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

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Abstract: 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 impact, however, on social cohesion, anti-social behavior, or protest. Impacts 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.

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I. INTRODUCTION

A third of the world 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 paper 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 grade eight years, earned less than a dollar a day, and worked less than 12 hours a week.

Cash is a controversial intervention, in part because of concerns 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 ambiguous.

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,

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[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] While 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; Blattman and Miguel 2010; Collier and Hoeffler 1998; Goldstone 2002; World Bank 2012; World Bank 2010; World Bank 2007).

or hairstyling. After four years the treatment group had 57% greater capital stocks, 38% higher earnings, and 17% more hours of work than the control group. Treatment group members also became more “firm-like” in that they were 40 to 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 applicants were women and the program had large and sustained impacts 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 impacts 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 non-economic impact at the individual level. There were little to no impacts on our measures of individual community integration, local and national collective action, anti-social behavior, or violent protest. Blattman, Fiala and Emeriau (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 paper 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 to help understand why YOP had such large economic impacts (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 afterwards. 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. Non-standard 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). While designed to help the poor cope with shocks or pay for education and health, it is also hoped these programs will stimulate new enterprise (Fizbein, Schady, and Ferreira 2009; IPA 2013; Karlan and Morduch 2009). These programs have successfully reduced risk and poverty, but so far show little impact on employment or earning capacities.[4]

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[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 impacts on men but none on women (Cho et al. 2013). In Uganda, girls' self-employment rose but not earnings (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 (Angelucci, Karlan, and Zinman 2012; Attanasio et al. 2011; Augsburg et al. 2012; Banerjee et al. 2013; Crépon et al. 2011). 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 non-farm production (Macours, Premand, and Vakis 2012; Maluccio 2010).

There are exceptions, and our evidence is consistent with three program evaluations in Asia and Latin America: Gertler et al. (2012) and Bianchi and Bobba (2013) show that a Mexican CCT program stimulates self-employment; Macours et al. (2012) show that a grant raises non-farm 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 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 (Aghion and Bolton 1997; Banerjee and Newman 1993; Galor and Zeira 1993; King and Levine 1993; Piketty 1997). We also see results consistent with canonical models of surplus labor, in that increasing non-agricultural 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 impact 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 GDP per capita of roughly $330. The economy has been stable and growing, with real GDP at market prices rising 6.5% per 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 onwards, 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 commonplace 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. While 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 non-agricultural employment. To do so, in 2006 it announced a third NUSAF component: the Youth Opportunities Program, or YOP.

II.B. Intervention: The Youth Opportunities Program

YOP invited groups of young adults, aged roughly 16 to 35, to apply for cash grants in order 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 rather than 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 micro-finance 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 non-agricultural skills training and enterprise start-up costs. They could request up to $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 groups proposed that different members would train two to three different trades. Females and mixed groups often chose trades common to both genders, such as tailoring or hairstyling. Males and a small number of females 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 commonplace 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 8km. 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 advisors, 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 and so YOP also required "facilitators"—usually a local government employee, teacher, or community leader—to meet with the group several times, 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 UGX 12.9 million Ugandan shillings (UGX) per group, or $7,497 (all figures in the paper are quoted in 2008 UGX and dollars). Per capita grant size varied across groups due to variation in group size and amounts requested. 80% 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 2006-07, prior to our study. By 2008, 14 NUSAF-eligible districts had funds remaining. Figure II.i 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

[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 since 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.

[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.

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 government disqualified about 70 applications, mainly for incomplete information or ineligibility (e.g. many group members over 35 years, 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 five million, and control groups are typically very distant from treatment villages. See Figure II.ii for a map of groups per parish.

III. DATA AND DESCRIPTION OF THE SAMPLE

III.A. Data and randomization balance

The 535 groups contained nearly 12,000 members. We survey five people per group three times over four years—a panel of 2,677 (seven 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 of 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]

[8] Control groups were not formally waitlisted for the program, though officials privately expressed an interest in funding them in 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.

[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 analysed 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). 4% of the groups had missing members, and these were not included in the baseline survey.

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 one of the 13 at endline.

The government disbursed funds between July and September 2008. We conducted the first 2-year endline survey between August 2010 and March 2011, 24 to 30 months after disbursement, and a 4-year survey between April and June 2012, 44 to 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. In Phase 1, we attempted to interview all 2,677 people in their last known location. 37% 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.; Friedman et al. 2011), though higher than some panels of existing entrepreneurs, who are typically urban, less mobile, and in some cases are screened for attrition before the experiment (de Mel, McKenzie, and Woodruff 2012; Fafchamps et al. 2011; Udry and Anagol 2006).

Of greater concern is correlation between attrition and treatment, as in Table 1. The treatment group was 5 pp 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 pp 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 below 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 pp more vocational training, 0.07 standard deviations (SD) greater wealth, 56% greater savings (though only in the linear, not in log form), and 5 pp 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 SD 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 2000 households. A quarter did not finish primary school, but 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, while 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% are engaged in a skilled trade. Cash earnings in the past month averaged a dollar a day. Savings were $15 on average. Only 11% reported savings. 33% held loans, but these were small: under $7 at the median among those who have any loans, mainly from friends and family. About 10% reported they could obtain a large loan of 1,000,000 UGX (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 a 2008 population-based household survey, our sample has 1.7 years more education, 0.15 SD more wealth, is 7.5 pp more urban and 5.4 pp 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, and 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 rather 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 for 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 where 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.

Anti-poverty 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 non-standard 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 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 we discuss 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 where 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 impact of credit constraints.

Credit constraints have additional predictions. In the absence of a credit constraint, people should divest after being compelled to invest, while with credit constraints only low ability types will do so. Furthermore, cash windfalls have the largest impact 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 groups, prepare proposals, and wait a long period of time before any 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 per 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).

[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.

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 2 (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, and so we top-code all currency-denominated, hours worked, and employee variables at the 99th percentile.

Finally, since outcomes are self-reported, we will overestimate the impact if the treatment group over-reports well being due to social desirability bias, or if the controls under-report 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 below) 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 that reported 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 pp more likely to report a non-YOP program from the government or a charity, and 2.6 pp 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 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 group, or were interested in their progress. Control groups report meeting just as frequently, in large part because many of these groups pre-existed and serve other purposes, and part because they hoped to receive transfers in the future.

VI.A. Impacts 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, and 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]

Between 2008 and 2010, 68% percent 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 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.

Gender differences. Women and men have very similar rates of enrolment 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% while control women show no increase—their stocks actually decrease 15%, though the estimate is not statistically significant. With the YOP program,

[12] For enrolment, we omit any training less than 16 hours, which tends to exclude minor, 1 or 2 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.

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

however, women do extremely well. By 2012 the impact on capital stocks is similar for both genders: UGX 257,000 for men and 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 impact of the program is much larger on them. Treatment women increase their stocks more than 100% relative to control women by 2012, while treatment men increase stocks by 50% relative to control men.

