below their efficient scale in enterprise, especially high-ability types. Their marginal and average returns to capital will exceed r. Those below efficient scale should invest a large cash windfall (restricted or unrestricted), increase the labor they supply to the enterprise, and earn high returns (greater than r). Entrepreneurs at efficient scale and low-ability types will save most of an unrestricted windfall. If restrictions force them to invest, they will earn low returns and divest as fast as possible.

Next we consider uncertainty. In general, uncertainty in a sector will reduce production below efficient scale among risk-averse individuals, unless sector risks are negatively correlated. If people exhibit constant relative risk aversion they will invest part of a windfall and earn returns greater than r. If both sectors are similarly risky, however, it's unlikely that people are so risk averse and below efficient scale that a windfall will be mainly invested and earn high average returns. Enterprise must be much more risky than traditional labor to generate a large distortion (Bianchi and Bobba 2013). As discussed in Section VII, trades and small enterprise in Uganda are not evidently riskier than subsistence agriculture and casual labor and may even be less so.

A large literature shows that people often make decisions in the interest of their present selves at the expense of their future selves (Frederick, Loewenstein, and O'Donoghue 2002). One can also imagine social pressures that resemble such time inconsistency. For example, women might have limited control over their finances, especially if windfalls are easier for others to capture than regular earnings (Fafchamps et al. 2011). In perfect financial markets, however, cash windfalls will not affect investment levels or returns. Pre-windfall levels of investment will be different from the case with no time inconsistency. But the time inconsistent will invest until the return is the same across savings and occupations and will be at optimal scale when the windfall arrives. As in the benchmark case, the windfall will simply be consumed and saved.

The time-inconsistent require some other constraint (such as missing credit markets) for a windfall to be invested and produce high returns. In this case, the effect is multiplicative: restricted windfalls will result in higher returns when people are both credit constrained and time inconsistent than when someone is credit constrained alone, at least in the short term.

The key insight is that there are many conditions in which people invest windfalls in enterprise, but of the standard imperfections, credit constraints are most consistent with a large sustained average impact on occupational choice and earnings.[11] The other constraints we discuss are not consistent with high returns by themselves but may magnify the effect of credit constraints.

Credit constraints have additional predictions. In the absence of a credit constraint, people should divest after being compelled to invest, whereas with credit constraints only low-ability types will do so. Furthermore, cash windfalls have the largest effect on the most constrained, and so under credit constraints impacts should decrease in initial wealth, increase in entrepreneurial ability, and are smaller for existing entrepreneurs above the minimum capital threshold.

In this sense the selected group in our sample may be ideal candidates. This was not accidental—the requirements to form

11. This is a statement of averages and is not to say there is no divestment when there are credit constraints. If people are ex ante uncertain of their abilities, or if they have bad luck, some who invest will eventually exit.

groups, prepare proposals, and wait a long period of time before receiving a grant were designed in part to allow patient, able people with an affinity for vocations to signal their "type." This may have been the most important function of the groups and proposal in terms of ensuring that the grants were channeled into new employment.

We have said little about employment so far. In standard settings, a windfall will shift labor from traditional to enterprise production, and total labor hours will fall due to higher wealth. Our setting, however, is one of initially low employment, where people may only work 10 or 20 hours a week. This could represent very low marginal returns to additional labor or some rationing of wage labor (it is difficult to say). In either case, it is possible for a windfall to increase entrepreneurial labor while traditional labor remains roughly constant. In our simplified setup, the potential for excess supply of labor could be captured by the curvature of the production function in the traditional sector (net of disutility of labor).

V. Empirical strategy

We are primarily interested in the average treatment effect of the program on investments in training and business assets, levels and type of employment, and incomes. Our main measure of impact is an intent-to-treat (ITT) estimate, β_{ITT}, from the weighted least squares regression:

$$Y_{ijt} = \beta_{ITT} T_{ij} + \lambda X_{ij} + \alpha_{dt} + \varepsilon_{ijt},$$

where Y denotes the outcome in year t for person i in group j; T is an indicator for assignment to treatment; X is the set of baseline covariates in Table II (using an age cubic); α are district fixed effects (required because the probability of assignment to treatment varies by strata); and ε is an individual error term clustered by group. We weight observations by their inverse probability of selection into endline tracking. We also estimate 2010 and 2012 impacts separately.

Several outcomes have a long upper tail, and some of these large values are potentially due to enumeration errors. Extreme values will be highly influential in any treatment effect, so we top-code all currency-denominated, hours worked, and employee variables at the 99th percentile.

Finally, because outcomes are self-reported, we overestimate the impact if the treatment group overreports well-being due to social desirability bias, or if the controls underreport outcomes in the hope it will increase their chance of future help. This is unlikely for two reasons. First, misreporting would have to be highly systematic: income and employment was collected through more than 100 questions across 25 activities, and assets and expenditures were calculated from more than 150 questions. Second, we would also expect to see such bias appear in the social outcomes, but (as we will see) we observe no treatment effects there. Misreporting would have to be confined to economic outcomes alone to bias our results.

VI. Results

Of the 265 groups assigned to a cash grant, 89% received it. We consider these groups "treated." The untreated include 21 groups that could not access funds due to unsatisfactory proposals, bank complications, or collection delays, plus 8 groups reporting that they never received funds due to some form of theft or diversion. A comparison of baseline characteristics shows that treated and untreated groups are generally similar, but groups were slightly more likely to be treated if they were educated and wealthier and did not have too many members (see Online Appendix B.4). These traits probably lowered the probability of a disqualifying error in the proposals.

In addition to the YOP grant, treatment group members were also more likely to report a slightly greater amount of aid from charities or other government programs. Table III reports control means and ITT estimates for the full sample (treatment means and raw differences are listed in Online Appendix B.5). In 2010, two years after the grant, treatment group members were 1.5 percentage points more likely to report a non-YOP program from the government or a charity, and 2.6 percentage points more likely by 2012. The average amount received in the first two years was UGX 61,800 ($36) higher than in the control group. In general these other programs were small in size—among those who reported other aid, controls valued it at $19 and treatment group members at $29. In terms of future transfers, both treatment and control groups had equally high expectations: 76% of both groups said it was likely they or their group

TABLE III

DESCRIPTIVE STATISTICS AND INTENT-TO-TREAT ESTIMATES OF PROGRAM IMPACT ON KEY OUTCOMES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2010 (2-year endline)				2012 (4-year endline)			
			ITT, with controls				ITT, with controls	
	Control mean	Obs	Coeff.	Std. err.	Control mean	Obs	Coeff.	Std. err.
Transfers								
Treated (group received YOP cash transfer)	0.000	2,677	0.886	[0.019]***				
Received non-YOP transfer or program	0.160	2,005	0.015	[0.019]	0.016	1,868	0.026	[0.009]***
Value of non-YOP program (000s 2008 UGX)	23.0	2,005	61.8	[19.082]***				
Investments								
Enrolled in vocational training	0.152	1,999	0.532	[0.023]***				
Hours of vocational training received	49.0	1,999	340.5	[22.521]***				
Business assets (000s 2008 UGX)	290.2	2,005	377.0	[78.217]***	392.8	1,868	225.0	[62.601]***
Employment								
Average employment hours per week	24.9	2,005	4.1	[1.070]***	32.2	1,864	5.5	[1.284]***
Agricultural	13.9	2,005	−1.2	[0.755]	18.8	1,864	0.4	[0.945]
Nonagricultural	11.0	2,005	5.3	[0.867]***	13.5	1,864	5.1	[0.998]***
Skilled trades only	2.9	2,005	4.7	[0.612]***	2.8	1,864	3.8	[0.548]***
No employment hours in past month	0.100	2,005	−0.011	[0.015]	0.05	1,868	−0.022	[0.009]***
Engaged in any skilled trade	0.170	2,005	0.272	[0.025]***	0.22	1,868	0.261	[0.026]***
Works $\geq$ 30 hours a week in a skilled trade	0.04	2,005	0.054	[0.013]***	0.03	1,868	0.037	[0.013]***