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 with both genders. This decline is not statistically significant, however. Nonetheless, some in the treatment group do divest. 89% 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 towards 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 non-agricultural 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 per 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 non-agricultural 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 per week, and this changes little by 2012. 22% 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 per 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 pp 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 pp more likely to keep records (a 48% increase over controls), 6.2 pp more likely to register their business (a 56% increase) and 8.5 pp more likely to pay business taxes (a 39% increase).[14]

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

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 commonplace 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. 65% of the control group reports any paid and unpaid labor from family or non-family members. There are 2.9 such laborers on average and in total hiring averages nearly 550 hours per month in the control group, or roughly 3.5 "full time equivalents" working 160 hours per month. Of this labor, 86% is in agriculture. 18% of the control group report hiring paid labor, but only 8% in non-agricultural pursuits. Unfortunately we only have data on all hours per month, not paid hours. These paid 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 since 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 impact is in agriculture, and treatment leads to just 7.8 additional hours of labor are used in the skilled trade. The number of paid employees increases by 0.26 people overall (significant at the 10% level). This 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—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. Impacts 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 since they do not capture non-market 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 indices are relatively reliable proxies of full consumption aggregates (Filmer and Scott 2008). Second, in 2012 we create an index of short-term non-durable 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 non-food 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 also arises from young people who are 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]

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[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] Since agricultural labor did not change with treatment, non-cash 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.

[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, 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.

[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

We see similar patterns in durable and non-durable consumption: rising over time and large program impacts. The control group's durable assets rise by 0.1 SD from 2008 to 2010, and rise by 0.21 SD from 2010 to 2012 (Tables II and III). The indices use the same assets and weights at each survey for comparability. The program impacts are of similar magnitudes: durable assets are 0.10 SD greater than the control group in 2010 and 0.18 SD greater in 2012. The impact of the program on non-durable consumption in 2012 is identical, 0.18 SD. Finally, at both endlines the program increases a measure of subjective well-being by 12 to 13% relative to controls (Online Appendix B.5).

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 impacts 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 non-durable assets are also roughly 0.10 SD and 0.16 SD lower than men's.

Treatment impacts 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 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, while 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=0.387, regression not shown).

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

shows that larger per capita grants are associated with more investment in business assets and more non-durable consumption, but not higher earnings, savings or durable wealth.

ITT from Table III, and Column 2 reports the TOT estimate. Mechanically, these are larger than the ITT estimates by roughly $^{1}/_{0.89}$. 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.

Rate of return. Annually, these earnings impacts are 30 to 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 to 30% common among firms in Uganda.

VI.E. Sensitivity of economic impacts to endogenous selection or attrition

Two concerns, discussed above, 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 missing data scenarios in Table VII (with more outcomes in Online Appendix B.9).

Results are robust to exclusions of the baseline covariates and to the 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 impact 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)

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

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 SD of the found control distribution; and for the treatment group we impute a low outcome, the found treatment mean minus 0.25 or 0.5 SD of the found treatment distribution. We re-estimate ITT effects in Columns 5 and 6 for +/- 0.25 and 0.5 SD. Note these imply large and systematic differences between missing treatment and control members—Column 6 assumes unfound control group member outcomes are roughly 1 SD greater than unfound treatment group member outcomes. All of the treatment effects in Table VII are robust 0.25 SD except for the 2-year durable asset impact. Nearly all of the effects are greater than zero for 0.5 SD, and hours in skilled trades remain robust.

VI.F. Non-economic impacts

Idle hands do the devil's work, 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 2012; World Bank 2010; World Bank 2007). We collected data on more than 50 self-reported measures of socio-political 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: (1) kin integration, capturing four measures of household relations; (2) community participation, capturing ten measures of associational life and collective action; (3) community public good contributions (2012 only) including seven types of goods; (4) anti-social behavior, based on eight forms of aggressive behavior with neighbors, community leaders, and police, plus 18 additional measures in 2012; and (5) protest attitudes and participation, based on 7 measures of participation in and attitudes around violent anti-government protests following the 2011 elections.[21]

Overall, we see little evidence of a positive social impact 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

[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.

estimates are typically less than 0.1 or 0.05 SD, and standard errors on these z-scores are equally small, suggesting we can rule out medium to large changes. Just two of the 27 regressions show a small, statistically significant impact, and both at the 2-year endline. We regard these as at best temporary effects and probably statistical anomalies.[22]

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 underemployed into non-agricultural 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

[22] First, treatment is associated with a 0.098 SD increase in community participation, but the treatment effect at four years is zero. Second, while we see no effect on anti-social behavior overall, when we disaggregate by gender we see an unusual pattern at two years: a 0.18 SD decline among men, and a 0.14 SD increase among women, both significant at least at the 10% level. The effect disappears at four years.

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 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.

Credit constraints. Several pieces of evidence suggest 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 impacts are also consistent with the predicted impacts 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 impact 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 indices 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 importantly, our sample is selective, and so may underrepresent the 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 males. 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 SD lower human capital and are

[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, and so we prefer to examine them together. The results are similar if we analyze each characteristic separately.

0.13 SD 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 to 50% we see could be sufficient to spur women to start enterprise even with lower initial abilities.

Studies of established female entrepreneurs have hypothesized that such differences in time preferences could account for low female 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 females report lower patience and self-control. But simple time inconsistency is insufficient to explain the takeoff 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 non-economic barriers, as discussed below. 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 males who do not face these same constraints.

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, risk is less likely to be the main constraint on our sample. First, initial levels of savings and non-enterprise 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).

[25] Since 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.

Social norms and social pressure. The fact that male's businesses and earnings grow in the absence of the program but female's do not could also point to some socially constructed constraint on women that a restricted cash transfer relieves. For example, Field et al. (2010) show that traditional norms against women's participation in business reduce the impact 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 be invested.

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 do have the 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 Appendix B.12, and find that groups with a better baseline working relationship have collectively higher outcomes, while 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 merit 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 impacts 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 that 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 that shows 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 their initial decisions. Nonetheless, our 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 it's 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 non-agricultural 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 the 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 a strong one.

VIII. References

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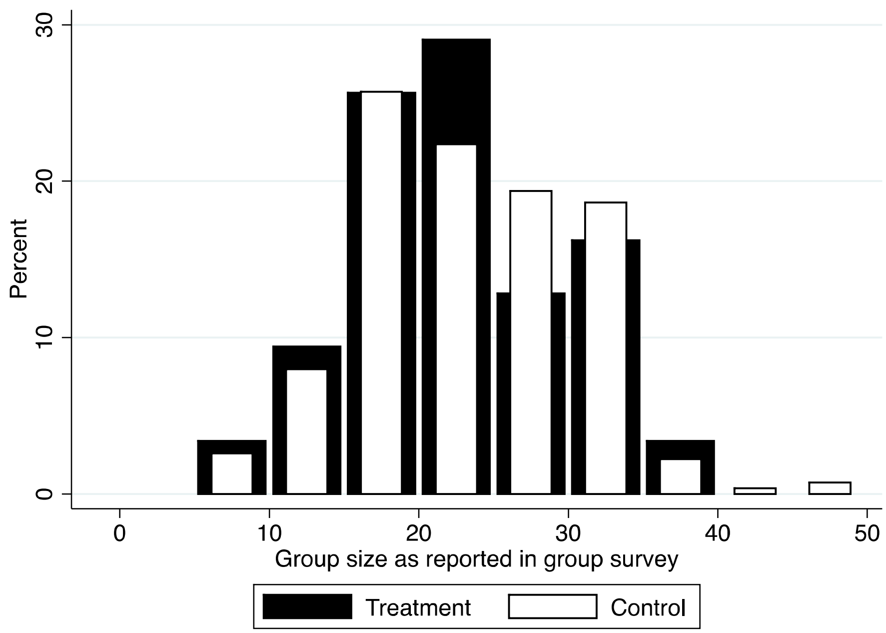
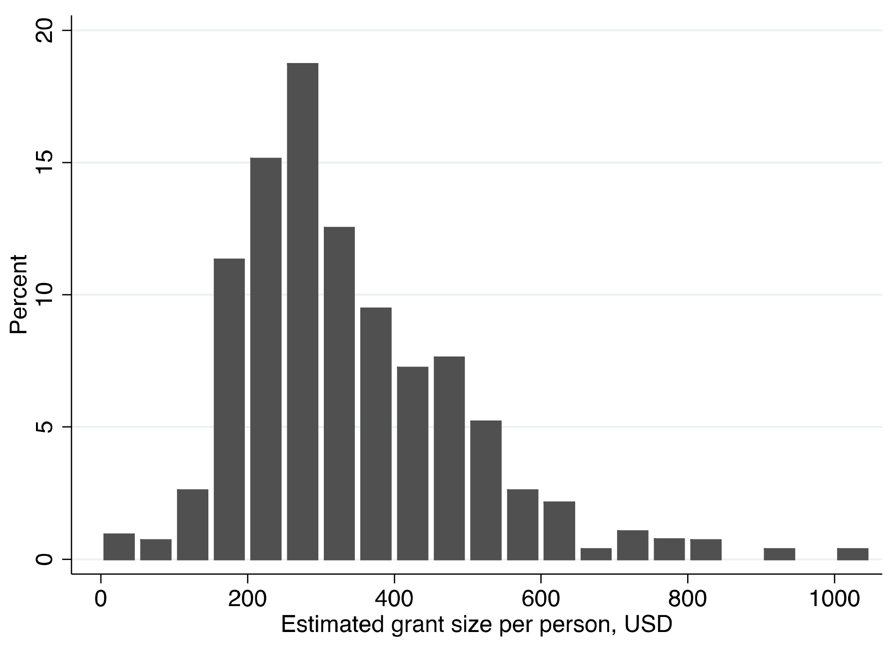
Figure I: Group and grant size

i. Distribution of group size



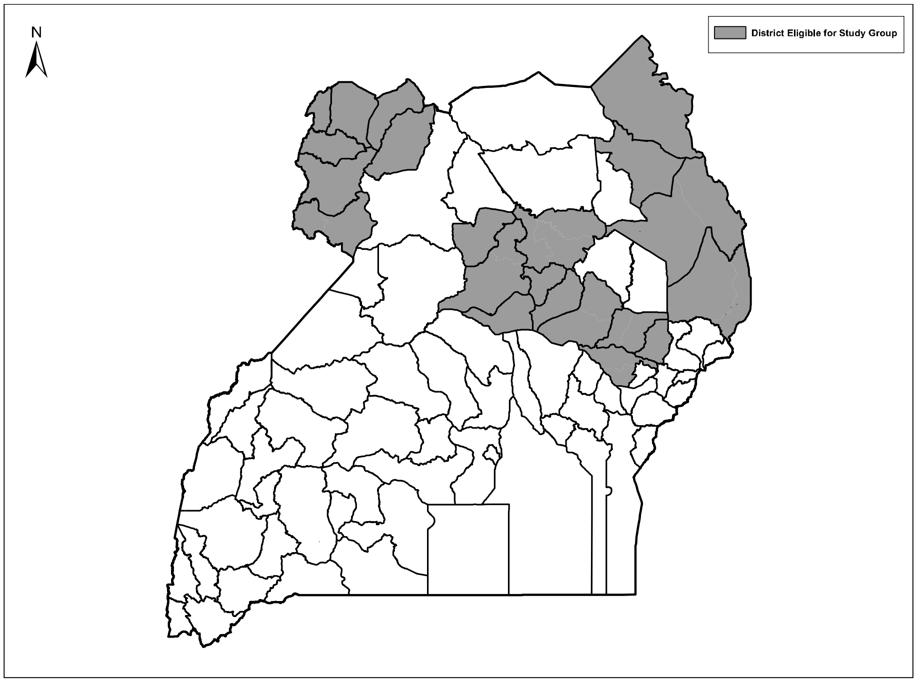
ii. Distribution of average grant size per person, treatment groups only



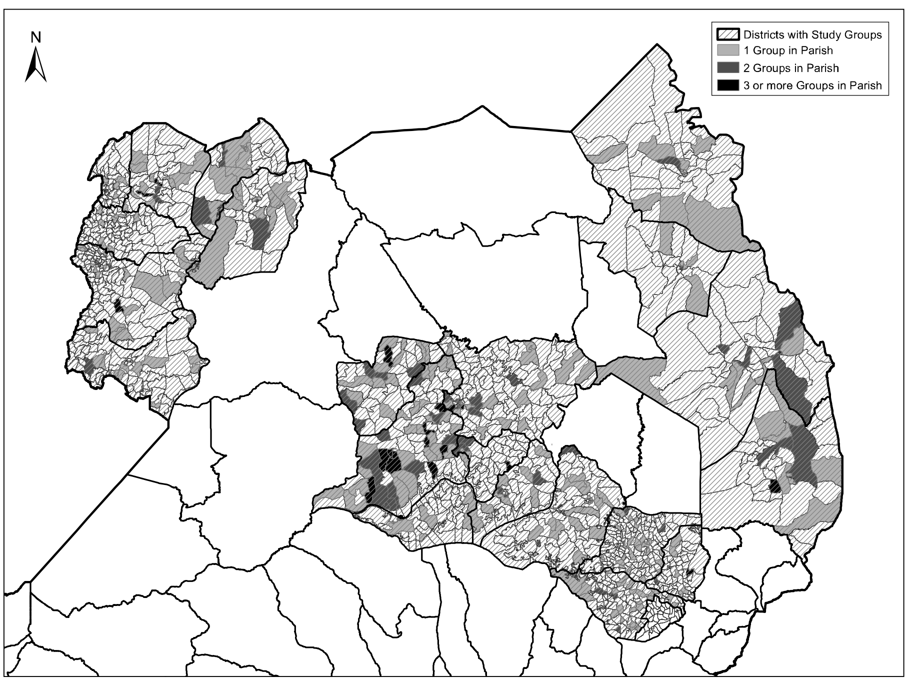
Notes: Estimated grant size does not include funds reported as not transferred or diverted by district officials. UGX-denominated grants are converted to dollars at the 2008 market exchange rate of 1,915 UGX per USD. The bin widths are (i) 5 years and (ii) $50.