TABLE III

(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2010 (2-year endline)				2012 (4-year endline)			
			ITT, with controls				ITT, with controls	
	Control mean	Obs	Coeff.	Std. err.	Control mean	Obs	Coeff.	Std. err.
Migration and urbanization								
Has changed parish since baseline	0.230	2,244	0.045	[0.024]*	0.350	2,029	−0.077	[0.026]***
Lives in large town or city	0.180	2,004	0.011	[0.017]	0.170	1,859	0.01	[0.019]
Business formality								
Maintains formal records	0.300	2,005	0.114	[0.023]***	0.260	1,868	0.124	[0.023]***
Enterprise is formally registered	0.150	2,005	0.051	[0.017]***	0.110	1,868	0.062	[0.019]***
Pays business taxes	0.210	2,005	0.077	[0.022]***	0.220	1,868	0.085	[0.023]***
Income								
Monthly cash earnings (000s 2008 UGX)	35.2	2,005	14.61	[4.073]***	47.8	1,868	18.19	[4.898]***
Durable assets (z-score)	−0.06	1,993	0.101	[0.047]**	0.150	1,853	0.181	[0.055]***
Nondurable consumption (z-score)					−0.011	1,862	0.180	[0.051]***

Notes. Columns (1) and (4) report the control group mean at each endline, weighted by the inverse probability of selection into the endline sample. Columns (3)–(4) and (7)–(8) report the intent-to-treat (ITT) estimate and standard error of program assignment at each endline. Standard errors are heteroskedastic-robust and clustered by group. We calculate the ITT via a weighted least squares regression of the dependent variable on a program assignment indicator, 13 district (randomization stratum) fixed effects, and a vector of control variables that includes all of the baseline covariates reported in Table II. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

would receive a program from a charity or the government in the future.

The survey data and qualitative interviews suggest that groups commonly elected management committee members to handle procurement, making major training and tool purchases in bulk. These tools were largely distributed to individual members, but about half the respondents said they shared some small or large tools with other group members. In 2010, 90% of group members said they felt the grant was equally shared, and 92% said the leaders received no more than their fair share. Most of the remainder reported only minor imbalances.

Group members typically went their own way to start individual businesses rather than form firms or cooperatives, though they commonly shared some tools. Nearly all treatment groups reported meeting together after the grant, typically several times a year. Half said their community facilitator still engaged with the group, in part because they are from the area, had previous ties to the group, or were interested in their progress. Control groups report meeting just as frequently, in large part because many of these groups preexisted and serve other purposes, and part because they hoped to receive transfers in the future.

VI.A. *Effects on Investment*

A majority of groups and members invest the funds in line with their plan. We assess this investment in two ways. First, and most crudely, we ask treatment group members to estimate how their group and fellow members spent funds. At the median, they estimate they spent 11% on skills training, 52% on tools, 13% on materials; 24% was shared in cash or spent on other things. Second, we ask all respondents to report human and physical capital investments: whether they enrolled in training, and the type and hours of any vocational training received between 2008 and the first endline in 2010, as well as their estimate of the current value of different business assets.[12]

12. For enrollment, we omit any training less than 16 hours, which tends to exclude minor, one- or two-day community-based trainings by charities or government extension officers. Respondents could report multiple trainings, and we report the sum of all hours trained. For physical capital, respondents self-assess the value of their raw materials, inventories, tools, and machines in UGX. We take the sum of these responses and top-code the variable in each round at the 99th percentile to account for extreme values and outliers.

Between 2008 and 2010, 68% of the treatment group enrolled in vocational training, compared to 15% of the control group.[13] On average, treatment translates into 340 more hours of vocational training than controls. Among those who enroll in any training, 38% train in tailoring, 23% in carpentry, 13% in metalwork, 8% in hairstyling, and the remainder in miscellaneous other trades (Online Appendix B.6). Of the 15% of the control group who train (largely in the same four trades), two fifths pay their own way, and the rest receive training from a church, government extension office, or charity. This implies only 6% of controls paid for vocational training themselves absent the grant.

Treatment also increases capital stocks. We calculate the respondent's estimated total value of all business assets and deflate it to 2008 UGX. From Table III, the control group reports UGX 290,200 ($167) of business assets in 2010 and UGX 392,800 ($228) in 2012. By 2010 treatment increases capital stocks by UGX 377,023 ($219), a 131% increase over the control group, and by 2012 stocks increase by UGX 224,986 ($130), a 57% increase over the control group. The relative impact falls over time as the control group's investment begins to catch up, rising 38% between 2010 and 2012 (Table IV). The bulk of this investment is in petty business and agriculture.

1. Gender Differences. Women and men have very similar rates of enrollment and hours of training in both the treatment and control group, and there is no significant difference by gender (regressions not shown).

We see starker gender differences in capital stocks. Table IV reports stock levels, changes, and program impacts. In 2010, control men have roughly twice the capital stock of control women—UGX 347,600 versus UGX 190,777. Between 2010 and 2012, control men also report an increase in their capital stock of 57% and control women show no increase—their stocks actually decrease 15%, though the estimate is not statistically significant. With the YOP program, however, women do extremely well. By 2012 the

13. Among the treated, there is little systematic difference in baseline characteristics between those who enrolled in training and those who did not (regressions not shown). Also, 12.3% of the treatment group returned to formal school versus 10.3% of the control group (Table III).

TABLE IV

Capital Stock Levels, Changes, and Intent-to-Treat Estimates of Program Impact by Gender

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: business assets (000s 2008 UGX)				
	Mean		Change 2010–12		
Estimate	2010	2012	Δ	%Δ	Std. err.
Full sample					
Treatment	725.8	607.8	−135.02	−19	[83.3]
Control	290.2	392.8	109.9	38	[53.5]**
ITT, with controls	377.0	225.0			
Std. err.	[78.2]***	[62.6]***			
Males					
Treatment	906.6	765.0	−168.7	−19	[111.4]
Control	347.6	535.4	199.2	57	[77.8]**
ITT, with controls	487.9	257.0			
Std. err.	[105.5]***	[89.5]***			
Females					
Treatment	343.4	278.8	−66.4	−19	[74.6]
Control	190.7	153.2	−29.1	−15	[50.1]
ITT, with controls	163.5	165.2			
Std. err.	[91.1]*	[54.3]***			
Female–male					
ITT, with controls	−324.4	−91.8			
Std. err.	[134.7]**	[101.5]			
Treatment subgroups (% of total):					
Not funded (11%)	172.9	568.6	375.6	217	[121.8]
Funded, did not train (22%)	331.4	446.5	91.7	28	[106.4]
Funded, trained, not practicing in 2012 (29%)	1005.4	301.8	−720.8	−72	[165.6]
Funded, trained, practicing in 2012 (38%)	1057.0	945.1	−75.4	−7	[153.4]

Notes. Columns (1) and (2) report treatment and control group means at the 2010 and 2012 endline surveys for the full sample, males and females. Below these means we report the intent-to-treat (ITT) estimate of the average treatment effect of program assignment for the full sample, males only, and females only. Robust standard errors are in brackets below the ITT, clustered by group. All statistics are weighted by the inverse of the probabilty of selection into the endline sample. Each ITT is calculated as in Table III. The male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between the program assignment and female dummies; thus the female ITT is the sum of the coefficients on program assignment and this interaction. This approach restricts the coefficients on baseline covariates, including district fixed effects, to be the same across the both genders. Relaxing this constraint has no material effect on the results. Column (3) reports the coefficient on a 2012 dummy in a regression of the dependent variable on the dummy and the full set of controls used in the ITT regressions. This coefficient represents the change in the dependent variable over time. Column (4) reports the precentage change in the dependent variable represented by the coefficient relative to the 2010 endline value. Finally, column (5) reports robust standard errors on this coefficient, clustered by group. ***$p < .01$, **$p < .05$, *$p < .1$.

effect on capital stocks is similar for both genders: UGX 257,000 for men and UGX 165,200 for women. Women's investment appears lower, but the difference from men is not statistically significant. Also, since the counterfactual level and growth of capital stock is so much lower for women, the relative effect of the program is much larger on them. Treatment women increase their stocks more than 100% relative to control women by 2012, whereas treatment men increase stocks by 50% relative to control men.

2. Divestment. Table IV also reports changes in capital stock levels over time. From 2010 to 2012 the treatment group's capital stock falls 19%, overall and for both genders. This decline is not statistically significant, however. Nonetheless, some in the treatment group do divest. Eighty-nine percent of the treatment groups received a grant, but only 48% of the treatment group worked any hours in a skilled trade in the month before the 2012 survey (Table III). Thus nearly half of the treated (and a third of those who trained in a trade) are not practicing a trade four years later. Table IV reports changes in capital stock over time in four endogenous subgroups. First, the 11% of the treatment sample who did not receive a grant look much like the control group in that capital stocks rise steeply over time as they accumulate through retained earnings. Second, the 21% who were funded but did not train have capital stocks close to the level of the control group in 2010 and 2012, suggesting that they did not participate meaningfully in the group grant. Their capital stocks rise over time, perhaps due to accumulation of retained earnings. Third, the 48% who were funded, trained, and still practice the trade in 2012 have capital stocks that hold basically steady, declining only 7%. Finally, the 20% who were funded and trained but did not practice a trade in the month before the 2012 survey see their capital stocks decline precipitously back to the level of the control group. These may be the low-ability or impatient types in our conceptual framework, who find it optimal to divest. We consider alternative explanations in Section VII.

VI.B. Impacts on Employment and Occupational Choice

With these investments, we see a shift in occupation toward skilled work and cottage industry, plus an increase in labor supply overall. Table III reports average weekly hours worked the previous month, broken down by occupation type. We also construct indicators for having no work hours in the past month, for nonagricultural work being the main activity by hours worked, for whether they reported any hours in a skilled trade, and more than 30 hours a week in a skilled trade (the 90th percentile of trade work hours).

The control group reports 11 hours of work a week in 2008, 25 hours in 2010, and 32 hours in 2012. Roughly half the hours are in agriculture, and most of the increase is in agriculture. Increases in nonagricultural work are smaller and mainly in casual labor and petty business. By 2010 controls report an average of just 2.9 hours of work in skilled trades a week, and this changes little by 2012. Twenty-two percent reported any work at all in a skilled trade, and 3% report 30 or more hours a week in a trade.

The program increases total hours worked a week by 4.1 in 2010 and 5.5 in 2012—a 17% increase in labor supply relative to controls both years. This increase is almost entirely in skilled trades. As a consequence, by 2010, 44% of the treatment group report at least one hour worked in a skilled trade, rising to 48% by 2012. Thus participation and hours in a skilled trade are 2 to 2.5 times greater than in the control group.

The treatment group does not decrease their hours in other activities, however. Agricultural hours rise at the same rate in the treatment and control groups. Moreover, even in 2012, the treatment group still works twice as many hours in agriculture hours as skilled work. Trades remain a supplement to income, and young adults are primarily engaged in agriculture. Only 7% of the treatment group report 30 or more hours a week in a trade, 4 percentage points more than the control group. Most are simply adding this new high-skill trade to their portfolio of work activities.

Finally, the sample tends to practice their trades in their original village or parish. As seen in Table III, treatment does not increase migration or urbanization rates. By 2012, treatment group members were actually less likely to have moved (measured by a shift in parish), and were no more likely to live in a large town or Kampala. One reason may be that agriculture remains a major occupation and so both treatment and control remain tied to their traditional land.

VI.C. Impacts on Business Formality and Hired Labor

The program also increases business formalization and employment of others. As reported in Table III, by 2012 the treatment group is 12.4 percentage points more likely to keep records (a 48% increase over controls), 6.2 percentage points more likely to register their business (a 56% increase), and 8.5 percentage points more likely to pay business taxes (a 39% increase).[14]

In spite of being underemployed, many in the sample report they recently hired labor. One reason is that hiring agricultural labor during peak periods of activity (e.g., harvest or land clearing) is common for those with cash. Table V reports hired labor, paid and unpaid, in 2012. As with earlier variables, these outcomes have a long upper tail, and we censor them at the 99th percentile. Sixty-five percent of the control group reports any paid and unpaid labor from family or nonfamily members. There are 2.9 such laborers on average and in total hiring averages nearly 550 hours a month in the control group, or roughly 3.5 "full-time equivalents" working 160 hours a month. Of this labor, 86% is in agriculture. Eighteen percent of the control group report hiring paid labor, but only 8% in nonagricultural pursuits. Unfortunately we only have data on all hours per month, not paid hours. These laborers get paid very little. On a "typical" day where labor was hired, the control group paid UGX 5,200 in total ($3). We estimate monthly pay to others using the product of the typical daily payment, the total days of paid and unpaid labor, and the ratio of paid to unpaid employees. By this (admittedly rough) estimate, the average pay to others in the full control sample is UGX 116,300. Note, however, this is only an estimate because we do not have data on the actual number of hours paid.

Treatment increases paid and unpaid hired labor. The bulk of this increase, however, is outside the skilled trade. The program increases hours of hired labor, paid and unpaid, by 210 hours (+38% relative to the control group). Most of this effect is in agriculture, and treatment leads to just 7.8 additional hours of labor used in the skilled trade. The number of paid employees increases by 0.26 overall (significant at the 10% level). This

14. Because of the effect on recordkeeping, measurement error in earnings could be correlated with treatment, biasing earnings effects in unknown directions. Experiments in Sri Lanka show that precise recordkeeping can lower profit estimates (de Mel, McKenzie, and Woodruff 2007). If true in Uganda, our earnings impact estimates underestimate the true effect.

TABLE V

INTENT-TO-TREAT ESTIMATES OF PROGRAM IMPACT ON HIRED LABOR, 2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Percent reporting nonzero values		Mean		ITT, with controls	
Dependent variable	Control	Treatment	Control	Treatment	Coefficient	Std. err.
No. of paid and unpaid laborers hired in past month, family and nonfamily	65	69	2.9	3.7	0.636	[0.243]***
Nonagricultural activities only	19	26	0.5	0.8	0.213	[0.108]**
Skilled trade only	6	10	0.1	0.2	0.091	[0.038]**
Total hours of paid and unpaid laborers hired in past month	65	69	549.7	785.6	210.6	[63.915]***
Nonagricultural activities only	19	26	75.6	108.9	34.3	[24.711]
Skilled trade only	6	10	7.8	14.6	7.3	[3.895]*
No. of paid laborers hired in past month, family and nonfamily	18	23	0.73	0.97	0.264	[0.148]*
Nonagricultural activities only	8	12	0.19	0.28	0.076	[0.050]
Skilled trade only	3	5	0.05	0.10	0.045	[0.026]*
Total pay to others on "a typical working day" (000s of 2008 UGX)	18	23	5.2	6.6	2.279	[1.414]
Nonagricultural activities only	8	12	2.1	2.8	0.817	[0.743]
Skilled trade only	3	5	0.4	0.8	0.423	[0.264]
Estimated total pay to others in past month (000s of 2008 UGX)	18	23	116.3	133.9	32.3	[33.990]
Nonagricultural activities only	8	12	41.6	43.8	7.2	[15.652]
Skilled trade only	3	5	4.0	7.8	5.5	[3.174]*

Notes. Columns (1) and (2) calculate the proportion of people in the sample reporting a value greater than 0, and hence an indication of the number of people with hired labor in the control and treatment groups and columns (3) and (4) the mean. Columns (5) and (6) calculate the ITT estimate of the treatment effect. See the notes to Table III for details of this regression. All "No. of laborers" figures are censored at the 99th percentile. From the 2010 survey, 49% of employees are family and 51% are nonfamily. Other data are not available on employees in 2010.

implies that for every four people in the treatment group, they hire one (presumably part-time) laborer. Again the bulk of the increase is in agriculture, but hired labor in skilled trades doubles from 0.05 to 0.10—one additional paid employee in a skilled trade for every 20 people directly treated. The treatment effect on the "typical" labor bill per day is UGX 2,279, about $1.33 a day (not statistically significant). The treatment effect on our estimate of total monthly pay to others is large—UGX 32,298 per month, an effect 1.8 times as large as the treatment effect on incomes of the sample, but the effect is so variable (and the measurement imprecise enough) that it is not statistically significant. More than three-quarters of these wages are paid in agriculture. We see a weakly significant increase in the treatment effect on wages paid to hired labor in skilled trades, however—UGX 5,500, about one third of the individual earnings treatment effect.