Figure II: Location of study communities

i. Districts participating in the study (2007 boundaries)

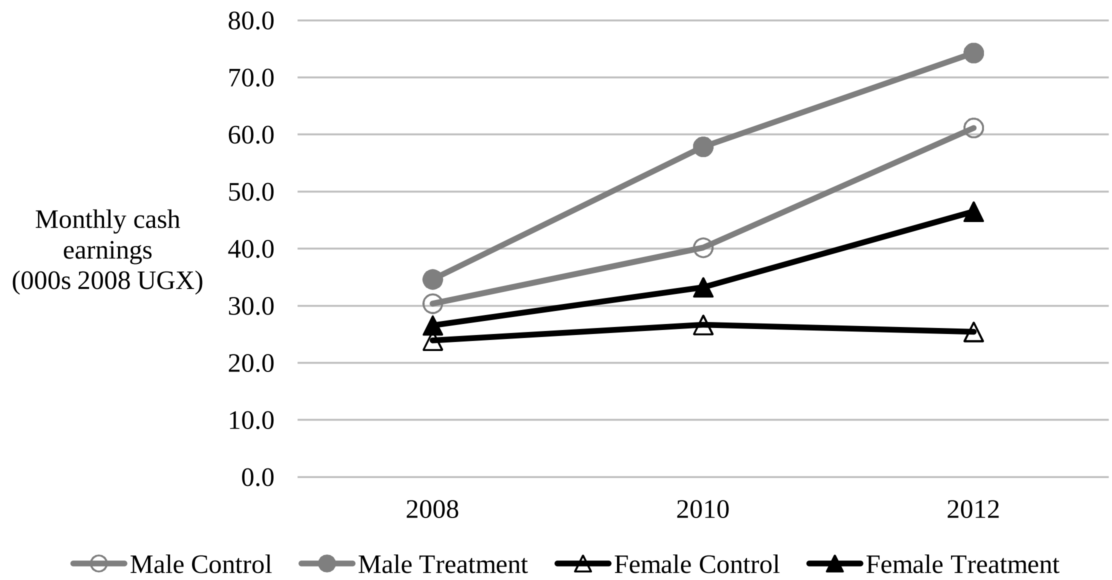


ii. Number of study communities (treatment and control) per parish



Notes: Panel (i) displays 2006 political boundaries (subdivided since 2003), with further subdivisions after 2006 marked by a white border line. In Panel (ii), 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 groups per parish, just six parishes/ have 4+ groups.

Figure III: Earnings trends, by treatment status and gender



Notes: 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 I: Survey response rates

	Selection and tracking, by survey phase					Effective response rates				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
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%	<0.001
2010 endline	2,677	63.4%	53.0%	74.7%	2,005	85.4%	85.6%	85.3%	-0.8%	0.717
2012 endline	2,677	61.0%	38.5%	58.6%	1,868	82.1%	79.1%	85.5%	7.1%	0.004

Notes: Column (1) reports the full study sample sought in each round--in general, five people per group over 535 groups, save for one groups 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 two 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 two 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 two receive zero weight. Column (10) reports p-value on the difference term, using robust standard errors clustered at the group level.

Table II: Pre-intervention descriptive statistics and test of balance

	Control		Treatment		Regression	
	(1)	(2)	(3)	(4)	(5)	(6)
Covariate in 2008 (baseline)	Mean	SD	Mean	SD	Mean	p-value
Grant amount applied for, USD	7,497	2,220	7,275	2,025	143.82	0.290
Group size	22.5	6.8	21.2	7.2	0.03	0.960
Grant amount per member, USD	363.1	159.4	381.7	170.9	14.09	0.250
Group existed before application	0.45	0.50	0.49	0.50	0.03	0.420
Group age, in years	3.8	2.0	3.8	1.9	-0.05	0.800
Within-group heterogeneity (z-score)	-0.03	0.92	0.03	1.06	-0.02	0.800
Quality of group dynamic (z-score)	-0.02	1.02	0.02	0.99	0.05	0.530
Distance to educational facilities (km)	6.84	6.50	7.26	5.71	0.48	0.350
Individual unfound at baseline	0.06	0.23	0.00	0.05	-0.05	0.000
Age at baseline	24.8	5.2	25.1	5.3	0.17	0.550
Female	0.35	0.48	0.32	0.47	-0.02	0.380
Large town/urban area	0.23	0.42	0.20	0.40	-0.02	0.610
Risk aversion index (z-score)	-0.02	1.00	-0.03	1.01	-0.01	0.750
Any leadership position in group	0.28	0.45	0.29	0.45	0.00	0.880
Group chair or vice-chair	0.11	0.31	0.12	0.32	0.01	0.330
Weekly employment, hours	10.7	15.8	11.4	15.5	0.55	0.490
All non-agricultural work	6.0	12.5	5.7	11.4	-0.45	0.440
Casual labor, low skill	1.0	5.2	1.1	5.0	-0.11	0.630
Petty business, low skill	2.2	7.0	2.4	6.8	0.21	0.520
Skilled trades	1.8	8.4	1.5	7.8	-0.33	0.400
High-skill wage labor	0.0	0.6	0.1	1.0	0.08	0.020
Other non-agricultural work	0.9	4.8	0.6	3.8	-0.29	0.100
All agricultural work	4.7	10.1	5.6	10.5	1.02	0.040
Weekly household chores, hours	9.0	17.6	8.7	16.1	0.30	0.730
Zero employment hours in past month	0.48	0.50	0.42	0.49	-0.04	0.180
Main occupation is non-agricultural	0.26	0.44	0.28	0.45	0.00	0.920
Engaged in a skilled trade	0.08	0.27	0.08	0.28	0.00	0.810
Currently in school	0.04	0.21	0.04	0.19	-0.01	0.450
Highest grade reached at school	8.0	2.9	7.8	3.0	-0.07	0.620
Able to read and write minimally	0.75	0.43	0.71	0.45	-0.03	0.170
Received prior vocational training	0.07	0.26	0.08	0.28	0.02	0.050
Digit recall test score	4.2	2.0	4.0	2.0	-0.04	0.640
Index of physical disability	8.7	2.5	8.6	2.2	-0.14	0.290
Durable assets (z-score)	-0.16	0.96	-0.07	1.05	0.07	0.120
Savings in past 6 mo. (000s 2008 UGX)	19.3	98.2	32.9	137.1	10.89	0.020
Monthly gross cash earnings (000s 2008 UGX)	62.2	129.0	67.7	135.2	6.9	0.300
Can obtain 100,000 UGX ($58) loan	0.33	0.47	0.40	0.49	0.05	0.010
Can obtain 1,000,000 UGX ($580) loan	0.10	0.30	0.12	0.32	0.01	0.460

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 is 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 OLS 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.

Table III: Descriptive statistics and intent-to-treat estimates of program impact on key outcomes

	2010 (2-year endline)				2012 (4-year endline)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control		ITT, with controls		Control		ITT, with controls	
	Mean	Obs	Coeff.	SE	Mean	Obs	Coeff.	SE
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]
Non-agricultural	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 ≥30 hours per week in a skilled trade	0.04	2,005	0.054	[0.013]***	0.03	1,868	0.037	[0.013]***
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]***
Non-durable consumption (z-score)	.	.	.	.	-0.011	1,862	0.180	[0.051]***

Notes: Columns (1) and (4) reportthe 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 (SE) 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table IV: Capital stock levels, changes, and intent-to-treat estimates of program impact by gender

	Dependent variable				
	Business assets (000s 2008 UGX)				
	(1)	(2)	(3)	(4)	(5)
	Mean		Change 2010-12		
Estimate	2010	2012	Δ	%Δ	SE
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			
SE	[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			
SE	[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			
SE	[91.1]*	[54.3]***			
Female - Male					
ITT, with controls	-324.4	-91.8			
SE	[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<0.01, ** p<0.05, * p<0.1

Table V: Intent-to-treat estimates of program impact on hired labor, 2012

	Percent reporting non-zero values		Mean		ITT, with controls	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Control	Treatment	Control	Treatment	Coefficient	SE
No. of paid and unpaid laborers hired in past month, family and non-family	65%	69%	2.9	3.7	0.636	[0.243]***
Non-agricultural 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]***
Non-agricultural 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 non-family	18%	23%	0.73	0.97	0.264	[0.148]*
Non-agricultural 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]
Non-agricultural 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]
Non-agricultural 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 zero, 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 99th percentile. From the 2010 survey, 49% of employees are family and 51% are non-family. Other data are not available on employees in 2010.