Who are these paying employers? If we look at the people in the top decile of payroll to others, they report 4.6 paid employees on average, work themselves an average of 53.6 hours a week, and have monthly cash earnings of UGX 138,000 ($80). They also tended to come from urban areas and show greater cognitive ability (digit recall) at baseline.

VI.D. Effects on Income

Increased investment and employment translate into large and growing earnings. Our main income measure is monthly cash earnings in 2008 UGX, net of expenses.[15] Earnings can be a noisy measure of income, however, and cash earnings can understate total earnings because they do not capture nonmarket household production.[16] Thus we complement it with two consumption measures. First, we construct an index of durable assets—a *z*-score constructed by taking the first principal component of 70 measures of land, housing quality, and household assets. Such indexes are relatively reliable proxies of full consumption

15. Respondents estimate gross and net earnings in the previous week and month by business activity, and we sum over all activities. This is a simple but common measure of profits that has been shown in South Asia to be less biased than a more detailed accounting of revenues and expenses in microenterprise experiments (de Mel, McKenzie, and Woodruff 2007).

16. Because agricultural labor did not change with treatment, noncash household production may also not have changed. Unfortunately we do not have data on output from household production. If it falls as a result of treatment, cash earnings will overstate income gains.

aggregates (Filmer and Scott 2008). Second, in 2012 we create an index of short-term nondurable consumption—a z-score constructed by taking the first principal component of 30 select food items consumed in the past three days and expenditures on 28 select nonfood items.[17] Table III reports means and program impacts.

The control group reports monthly cash earnings of approximately UGX 30,825 ($18) in 2008, UGX 35,200 ($20) in 2010, and UGX 47,800 ($28) in 2012.[18] Such growth may come in part from a growing economy, but it also arises from young people gradually increasing their hours worked, capital stocks, and output over time by investing earnings.

Assignment to receive a YOP grant increases earnings by UGX 14,605 ($8.50) in 2010 and UGX 18,186 ($10.50) in 2012—increases of 41% and 38% relative to controls (Table III). We cannot reject the hypothesis that the earnings treatment effect is equal at both endlines.[19]

We see similar patterns in durable and nondurable consumption: rising over time and large program effects. The control group's durable assets rise by 0.1 standard deviations from 2008 to 2010, and rise by 0.21 standard deviations from 2010 to 2012 (Tables II and III). The indexes use the same assets and weights at each survey for comparability. The program effects are of similar magnitudes: durable assets are 0.10 standard deviations greater than the control group in 2010 and 0.18 standard deviations greater in 2012. The impact of the program on nondurable consumption in 2012 is identical, 0.18 standard deviations. Finally, at both endlines the program increases a measure of

17. We use a z-score rather than the additive total for comparability to the durable assets index. Also, these are a selection of total items consumed and so do not sum to a consumption measure. Such abbreviated consumption surveys have been shown to be a relatively reliable proxy of a full consumption survey (Beegle et al. 2012).

18. The 2008 survey has data on gross cash revenues only, whereas gross and net earnings are available in 2010. For the 2008 value of net earnings, we use the 2008 gross amount multiplied by the 2010 ratio of gross to net. This number is merely for descriptive purposes and has no bearing on treatment effect estimates.

19. Online Appendix B.7 shows that among skilled trades, total and hourly earnings are greatest in male-dominated trades such as carpentry and metalworking compared to mixed-gender tailoring. Online Appendix B.8 shows that larger per capita grants are associated with more investment in business assets and more nondurable consumption, but not higher earnings, savings, or durable wealth.

subjective well-being by 12% to 13% relative to controls (Online Appendix B.5).

I. Gender Differences. As with capital, we see some striking gender differences. Figure III displays the levels and trends of real earnings by gender, and Table VI reports program effects by gender. Figure III clearly shows that control women are poorer than men, and this gap widens over time since control women's real cash earnings stagnate over time while men's rise by about 50%. As a result, by 2010 control women's earnings are a third less than men's and by 2012 they are almost two thirds less (Table VI). By 2012, women's durable and nondurable assets are also roughly 0.10 standard deviations and 0.16 standard deviations lower than men's.

Treatment affects both men and women equally, though women take longer to realize these gains. By 2012, treatment increases men's earnings by UGX 17,949 (a 29% increase over control men) and increases women's earnings by UGX 18,630 (a 73% increase over control women). The gender difference in

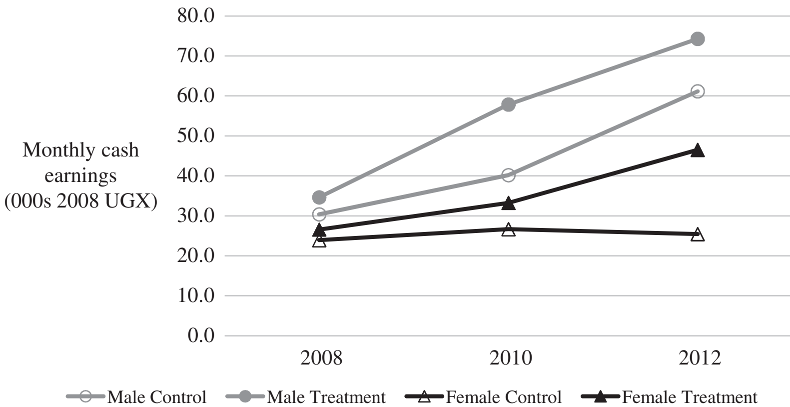

FIGURE III

Earnings Trends, by Treatment Status and Gender

The figure reports the average monthly cash earnings in thousands of 2008 UGX, top-coded at the 99th percentile in each survey round. The 2008 survey has data on gross cash revenues only, while gross and net earnings are available in 2010. For the 2008 value of net earnings, we use the 2008 gross amount multiplied by the 2010 ratio of gross to net. This number is merely for descriptive purposes and has no bearing on treatment effect estimates.

TABLE VI

INTENT-TO-TREAT ESTIMATES OF PROGRAM IMPACT ON INCOME AND POVERTY, BY SEX

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable						
	Monthly cash earnings (000s 2008 UGX)		Durable assets (z-score)		Nondurable consumption (z-score)	Subjective well-being, 1–9 scale	
Estimate	2010	2012	2010	2012	2012	2010	2012
Male ITT	19.47	17.95	0.14	0.18	0.17	0.44	0.52
Std. err.	[5.558]***	[6.287]***	[0.058]**	[0.063]***	[0.065]***	[0.095]***	[0.103]***
Control mean	40.18	61.17	−0.02	0.19	0.05	2.75	3.30
Female ITT	5.23	18.63	0.03	0.19	0.19	0.22	0.20
Std. err.	[5.51]	[7.207]**	[0.073]	[0.1]*	[0.077]**	[0.121]*	[0.155]
Control mean	26.68	25.46	−0.11	0.08	−0.11	2.69	3.29
Female–male ITT	−14.24	0.68	−0.10	0.01	0.02	−0.22	−0.31
Std. err.	[7.932]*	[9.352]	[0.091]	[0.116]	[0.098]	[0.153]	[0.189]
Observations	2,005	1,868	1,993	1,853	1,862	1,996	1,861

Notes. Columns (1) to (7) report the intent-to-treat (ITT) estimate of the impact of program assignment for the full sample, males only, and females only. Dependent variables are described in the text and in the notes to Table III. Robust standard errors are in brackets below the ITT, clustered by group. The mean level of the dependent variable in the control group is reported below the standard error. All statistics are weighted by the inverse of the probabilty of selection into the endline sample. Each ITT is calculated via a wighted least squares regression of the dependent variable on a program assignment indicator, 13 district (randomization stratum) fixed effects, and a vector of control variables that includes all of the covariates reported in Table II: an age cubic, a female dummy, and the variables capturing all group characteristics, employment type and levels, levels of human capital, and initial level of credit access and capital. Each endline is estimated separately. The male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between the program assignment and female dummies; thus the female ITT is the sum of the coefficients on program assignment and this interaction. This approach restricts the coefficients on the control variables, including district fixed effects, to be the same across the both genders. Relaxing this constraint has no material effect on the results. ***$p < .01$, **$p < .05$, *$p < .1$.

treatment effects is not statistically significant. In 2010, the treatment effect on women is not statistically significantly different from zero, while the treatment effect on men is significantly higher, roughly the same level as at four years.

The earnings of treatment women are clearly diverging from control women in Figure III, whereas the earnings of control men are at least keeping pace with treatment men. Between 2012 and 2010 the change of earnings is slightly greater among control than treatment men, but this difference is not statistically significant ($p = .387$, regression not shown).

II. Impact of Treatment on the Treated (TOT). Recall that 11% of groups assigned to treatment did not receive a grant. In Table VII, we estimate the TOT estimate of program impact for key outcomes, using assignment to treatment as an instrument for being treated. Column (1) presents the ITT from Table III, and column (2) reports the TOT estimate. Mechanically, these are larger than the ITT estimates by roughly $\frac{1}{0.89}$. The treated are slightly younger, more educated, and wealthier than the average, which may be why their proposal was not disqualified for administrative reasons or diverted. Nonetheless, failure to receive the grant was relatively unsystematic, and one could consider the ITT a conservative estimate of the grant's impact on YOP applicants.

III. Rate of Return. Annually, these earnings effects are 30–50% the size of the initial cash grant. The annualized 2010 and 2012 earnings ITT estimates in Table III are 30% and 39% of the per capita grant.[20] The 2010 and 2012 TOT estimates are 36% and 49%. All these rates are large relative to real commercial lending rates of 10–30% common among firms in Uganda.

VI.E. *Sensitivity of Economic Impacts to Endogenous Selection or Attrition*

Two concerns, already discussed, are potential bias arising from baseline imbalance and systematic attrition, almost all of which comes from unfound migrants. To address these concerns we test the sensitivity of our results to alternative estimators and

20. We calculate the average annual return as $\left[1+\left(\frac{\text{Earnings ITT}}{\text{Average per capita grant}}\right)\right]^{12}-1$.

TABLE VII

SENSITIVITY ANALYSIS OF INTENT-TO-TREAT ESTIMATES TO ALTERNATE MODELS AND MISSING DATA SCENARIOS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Program impact under alternative models						
					Impute missing dependent variable with mean + (-) X std. dev. for missing control (treatment) indidivuals		
Dependent variable	ITT with controls	TOT with controls	ITT without controls	Diff-in-diff ITT with controls	0.25 Std. dev.	0.5 Std. dev.	"Worst case" Manski bound
Business assets							
2010	377.023	442.138	407.250	407.250	286.196	190.768	−730.579
Std. err.	[78.217]***	[89.274]***	[82.511]***	[82.511]***	[67.517]***	[69.013]***	[143.725]***
2012	224.986	275.556	250.532	250.532	134.696	34.989	−1,384.30
Std. err.	[62.601]***	[72.083]***	[68.404]***	[68.404]***	[53.163]**	[54.588]	[166.878]***
Skilled trade work (hours)							
2010	4.703	5.394	4.551	4.763	3.911	3.24	−3.98
Std. err.	[0.612]***	[0.675]***	[0.621]***	[0.664]***	[0.543]***	[0.555]***	[1.095]***
2012	3.776	4.380	3.666	4.092	3.008	2.277	−3.821
Std. err.	[0.548]***	[0.618]***	[0.569]***	[0.604]***	[0.473]***	[0.486]***	[0.847]***
Monthly cash earnings							
2010	14.605	17.087	15.044	9.112	9.038	3.851	−43.463
Std. err.	[4.073]***	[4.636]***	[4.324]***	[5.472]*	[3.601]**	[3.706]	[7.312]***
2012	18.186	22.045	19.049	16.453	11.333	3.85	−82.326
Std. err.	[4.898]***	[5.560]***	[5.475]***	[5.911]***	[4.346]***	[4.489]	[10.356]***

TABLE VII
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Program impact under alternative models						
					Impute missing dependent variable with mean + (-) X std. dev. for missing control (treatment) indidivuals		
Dependent variable	ITT with controls	TOT with controls	ITT without controls	Diff-in-diff ITT with controls	0.25 Std. dev.	0.5 Std. dev.	"Worst case" Manski bound
Durable assets (z-score)							
2010	0.101	0.119	0.106	0.053	0.03	−0.027	−0.787
Std. err.	[0.047]**	[0.053]**	[0.054]**	[0.059]	[0.043]	[0.044]	[0.089]***
2012	0.181	0.203	0.190	0.153	0.104	0.024	−1.334
Std. err.	[0.055]***	[0.062]***	[0.061]***	[0.072]**	[0.049]**	[0.05]	[0.14]***

Notes. Column (1) replicates the ITT results from Table III. Column (2) estimates the effect of treatment on the treated (TOT) via two-stage least squares, where assignment to treatment is used as an instrument for having received the grant. Otherwise weights and controls are identical to the ITT estimate. Column (3) reproduces the ITT estimates omitting the baseline covariates listed in Table II, but keeping the district (randomization strata) fixed effects. Column (4) estimates the differences-in-differences (DD) treatment effect, where weights and controls are identical to the ITT, except the baseline value of the depedent variable is omitted from the control vector. In columns (5)–(7), we reestimate the ITT in column (1) making hypothetical assumptions about missing data. We impute relatively high values of the dependent variables for missing control group members, and relatively low values for missing treatment group members, thus giving us "bad" or "worst" case scenarios for attrition. Columns (5) and (6) impute the mean of the control (treatment) group plus (minus) 0.25 and 0.5 std. dev. of the group's distribution. Column (7) imputes the highest value in the distribution for controls and the lowest for treatment, often called the worst-case Manski bound. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

missing data scenarios in Table VII (with more outcomes in Online Appendix B.9).

Results are robust to exclusion of the baseline covariates and to the difference-in-differences (DD) estimator. Column (3) of Table VII estimates the ITT without controls. In general the impacts are unchanged or grow larger. Column (4) estimates the DD treatment effect controlling for other baseline variables. Earnings and durable assets were systematically higher in the treatment group at baseline, and so the DD estimate is systematically lower than in our main ITT specification. The 2010 DD treatment effects on earnings and durable assets are indeed lower, with earnings only significant at the 10% level and the effect on assets not significant. Moreover, the 2012 DD treatment effects are uniformly large and robust, and we cannot reject equality with the main ITT estimates. We also show in Online Appendix B.3 that reweighting treatment effects to match population characteristics (age, wealth, education) provides relatively similar impacts, especially for 18- to 30-year-olds.