Table VI: Intent-to-treat estimates of program impact on income and poverty, by gender

	Dependent variable						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Monthly cash earnings (000s 2008 UGX)		Durable assets (z-score)		Non-durable consumption (z-score)	Subjective well-being, 1 to 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
SE	[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
SE	[5.51]	[7.207]**	[.073]	[.1]*	[.077]**	[.121]*	[.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
SE	[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<0.01, ** p<0.05, * p<0.1

Table VII: Sensitivity analysis of intent-to-treat estimates to alternate models and missing data scenarios

	Program impact under alternative models						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
					Impute missing dependent variable with mean + (-) X SD for missing control (treatment) indidivuals		
Dependent variable	ITT with controls	TOT with controls	ITT without controls	Diff-in-diff ITT with controls	0.25 SD	0.5 SD	"Worst case" Manski bound
Business assets							
2010	377.023	442.138	407.250	407.250	286.196	190.768	-730.579
SE	[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
SE	[62.601]***	[72.083]***	[68.404]***	[68.404]***	[53.163]**	[54.588]	[166.878]***
Skilled trade work (hrs)							
2010	4.703	5.394	4.551	4.763	3.911	3.24	-3.98
SE	[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
SE	[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
SE	[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
SE	[4.898]***	[5.560]***	[5.475]***	[5.911]***	[4.346]***	[4.489]	[10.356]***
Durable assets (z-score)							
2010	0.101	0.119	0.106	0.053	0.03	-0.027	-0.787
SE	[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
SE	[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) to (7), we re-estimate 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 SD 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<0.01, ** p<0.05, * p<0.1

Table VIII: Intent-to-treat effects on social outcome families

	Dependent variable: Family indices (z-scores)								
	Kin integration		Community participation		Community public good contributions	Anti-social behavior			Protest attitudes & participation
						Original		Extended	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	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
SE	[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
SE	[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
SE	[.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
SE	[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) to (9) report the intent-to-treat (ITT) estimate of the impact 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 socio-political baseline controls: baseline values of kin integration, community integration, anti-social behavior (self-reported aggession 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<0.01, ** p<0.05, * p<0.1

Table IX: Treatment heterogeneity by initial working capital, human capital, patience, and engagement in a skilled trade

	Dependent variable (2010 and 2012 endline data pooled)					
	Business assets (000s 2008 UGX)			Monthly cash earnings (000s 2008 UGX)		
	(1)	(2)	(3)	(4)	(5)	(6)
	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]**
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) to (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<0.01, ** p<0.05, * p<0.1

Table X: Male-female differences in the absence of the program

	Baseline, full sample		
	(1)	(2)	(3)
Dependent variable	Female coefficient	SE	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 non-zero 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.

Supplemental Online Appendix to "Generating Skilled Self-Employment in Developing Countries: Experimental Evidence from Uganda"

Christopher Blattman, Nathan Fiala, and Sebastian Martinez

Appendix A: Ramsey model of investment and occupational choice

Our conceptual framework is rooted in a simple Ramsey model of investment and occupational choice with heterogeneous indidivuals. We draw on a one sector model from Fafchamps et al (2012) and introduce occupational choice and labor supply.[1] We then consider a variety of constraints and utility functions.

1 Setup

Consider an individual who can spend time working in one of two sectors: enterprise or traditional labor. Production functions for enterprise and traditional labor are $f^E(k, l^E, \theta)$ and $f^T(l^T, \omega)$, where k is accumulated physical and human capital used in enterprise, l^E is hours spent on enterprise, l^T is hours on traditional labor, and θ is individual specific talent in enterprise, and ω is individual specific talent in traditional labor. Working in enterprise requires a minimum capital stock $\underline{k} \geq 0$, while traditional labor has no capital requirement. We assume positive but diminishing marginal returns to inputs, $f_k^E > 0 > f_{kk}^E$, $f_l^E > 0 > f_{ll}^E$; inputs are complements, $f_{kl}^E > 0$; and the returns to inputs are increasing in ability, $f_{k\theta}^E > 0$, $f_{l\theta}^E > 0$ and $f_{l\omega}^T > 0$.[2] Also, note that $l_t = l_t^E + l_t^T \in [0, 1]$.

[1] This model was developed by the authors in collaboration with Julian Jamison and Xing Xia for a suite of experimental studies of microenterprise and cash transfer interventions.

[2] We also assume the minimum capital requirement means that $f^E(k, l^E, \theta) \equiv 0$ as long as $k < \underline{k}$, and that for any ability level, at very low levels of k, marginal product of the first unit of labor is always higher in traditional labor than in enterprise, while at higher levels of k it is the opposite, $\underset{k \downarrow 0}{lim} \frac{f_l^E(k,0,\theta)}{f_l^T(0,\omega)} = 0$ and $\underset{k \uparrow +\infty}{lim} \frac{f_l^E(k,0,\theta)}{f_l^T(0,\omega)} = +\infty$. For simplicity, we assume $f^E(k, l^E, \theta)$ is homogeneous of degree 1 in (k, l^E).

The individual thus faces the problem:

$$\begin{aligned}
\underset{c_t>0,l_t\geq 0,k_{t+1}\geq 0,a_{t+1}}{max} & \quad \sum_{t=0} \delta^t u(c_t,l_t) \\
s.t. \;\; c_t+a_{t+1}+k_{t+1} &= (1+r_t)a_t+k_t+f^E(k_t,l_t^E,\theta)+f^T(l_t^T,\omega) \\
l_t &= l_t^E+l_t^T\leq 1 \\
k_0 &= 0 \\
a_0 & \quad given
\end{aligned}$$

where a_t is any financial assets other than capital invested in enterprise and r_t is the returns to these alternative financial assets at time t. a_t is any financial assets other than capital invested in enterprise and r_t is the returns to these alternative financial assets at time t. Without loss of generality, we assume $k_0=0$ and all initial wealth is in the financial asset, a_0. To make analysis simple, we fix $r_t=r>0$. Finally, to fully characterize the equilibrium we add a transversality condition: $\underset{t\to\infty}{lim}\delta^t u_c'(c_t,l_t)a_t=0$.