We also bound treatment effects for possible attrition bias. We impute outcome values for unfound individuals at different points of the observed outcome distribution. The most extreme bound, from Manski (1990), imputes the minimum value for unfound treated members and the maximum for unfound controls. Column (7) of Table VII reports the lower (most pessimistic) Manski bound. Following Karlan and Valdivia (2011), we also calculate less extreme bounds. We report the scenarios that would reduce program impacts: ones where for the control group we impute a high outcome, the found control mean plus 0.25 or 0.5 standard deviations of the found control distribution; for the treatment group we impute a low outcome, the found treatment mean minus 0.25 or 0.5 standard deviations of the found treatment distribution. We reestimate ITT effects in columns (5) and (6) for plus or minus 0.25 and 0.5 standard deviations. Note these imply large and systematic differences between missing treatment and control members—column (6) assumes unfound control group member outcomes are roughly 1 standard deviation greater than unfound treatment group member outcomes. All of the treatment effects in Table VII are robust 0.25 standard deviations except for the two-year durable asset impact. Nearly all of the effects are greater than 0 for 0.5 standard deviations, and hours in skilled trades remain robust.

VI.F. Noneconomic Impacts

Idle hands do the devil's work, as the saying goes. This folk wisdom is pervasive, and enhancing social cohesion and stability is a common rationale for employment programs, including YOP (World Bank 2007, 2010, 2012). We collected data on more than 50 self-reported measures of sociopolitical attitudes and behavior. We collect the variables into conceptual "families" and create additive standardized indices (Kling, Liebman, and Katz 2007).

Our measures are based mainly on existing measures and include indices of: (i) kin integration, capturing 4 measures of household relations; (ii) community participation, capturing 10 measures of associational life and collective action; (iii) community public good contributions (2012 only) including 7 types of goods; (iv) antisocial behavior, based on 8 forms of aggressive behavior with neighbors, community leaders, and police, plus 18 additional measures in 2012; and (v) protest attitudes and participation, based on 7 measures of participation in and attitudes around violent antigovernment protests following the 2011 elections.[21]

Overall, we see little evidence of a positive social effect on males after two years, and none whatsoever after four years. Table VIII reports impacts on the main outcome families (disaggregated summary statistics and treatments effects are reported in Online Appendix B.10). The point estimates are typically less than 0.1 or 0.05 standard deviations, and standard errors on these z-scores are equally small, suggesting we can rule out medium to large changes. Just 2 of the 27 regressions show a small, statistically significant impact, both at the two-year endline. We regard these as at best temporary effects and probably statistical anomalies.[22]

21. After the 2011 elections and the "Arab Spring" protests, the opposition organized marches in major towns. Some turned into rioting and looting, especially in Gulu, the largest northern town. Our sample seldom lived in these towns, so only 2–3% actively participated. Nearly half the sample, however, said they felt the protests were justified, nearly a quarter said the violent tactics were justified, and roughly a tenth said they wished there had been a protest in their district and that they would attend, even if it turned violent.

22. First, treatment is associated with a 0.098 standard deviation increase in community participation, but the treatment effect at four years is 0. Second, although we see no effect on antisocial behavior overall, when we disaggregate by gender we see an unusual pattern at two years: a 0.18 standard deviation decline

TABLE VIII

INTENT-TO-TREAT EFFECTS ON SOCIAL OUTCOME FAMILIES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: family indices (z-scores)								
	Kin integration		Community participation		Community public good contributions	Antisocial behavior			Protest attitudes and participation
						Original		Extended	
	2010	2012	2010	2012	2012	2010	2012	2012	2012
Full sample ITT	0.011	0.044	0.095	0.005	0.010	−0.069	0.049	0.013	−0.019
Std. err.	[0.049]	[0.047]	[0.047]**	[0.050]	[0.049]	[0.045]	[0.047]	[0.046]	[0.043]
Male ITT	0.058	−0.001	0.081	0.076	0.054	−0.177	0.056	0.016	−0.003
Std. err.	[0.056]	[0.052]	[0.054]	[0.062]	[0.061]	[0.055]***	[0.057]	[0.057]	[0.057]
Female ITT	−0.0801	0.128	0.122	−0.129	−0.0731	0.140	0.0349	0.00847	−0.0491
Std. err.	[.091]	[.09]	[.084]	[.073]*	[.078]	[.072]*	[.08]	[.083]	[.079]
Female–male ITT	−0.138	0.130	0.042	−0.205	−0.128	0.317	−0.021	−0.008	−0.046
Std. err.	[0.106]	[0.104]	[0.097]	[0.090]**	[0.099]	[0.088]***	[0.098]	[0.102]	[0.104]
Observations	2,005	1,868	2,005	1,868	1,868	2,005	1,868	1,868	1,868

Notes. Columns (1)–(9) report the intent-to-treat (ITT) estimate of the effect of program assignment for the full sample, males only, and females only. Each dependent variable is an index of several related outcomes, classified as being in the same family. Each family index is a z-score and is constructed by standardizing each outcome, adding them, and standardizing the sum. The specific outcomes in each family are described in the text. Robust standard errors are in brackets below the ITT, clustered by group. All statistics are weighted by the inverse of the probabilty of selection into the endline sample. Each ITT is calculated via a wighted least squares regression of the dependent variable on a program assignment indicator, 13 district (randomization stratum) fixed effects, and a vector of control variables that includes all of the covariates reported in Table II (an age cubic, a female dummy, and the variables capturing all group characteristics, employment type and levels, levels of human capital, and initial level of credit access and capital) as well as additional sociopolitical baseline controls: baseline values of kin integration, community integration, antisocial behavior (self-reported aggression and disputes), and acts of war violence experienced. ITT estimates for each endline are estimated separately. The male- and female-only ITTs are calculated in a pooled regression (within each endline round) that includes an interaction between the program assignment and female dummies; thus the female ITT is the sum of the coefficients on program assignment and this interaction. This approach restricts the coefficients on the control variables, including district fixed effects, to be the same across the both genders. Relaxing this constraint has no material effect on the results. ***$p < .01$, **$p < .05$, *$p < .1$.

Admittedly, our data have limitations: they are self-reported, and there were no major episodes of unrest to measure. We also did not measure every possible externality, especially collective or general equilibrium changes that accompany broader structural change. Nonetheless, the absence of a large change on the individual margin runs counter to many expectations.

VII. Discussion and conclusions

These results show that cash grants to groups of young people who develop business plans have large and persistent impacts in moving the underemployed into nonagricultural jobs, increasing earnings and work hours. For men, the counterfactual is also growing incomes and employment, although mainly in agriculture and petty business rather than trades. One of the most striking findings, however, is that women's investment and earnings stagnate in the absence of the program, but the program sets them on a solid growth path (at least over four years).

Few people create small formal enterprises, but a sizable proportion of the treatment group has several of the ingredients: paid employees, formal registration, or taxation. Future surveys will show whether such "proto-firms" become larger and more formal over time and grow employment. It will also be important to assess whether the intervention crowded out others from these professions within treatment villages.

In the end, YOP appears to have reached a group of motivated, able young people, who on average were neither exceptionally poor nor uneducated relative to their peers, in an economy with little financial depth but bouncing back from civil strife. Our conceptual framework suggests this is exactly the group to benefit from a windfall.

Several patterns also suggest that the sample should continue to grow: the regional economy is growing; earnings growth barely slows between the first and second endlines, and the average treatment person is still working less than 40 hours a week. We do not, however, see sustained growth in capital stocks in the treatment group, even though they enjoyed robust and rising earnings. One possibility is the program brought the

among men, and a 0.14 standard deviation increase among women, both significant at least at the 10% level. The effect disappears at four years.

average person to their efficient scale given their current entrepreneurial abilities. Alternatively, the treatment group may have yet to take full advantage of their initial capital investments. Only future follow-up of the sample will tell.

VII.A. *What Constraints Did the Program Relieve?*

These patterns also imply that applicants to the program began below their steady state, but the program was sufficient to relieve some constraint. That constraint merely slowed men's capital and earnings growth, but was severe enough to seemingly trap women. If we can pinpoint this constraint, we can learn why the program was effective and whether we can generalize. We consider evidence for several alternatives.

I. Credit Constraints. Several pieces of evidence suggest that credit constraints are an important ingredient. In Section II, we saw that our sample began severely credit constrained. Qualitatively it is also clear that trades had large start-up costs in terms of skills training and equipment, and that the cash grant was large enough to pay these costs. Almost none of the control group paid for training on their own, even though they had made specific plans and (as we've seen) their potential returns were high.

Credit constraints and fixed costs may also help explain why the control group is investing lesser amounts in agriculture and petty business instead of trades. Qualitatively, these occupations appear to have lower fixed costs of start-up, and people can incrementally invest their earnings in them over time. We cannot say whether the treatment group would have been better off investing some of their cash grants in businesses other than trades. But for most, skilled trades simply were not an option in the absence of a grant.

YOP effects are also consistent with the predicted effects of a restricted cash windfall under credit constraints. Most of all, the average returns to capital appear to be quite high, which would not be expected with savings constraints or time inconsistency alone.

Furthermore, a majority of those who received the grant did not divest. On average, capital stocks did decline, but not significantly so, and principally in a small subgroup of those who decided to exit trades entirely. This subgroup could include those who failed, those who discovered they did not have an affinity for trades, or those who found themselves above efficient scale.

Our conceptual framework predicts heterogeneity of this nature. Cash windfalls should have the largest effect on the most constrained, and so impacts should decrease in initial wealth and for existing entrepreneurs and increase with ability. It is ambiguous about the role of present orientation. Patterns of treatment heterogeneity in our sample are consistent with these predictions, but for the most part the relationships are not statistically significant. We analyze treatment heterogeneity in Table IX by interacting assignment to treatment with a proxy for each form of heterogeneity. These include an indicator for being in a skilled trade and three standard normal indexes that are weighted averages of their components, including working capital (initial asset wealth, savings and lending, and perceived credit access), human capital and ability (education, working memory, and health), and patience (10 self-reported measures of time preferences, including both patience and self-control).[23] We analyze heterogeneity in business assets (columns (1) to (3)) and earnings ((4) to (6)), pooling the 2010 and 2012 endlines.

In general, the coefficients on the characteristics and their interactions with treatment have the expected signs: those with existing skilled trades and more working capital have higher capital and earnings but lower treatment effects; those with higher human capital have higher capital stocks and earnings and higher treatment effects; and the patient have higher capital stocks and earnings and an ambiguous change in earnings treatment effects. None of the interactions are statistically significant, however, except for the effect of working capital on the women's treatment effect on earnings (column (6)). In practice, looking at multidimensional heterogeneity reduces power.[24] More important, our sample is selective, and so may underrepresent the

23. See Online Appendix B.11 for further details. All are measured at baseline except for patience, which is measured in 2010. This patience measure is invariant to treatment, and we consider it a time-invariant characteristic while treating the results with caution. The other coefficients are not materially affected by its inclusion or exclusion.

24. In our case the model makes predictions conditional on the levels of other characteristics, so we prefer to examine them together. The results are similar if we analyze each characteristic separately.

TABLE IX

TREATMENT HETEROGENEITY BY INITIAL WORKING CAPITAL, HUMAN CAPITAL, PATIENCE, AND ENGAGEMENT IN A SKILLED TRADE

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable (2010 and 2012 endline data pooled)					
	Business assets (000s 2008 UGX)			Monthly cash earnings (000s 2008 UGX)		
	Full sample	Males	Females	Full sample	Males	Females
Assigned to treatment	419.8	550.6	189.0	15.9	20.3	5.9
	[68.9]***	[96.6]***	[89.6]**	[4.2]***	[5.5]***	[6.1]
2012 endline	144.3	202.8	−17.6	19.2	22.1	0.8
	[70.5]**	[79.9]**	[58.5]	[4.7]***	[5.2]***	[4.9]
Assigned × 2012 endline	−238.3	−354.5	−45.8	1.5	−4.0	11.9
	[90.8]***	[128.9]***	[97.0]	[5.8]	[7.8]	[7.5]
Female	−298.1			−11.4		
	[65.6]***			[4.3]***		
Female × 2012 endline	−84.0			−13.3		
	[80.4]			[5.6]**		
Engaged in skilled trade	201.8	275.2	86.6	15.5	26.9	−33.0
	[229.1]	[252.9]	[177.4]	[15.3]	[16.5]	[17.1]*
Assigned × Skilled trade	−90.3	−137.1	−205.7	−10.0	−15.9	12.0
	[220.7]	[256.2]	[214.4]	[14.9]	[16.9]	[21.7]
Working capital index (z-score)	127.8	128.9	162.8	15.7	12.0	23.1
	[50.6]**	[66.4]*	[59.8]***	[5.2]***	[6.0]**	[7.7]***
Assigned × working capital index	−83.7	−75.4	−171.4	−4.3	2.2	−18.3
	[66.8]	[89.4]	[75.7]**	[6.3]	[7.7]	[8.4]**

TABLE IX
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable (2010 and 2012 endline data pooled)					
	Business assets (000s 2008 UGX)			Monthly cash earnings (000s 2008 UGX)		
	Full sample	Males	Females	Full sample	Males	Females
Human capital index (z-score)	45.9	108.9	10.8	9.0	12.3	5.9
	[32.3]	[47.5]**	[40.8]	[2.4]***	[3.4]***	[3.3]*
Assigned × human capital index	25.1	−50.3	43.7	3.0	1.4	3.6
	[48.6]	[71.6]	[44.5]	[3.6]	[4.9]	[5.3]
Patience index (z-score)	23.8	13.9	13.5	5.7	7.5	0.7
	[31.5]	[46.7]	[32.3]	[2.1]***	[3.0]**	[2.4]
Assigned × patience index	−27.6	−6.7	−36.5	2.0	−0.3	7.4
	[50.2]	[71.9]	[46.2]	[3.3]	[4.3]	[4.8]
Observations	3,873	2,574	1,299	3,873	2,574	1,299

Notes. Columns (1)–(6) report coefficients and standard errors from a weighted least squares regression of the dependent variable on the listed independent variables plus all control variables in Table II, weighted by the inverse probabilty of selection into the endline sample. Standard errors are clustered at the individual level. ***$p < .01$, **$p < .05$, *$p < .1$.

capital-rich, low-ability, and highly impatient. That is, the important heterogeneity may be outside the sample.

Finally, the male-female differences we see in impacts (e.g., Figure III) are somewhat puzzling, but part of the explanation could be that women are more credit constrained and more present biased. Table X reports male-female differences at baseline. Women begin with much lower liquidity and credit access, and more debt, than do men. They are thus more likely to find themselves below any minimum capital requirement for a business, and have fewer earnings to save and reach that requirement over time. They also start with 0.43 standard deviations lower human capital and are 0.13 standard deviations less patient than men. Thus at the interest rates they currently face, they are less likely than men to be below their optimal steady state level of capital. The fact that women do well under the program suggests that borrowing rates lower than the returns of 30–50% we see could be sufficient to spur women to start enterprises even with lower initial abilities.

Studies of established female entrepreneurs have hypothesized that such differences in time preferences could account for low returns to cash but high returns to in-kind capital (Fafchamps et al. 2011). Our results are consistent with this finding, to the extent that our restricted grant shows high returns and women report lower patience and self-control. But simple time inconsistency is insufficient to explain the take-off of women with the grant. Some other constraint is needed to place women below steady state.

Finally, recall that treatment women also take longer to reach treatment men's level of earnings. It is difficult to say why. Since they start with lower wealth, in principle the grant should speed them to their steady state faster. Women may need to overcome noneconomic barriers, as discussed shortly. But as we see in Table X, women start with less experience and human capital and are more present biased. They may need to acquire entrepreneurial abilities through practicing business, more so than men who do not face these same constraints.