This benchmark case considers perfect financial markets and consistent time preferences. In this case, individuals will allocate assets between the enterprise and savings until the returns of capital are equal, and will allocate their time across sectors until the marginal disutility is equal. The solution to the problem is characterized as time-paths of quantities $\{c_t,l_t^E,l_t^T,k_{t+1},a_{t+1}\}_{t=0}^{\infty}$ that satisfy the following set of conditions given $k_0=0$ and $a_0>0$:

$$\begin{aligned}
\frac{u_c'(c_t,l_t)}{u_c'(c_{t+1},l_{t+1})} &= \delta(1+r) && (1)\\
-\frac{u_l'(c_t,l_t)}{u_c'(c_t,l_t)} &= f_l^{E'}(k_t,l_t^E,\theta) \quad if\ l_t^E>0 && (2)\\
-\frac{u_l'(c_t,l_t)}{u_c'(c_t,l_t)} &= f_l^{T'}(l_t^T,\omega) \quad if\ l_t^T>0 && (3)\\
\frac{u_c'(c_t,l_t)}{u_c'(c_{t+1},l_{t+1})} &= \delta(1+f_k^{E'}(k_{t+1},l_{t+1}^E,\theta)) \quad if\ k_{t+1}>0 && (4)\\
c_t+a_{t+1}+k_{t+1} &= (1+r)a_t+k_t+f^E(k_t,l_t^E,\theta)+f^T(l_t^T,\omega) && (5)\\
\underset{t\to\infty}{lim}\delta^t u_c'(c_t,l_t)a_t &= 0 && (6)
\end{aligned}$$

Conditions 1 and 4 imply that whenever investment in enterprise is positive the individual always produces at efficient scale, i.e. $f_k^{E'}(k_{t+1},l_{t+1}^E,\theta)=r$. For simplicity, we focus on interior solutions only throughout.

Who runs an enterprise?

For $\underline{k} > 0$, there will be low θ types who cannot reach efficient scale because their returns to capital are lower than r. We can define a minimum ability before enterprise is feasible, $\underline{\theta} = \underline{\theta}(r, \underline{k})$ for $f_k^{E'}(\underline{k}, 1, \underline{\theta}) = r$.[3] Note that $\underline{\theta}$ does not depend on a_0.

As θ rises above $\underline{\theta}$, the returns to capital and labor increase in enterprise, and enterprise becomes a better alternative than saving all assets in a_t. This does not guarantee that the individual will invest, since time could be allocated instead to traditional labor. Not surprisingly, those with high values of ω and low values of θ will only engage in traditional labor. Specifically, there will be a second threshold, $\tilde{\theta}$, above which individuals will invest in enterprise if $\theta > \underline{\theta}$ is also satisfied. $\tilde{\theta}$ is a function of the relative marginal products of labor. The marginal product of labor in enterprise, $MPL^E(r, \theta)$, is decreasing in r and increasing in θ.[4] In the traditional sector, $MPL^T(\omega, a_0, r)$ is determined by the equilibrium level of l^T, and is increasing in ω, a_o and r.[5] For high enough ω, $MPL^T(\omega, a_0, r) \geq MPL^E(r, \theta)$, and these individuals will engage only in traditional labor and save all their assets in a_t. $MPL^T(\omega, a_0, r) = MPL^E(r, \theta)$ defines a threshold level of $\tilde{\theta} = \tilde{\theta}(\omega, r, a_o)$ where $\theta > \tilde{\theta}$ if and only if $MPL^T(\omega, a_0, r) < MPL^E(r, \theta)$. $\tilde{\theta}(\omega, r, a_0)$ is increasing in all three arguments r, ω and a_o. However, the effect of a_o on $\tilde{\theta}$ will be negligible and so we simplify to $\tilde{\theta}(\omega, r)$.

Finally, in the steady state[6], occupational choice is determined by the threshold $\theta^* = \theta^*(r, \underline{k}, \omega) = max\{\underline{\theta}(r, \underline{k}), \tilde{\theta}(\omega, r)\}$. Low ability individuals whose $\theta < \theta^*(r, \underline{k}, \omega)$ will set $k = 0$ and $l^E = 0$. c and l^T will be determined by a_o, r and w. High ability individuals

[3]Since hours working in the enterprise are upward bounded by 1 while capital invested in skilled trade must be higher than $\underline{k}$, for any individual that invests in the enterprise, $\frac{k}{l^E}$ must be higher than $\underline{k}$. Then for any $\theta < \underline{\theta}$, $k \geq \underline{k}$, and $l^s < 1$, $f_k^{E'}(k, l^E, \theta) < f_k^{E'}(\underline{k}, 1, \underline{\theta}) = r$. The inequality arises because $f_k^{E'}$ is decreasing in k but increasing in l^E and θ. Therefore, for individuals with $\theta < \underline{\theta}$, their returns to capital in enterprise is below r regardless of the level of l^E and k. Note that $\underline{\theta}$ is an increasing function of $\underline{k}$, r and other parameters in the production function f^E.

[4]If there is positive investment in enterprise, condition $f_k^{E'}(k, l^E, \theta) = r$ pins down the level of $\frac{k}{l^E}$ (this is because we assumed f^E is homogenous of degree one in (k, l^E)). $\frac{k}{l^E}$ then pins down the marginal product of labor on the right hand side of condition 3: $-\frac{u_l'(c_t, l_t)}{u_c'(c_t, l_t)} = f_l^{E'}(k_t, l_t, \theta)$.

[5]If an individual does not invest in enterprise and only engages in traditional labor, conditions 1,3,5 and 6 will pin down a level of l^T.

[6]Note that we cannot have growth on the steady state because total hours available to the individual is 1, and we assume there is no exogenous growth in productivity or individual ability. Therefore, we characterize a steady state where c_t, k_t, l_t and a_t are all constant. From optimal condition 1, we can see that, without any restraints on savings or borrowing, the existence of a steady state requires $\delta(1 + r) = 1$, otherwise we cannot keep consumption constant. Notice that this is because we assumed there is free lending and free borrowing, both at the same rate r. Patient individuals whose $\delta > \frac{1}{1+r}$ would over save and accumulate infinite wealth when $t \to \infty$; impatient individuals whose $\delta < \frac{1}{1+r}$ would borrow too much today and their assets would approach negative infinity as $t \to \infty$. In both of these cases, the transversality condition would be violated. While this condition $\delta(1 + r) = 1$ seems restrictive, we could argue that in reality, there will bounds for borrowing and savings. As long as there is lending and borrowing within some bounds at the rate r, our results would hold. We do not need $\delta(1 + r) = 1$ for our comparative analysis.

whose $\theta \geq \theta^*$ will invest in enterprise and set $k^* > 0$ and $l^{E*} > 0$ such that $f_k^{E'}(k^*, l^{E*}, \theta) = r$ and $-\frac{u_l'(c^*,l^*)}{u_c'(c^*,l^*)} = f_l^{E'}(k^*, l^{E*}, \theta) = f_l^{T'}(l^{T*}, \omega)$. Their returns to capital will be r regardless of their level of wealth. The ratio $\frac{k^*}{l^{E*}}$ will be determined by θ and r, while hours in traditional labor l^{T*} will be determined by w and r. Initial wealth a_o will have a small effect on the level of k^*, l^{E*}, c and a. Without any constraints, all individuals will immediately jump to these efficient steady state levels of production and consumption at time $t = 1$.

Impact of a windfall

With perfect financial markets, an unrestricted windfall will have no effect on entry into enterprise and little effect on earnings because it does not change θ, ω or r. The individual will immediate jump to a new steady state with higher level of savings a and consumption c, and will slightly reduce investment k and hours in enterprise l^E because of greater wealth.