II. Risk and Missing Insurance. There is no formal insurance in northern Uganda, and informal insurance is partial at best. In this case, risk-averse people will favor lower-return, safer investments. There are three reasons, however, that risk is less likely to

TABLE X

Male–Female Differences in the Absence of the Program

	(1)	(2)	(3)
	Baseline, full sample		
Dependent variable	Female coefficient	Std. err.	Male mean
Durable assets (z-score)	0.01	[0.045]	−0.11
Savings (000s of UGX)	−5.55	[4.973]	28.51
Monthly cash earnings (000s 2008 UGX)	−17.4	[5.794]***	70.95
Can obtain 100,000 UGX ($58) loan	−0.07	[0.021]***	0.39
Can obtain 1,000,000 UGX ($580) loan	−0.04	[0.013]***	0.12
Working capital index	−0.07	[0.043]*	0.04
Debt (000s 2008 UGX)	6.79	[4.549]	16.65
Conditional on nonzero debt	41.3	[15.177]***	91.76
Human capital index (z-score)	−0.43	[0.047]***	0.15
Patience index (z-score), in 2010	−0.14	[0.051]***	−0.04

Notes. Column (1) reports coefficients on a female dummy from a least squares regression of each dependent variable on the dummy and district (randomization strata) fixed effects. The regressions include the full sample at baseline but only the control group at each endline. Column (2) reports robust standard errors on the female dummy, clustered by group. For comparison purposes, column (3) reports the mean value of the dependent variable for males.

be the main constraint on our sample. First, initial levels of savings and nonenterprise work hours are very low to begin with. Thus there is little indication that traditional work or savings is preferred to enterprise work. Second, traditional work appears to be at least as risky as enterprise. We unfortunately do not have measures of individual income uncertainty, but from Table III we can see that the standard deviation of earnings in the treatment group after the program is smaller than the full sample at baseline or the control group at endline.[25] Finally, a regular stream of transfers is better suited than a one-time grant at stimulating productive investment (Bianchi and Bobba 2013).

III. Social Norms and Social Pressure. The fact that men's businesses and earnings grow in the absence of the program but women's do not could also point to some socially constructed

25. Because the sample includes a mix of high- and low-ability people, where some succeed at enterprise and some do not, if anything we expect the variance of earnings to increase with treatment. The fact that it does not is suggestive evidence that entrepreneurship is no risker than traditional work.

constraint on women that a restricted cash transfer relieves. For example, Field, Jayachandran, and Pande (2010) show that traditional norms against women's participation in business reduce the effect of an entrepreneurship program. Another explanation is social pressure. Fafchamps et al. (2011) review evidence that suggests that people, especially women, are subject to external pressure to share resources. To the extent that husbands and fathers can draw on their wife's finances, wealth could be diverted before they can invest it.

Social pressure and norms are hard to reconcile with the fact that women take off after a grant and do not divest. It is possible, however, to imagine a constraint that binds women only before they have started a business. For instance, starting a business could remove the social approbation to working in business, or relatives or husbands could find it harder to capture ongoing earnings than an initial lump sum. In our model, these situations resemble the case where a restricted cash grant allows a sophisticated time-inconsistent person to commit to their investment. We have no evidence to weigh for or against these social constraints, but note that in one of the few experiments to attempt to test it directly, Fafchamps et al. (2011) find little evidence of external pressure playing a role in women's differential performance.

One possibility is that the group structure provided the commitment device necessary to help some people invest the lump sum, and the ongoing presence of the group and periodic meetings maintained a degree of social pressure to not divest. We examine heterogeneity of investments and earnings by group characteristics in Online Appendix B.12 and find that groups with a better baseline working relationship have collectively higher outcomes, whereas heterogeneous backgrounds, size, and length of existence play little role. This finding could reflect groups providing mutual support, positive peer effects, and economies of scale (e.g., shared tools). The role of group organization in cash transfers merits experimental exploration in the future, not least because group disbursement may be an inexpensive method of self-selection, targeting, and delivery.

VII.B. Potential for Replication and Scaling

The scale of this program is limited by the reliance on grants and the absence of any repayment mechanism. Whether it is worth expanding with existing aid or state revenues thus depends on the relative returns to other programs, such as cash, agricultural extension (an important development investment in Uganda), or alternate job creation models. Comparable evidence is almost nonexistent at present. In the meantime the high returns we observe to YOP are promising.

The potential for replication and scale also depends on whether other young adults would experience similar treatment effects. In our results, program effects are similar across people with widely different education and wealth levels. The population average treatment effects, moreover, suggest that treatment effects could be high among those aged 18–30 in northern Uganda. There is little doubt that our sample's unobserved initiative and ability improved their performance, but continuing to target the "motivated poor" would only limit the scalability of a program within communities, not across new villages and countries.

Furthermore, several aspects of YOP's design probably limited returns—YOP encouraged people to invest in a narrow menu of trades that might not fit everyone's abilities or interests, and led a sizable number of people in a community to practice the same one or two trades. YOP-like programs could conceivably raise returns and appeal to a wider swath of people by promoting investment in a wider set of sectors. Uganda is currently replicating and expanding YOP, and this is exactly their approach.

There are also a variety of settings that resemble Uganda in key respects. It sits at roughly the median level of development in sub-Saharan Africa. Like northern Uganda, the majority of African countries are growing after a long period of political uncertainty, but access to finance continues to be scarce, expensive, and short-term, especially in rural areas.

Scaling will nonetheless introduce unknown general equilibrium effects. This is an important limit on expansion. It's not clear whether village economies can support numerous new businesses, even if a program expanded the range of permissible enterprises. Moreover, the effects of a large program on aggregate demand and inflation are uncertain. Even so, a government-led program that treated 2–5% of young people in a rural community—much as YOP did in Uganda—has considerable scope for replication and expansion while minimizing the risk of depressed returns. This is largely speculative, however, and impacts should be tested in additional settings. The external validity of the intervention, general equilibrium effects, and cost-benefit comparison to pure cash grants remain important questions for future research.

VII.C. Broader Significance

Youth unemployment is a huge and important challenge, and the YOP results show that a reasonably simple and replicable intervention worked extremely well for a broad range of young people. The results contrast with somewhat disappointing results from job training programs in developed and middle-income countries and complement related work showing that cash grants increase businesses' profits on the intensive margin. In contrast to this literature, however, we find that grants to women generate equally high returns. This could be a feature of the Ugandan setting or the design of the program, but it may also be an indication that cash has more promise for women's self-employment on the extensive margin.

The results complement the growing enthusiasm for unconditional cash transfers (UCTs) to the poorest. Existing research on UCTs mostly focuses on education and health investments in children, and finds high impacts (Baird, McIntosh, and Özler 2011; Benhassine et al. 2013). Recent evidence from Kenya suggests large unconditional grants are partly invested and earn high returns (Haushofer and Shapiro 2013). YOP was not an unconditional program, screens for initiative, and likely restrains participants initial decisions. Nonetheless, the sample contains many very poor young people, and the evidence suggests they invest the money wisely when unsupervised. Whether such restrictions play an important role remains to be tested.

In principle, microfinance could play the same role as grants. In practice, however, microfinance in Uganda tends to be an expensive, short-term credit source. We estimate that the YOP program, for all its high returns, could be "paid back" in 4.7 years at a real interest rate of 15% if it were a loan and in 7.3 years at 25%, assuming the full earnings increase went to repay it (calculations in Online Appendix B.13). In Uganda (and many other African countries) such loan terms are rare, and it would take infinite time to pay back at microfinance rates of 100% or more. This suggests that lowering the cost and raising the term length of microfinance is crucial.

More generally, the role of credit constraints we see here provides rare micro-level evidence for influential macro-level theories of development, ones that stress the importance of credit constraints in occupational choice and the economy-wide shift from agricultural to nonagricultural work central to the "process of development" (Banerjee and Newman 1993; Levine 1997). Indeed, one of the Ugandan government's major aims with the YOP program was to accelerate such structural change, however small in scale.

Finally, the government also hoped to promote social cohesion and stability. We found no evidence, however, that reducing individual idleness and poverty also reduces dislocation, aggression, or other unrest. Other experiments come to similar conclusions (Blattman and Annan 2011; Blattman et al. 2013). This suggests that the case for public investments in employment should be made on the economic returns alone. Fortunately, this economic case is strong.

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Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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