Suppose instead the windfall is granted in the form of in-kind transfers or restricted funding and there is some minimal "flypaper effect" such that capital stocks are "sticky" and cannot be divested immediately. This "restricted windfall" will force individuals to produce above their efficient scale, $f_k^{E'}(k, l, \theta) < r$. c will increase as output increases in the enterprise. l^T will decrease and l^E could go either direction, depending on parameter values. Over time, if it is possible to shift capital to a, individuals will divest until the returns in enterprise drops back to r.

2 Cash windfalls in imperfect financial markets

2.1 Credit constraint $a_t \geq 0$

For simplicity, we consider an extreme credit constraint, $a_t \geq 0$. The intuition and comparative statics are similar for other less restrictive credit constraints. The credit constraint affects optimality conditions 1, which becomes:

$$\frac{u_c'(c_t, l_t)}{u_c'(c_{t+1}, l_{t+1})} \geq \delta(1 + r) \quad with\ equality\ \ if \quad a_{t+1} > 0 \tag{7}$$

Initially wealthy entrepreneurs tend to operate at efficient scale, with marginal returns equal to r. The less wealthy, more impatient and higher ability do not have savings, will satisfy 7 with equality, and will invest below efficient scale with marginal returns are higher than r.

Credit constraints also change the steady state level of investments, returns to investments, and threshold θ^*. Define $\rho = \frac{1-\delta}{\delta}$, where a high level of ρ indicates impatience. Define k^{**} and l^{E**} such that $f_k^{s'}(k^{**}, l^{E**}, \theta) = \rho$. For impatient individuals whose $\rho > r$, the steady state level of capital and hours in the enterprise would be $k^{**} < k^*$ and l^{E**}, and their returns to capital will be ρ. These individuals are investing below the efficient scale. For those whose $\rho < r$, the steady state level of returns will still be r and investments will still be k^* as before. To sum up, the steady state returns to capital will be $max\{r, \rho\}$. For simplicity we will still refer to the threshold as θ^*, while here $\theta^* = \theta^*(r, \underline{k}, \omega, \rho)$ and θ^* is increasing in ρ whenever $\rho > r$. This means that with a credit constraint, more impatient individuals will find enterprise undesirable than in the benchmark case.

Not surprisingly, with a credit constraint, not all whose $\theta > \theta^*$ will immediately engage in enterprise. Specifically, if there is a credit constraint, $a_t \geq 0$, then compared to the benchmark case with no constraints at all then occupational choice and investment will vary by type and initial wealth in the following manner:

1. Low ability individuals, $\theta < \theta^*$. A credit constraint will not change occupational choice, consumption or labor supply as they would not invest in the enterprise even if they are allowed to borrow.

2. High ability and High wealth, $\theta \geq \theta^*$ and $a_0 \geq k^{**}$. A credit constraint will not change occupational choice. However, investment levels and returns will depend on time preferences:

   (a) Patient types ($\rho < r$) will invest k^*, with marginal returns r.

   (b) Impatient types ($\rho > r$) will invest $k^{**} < k^*$, with marginal returns ρ.

3. High ability and below steady state wealth ($\theta \geq \theta^*$ and $\underline{k} \leq a_0 \leq k^{**}$). A credit constraint will reduce initial investment in enterprise only. They will start with an enterprise below efficient scale and over time will accumulate enough capital to reach the steady state level of investment.

4. High ability and below minimum scale wealth ($\theta \geq \theta^*$ and $a_0 \leq \underline{k}$). A credit constraint will change initial occupational choice, but whether this effect is long-term or not depends on a_0, δ and abilities in each sector:

   (a) if a_0 is close to $\underline{k}$ or ω is very high, and δ is close to one, the individual would be able to save in the initial periods and eventually accumulate enough wealth to invest in enterprise. In this case, the credit constraint only temporarily alters the individual's occupational choice. Individuals will not invest in enterprise until a_{t+1} is above $\underline{k}$, after which they start investing in enterprise and reach the steady state level of investment over time.

(b) if a_0 is far below $\underline{k}$ or ω is very low, and δ is close to zero, the individual would remain in traditional labor forever. In this case, the credit constraint has a permanent effect on the individual's occupational choice.

Impact of a windfall

We first consider an unrestricted cash windfall. In cases 1 and 2(a), individuals are in their optimal steady state and the windfall will increase consumption and savings, and slightly reduce labor supplied, but will not affect entry into enterprise or earnings. In case 2(b), individuals will increase investments in enterprise temporarily above k^{**}, increase total earnings but reduce marginal returns to capital to a level below ρ but not lower than r; over time they will reduce capital in the enterprise until capital returns in the enterprise rise up to ρ again. Consumption will rise in the long run, but savings will be zero in the long run. In case 3, the windfall will immediately increase their investments in enterprise and earnings, and they will continue to increase capital and earnings over time until they reach k^*. Likewise, in case 4, individuals will start and sustain an enterprise if the windfall is enough to cover the capital requirement $\underline{k}$. For those with extremely low level of initial wealth $a_0 < \underline{k} - M$, the windfall will not immediately affect their involvement in enterprises, but it does increase the chances of their engaging in enterprise in the long run. Whether they will eventually engage in the enterprise will again depend on their patience and productivity in traditional labor.

Next we consider a restricted windfall with some flypaper effect. In cases 1 and 2, individuals are in their optimal steady state and the results are the same as in the case of perfect financial markets: they will be forced to invest above efficient scale in the short run, earnings will increase, but returns will be low. In the long-run, they will divest and go back to the their steady state level of production, merely saving and consuming divested funds. In cases 3 and 4, individuals are below steady state and the impact will be similar to the case of the unrestricted windfall.

2.2 Savings constraint $a_t \leq 0$

Now we consider the case of a savings constraint where individuals do not have any alternative means to invest other than enterprise. They are, however, still allowed to borrow at rate r. Condition 1 now becomes

$$\frac{u'_c(c_t, l_t)}{u'_c(c_{t+1}, l_{t+1})} \leq \delta(1+r) \quad \textit{with equality} \;\; \textit{if} \;\; a_{t+1} < 0. \tag{8}$$

Savings constraints can lead to investment above the efficient scale. For those with debts $a_{t+1} < 0$ (the impatient and poor ones), the first order conditions require their returns to capital to be the same as r; however, for those without debts $a_{t+1} = 0$ (the patient and savings constrained ones), these conditions mean marginal returns are lower than r.

Among those who do invest in the enterprise, for the patient individuals whose $\rho < r$, the steady state level of capital and hours are k^{**} and l^{E**}, and their returns will be ρ. Notice $k^{**}/l^{E**} > k^{*}/l^{E*}$. For those impatient ones whose $\rho > r$, steady state returns are still r and investments are still k^* as before. Thus the steady state returns to capital are $min\{r, \rho\}$. Because individuals are still allowed to borrow, any individual with $\theta \geq \theta^*$ would invest in enterprise, though this θ^* is lower than in the benchmark and credit constraint cases for patient individuals whose $\rho < r$.[7] Thus, under a savings constraint, more people run an enterprise at any $t > 0$, before and after everyone reaches their steady state. However, the average rate of returns among entrepreneurs will be lower than r.

Impact of a windfall

An unrestricted windfall will not change any individual's decision to engage in enterprise, since all those with $\theta \geq \theta^*$ will be already engage in enterprise at the outset. However, in the short run, since individuals cannot save, the windfall will increase consumption and capital stocks, and thus further reduce the marginal returns to capital in the enterprise below $min\{r, \rho\}$. In the long run, however, capital and consumption will drop back to the steady state level and rate of returns will rise back to $min\{r, \rho\}$.

A restricted cash transfer with a flypaper effect will immediately increase capital stocks and lower the rate of return while having no immediate impact on consumption. Over time, the individual will consume these transfers until consumption and capital stock falls back to the steady state level. The average impact on earnings will not be as high as under a credit constraint.

2.3 Savings and credit constraints $a_t = 0$

Finally we consider the effect of a savings constraint on top of a credit constraint. For those who do invest in the enterprise, their rate of returns will be $f_k^{E'}(k, l^E, \theta) = \frac{1-\delta}{\delta} = \rho$. This means the less patient will be investing below the efficient scale while the more patient will be

[7] Savings constraints will lower the threshold level of θ^* for those whose $\rho < r$. This is because now we would need to define θ^* based on the new level of returns to capital $min\{r, \rho\}$ instead of r. For simplicity of discussion, we will still refer to the threshold as θ^*, while here $\theta^* = \theta^*(r, \underline{k}, \omega, \rho)$ and θ^* is increasing in ρ whenever $\rho < r$. This means that with a credit constraint, more individuals will be engaging in enterprise than in the benchmark case.

investing above the efficient scale. This also changes the threshold level θ^* for all individuals. We would need to define θ^* using $f_k^{E'}(k, l^E, \theta) = \rho$ instead of r. For impatient ones whose $\rho > r$, θ^* would be higher than in the benchmark case; while for patient ones whose $r > \rho$, θ^* would be lower than in the benchmark case. This means, compared to the benchmark case, there will be more patient individuals and less impatient ones investing in enterprise. Individuals with $\theta < \theta^*$ (case 1 above) will be engaging in traditional labor only, as are those with high ability and below minimum scale wealth (case 4(a) and (b)).

Cash windfalls, restricted or unrestricted, will be invested in all cases.[8] Those at or near their optimal steady state level of capital (including no enterprise) will have an average return below $min\{r, \rho\}$, and those below their steady state will have average returns higher than this level.

2.4 Risk and missing insurance markets

Next we consider the case of risky enterprise and risky traditional labor but a riskless financial alternative. It is possible to model risk in several ways. To incorporate uncertainty, we illustrate the case where the productivity measures θ_t and ω_t are uncertain and vary over time.

Specifically, we assume that realizations of ability are normally distributed around average expected productivity $\bar{\theta}$ and $\bar{\omega}$, $\theta_t \sim N(\bar{\theta}, \delta_\theta)$ and $\omega_t \sim N(\bar{\omega}, \delta_\omega)$. Hours in enterprise and traditional labor are determined after the realization of θ_t and ω_t. Investment decisions k_t and a_t, however, are made in time $t-1$, before the realization of productivity θ_t and ω_t. We can view the individual as having a stochastic income stream delivered by the stochastic wage from traditional labor. At the same time, the individual has the option of investing his asset in either the risky enterprise with expected return $\mathbb{E}_t\left(1 + f_k^{E'}(k_{t+1}, l_{t+1}^E, \theta)\right)$ or the riskless asset with return $1 + r$.

The solution to the problem is characterized as time-paths of quantities $\{c_t, l_t^E, l_t^T, k_{t+1}, a_{t+1}\}_{t=0}^{\infty}$ that satisfy the following set of conditions for all time periods t and for all states of the world

[8] The sole exception is the very poor with initial wealth $a_0 < \underline{k} - M$. They will no longer pass a point where they have incentives to save in order to accumulate $\underline{k}$. This is a moot point if $M > \underline{k}$.

at time t:

$$
\begin{aligned}
\mathbb{E}_t\left[\frac{\delta u_c'(c_{t+1},l_{t+1})}{u_c'(c_t,l_t)}(1+r)\right] &= 1 && (9)\\
-\frac{u_l'(c_t,l_t)}{u_c'(c_t,l_t)} &= f_l^{E'}(k_t,l_t^E,\theta_t) \quad if\ l_t^E>0 && (10)\\
-\frac{u_l'(c_t,l_t)}{u_c'(c_t,l_t)} &= f_l^{T'}(l_t^T,\omega_t) \quad if\ l_t^T>0 && (11)\\
\mathbb{E}_t\left[\frac{\delta u_c'(c_{t+1},l_{t+1})}{u_c'(c_t,l_t)}\left(1+f_k^E(k_{t+1},l_{t+1}^E,\theta_t)\right)\right] &= 1 \quad if\ k_{t+1}>0 && (12)\\
(1+r)a_t+k_t+f^E(k_t,l_t^E,\theta_t)+f^T(l_t^T,\omega_t) &= c_t+a_{t+1}+k_{t+1} && (13)\\
\lim_{j\to\infty}\mathbb{E}_t\beta^j u_c'(c_{t+j},l_{t+j})a_{t+j} &= 0 && (14)
\end{aligned}
$$

given $k_0=0$ and $a_0>0$.

Following the asset pricing literature, we define $M_t=\frac{\delta u_c'(c_{t+1},l_{t+1})}{u_c'(c_t,l_t)}$ as the stochastic discount factor. Condition 9 and 12 imply that investment in the enterprise, if positive, must satisfy the usual asset pricing equation:

$$\mathbb{E}_t f_k^{E'}(k_{t+1},l_{t+1}^E,\theta_t)-r=-(1+r)Cov_t\left(f_k^{E'}(k_{t+1},l_{t+1}^E,\theta_t),M_{t+1}\right)$$

Risk neutral individuals will invest until $\mathbb{E}f_k^{E'}(k,l^E,\theta)=r$. As in the case without risk, we refer to the level of investment that corresponds to $\mathbb{E}f_k^{E'}(k,l^E,\theta)=r$ as the efficient scale of investment.

For any risk averse individual, if θ_{t+1} and ω_{t+1} are *positively* correlated or uncorrelated, then $Cov_t\left(f_k^{E'}(k_{t+1},l_{t+1}^E,\theta_t),M_{t+1}\right)<0$ and $\mathbb{E}_t f_k^{E'}(k_{t+1},l_{t+1}^E,\theta_t)>r$. This is saying that if the returns to enterprise and traditional labor are *positively* correlated, then the riskless asset will deliver higher expected utility than the risky enterprise, and the individual will invest below the efficient scale in the enterprise as long as he is risk averse. In this case, the more risk averse the individual is, the less he invests in risky enterprise, and the higher the returns to the enterprise. Similarly, the higher the variability of θ or w, the less the individual invests in risky enterprise,

If instead, θ_{t+1} and ω_{t+1} are *negatively* correlated, i.e. the returns to enterprise and traditional labor are *negatively* correlated, then the enterprise and traditional labor are a good hedge against each other. The individual will invest more in the enterprise, or even invest above the efficient scale. The returns to enterprise $\mathbb{E}_t f_k^{E'}(k_{t+1},l_{t+1}^{sE},\theta_t)$ will be close to r, or even lower than r if the variability of ω is high.

Here the optimal level of investment is a function of interest rate r, the mean and variance of productivity $\bar{\theta}$, δ_θ, $\bar{\omega}$, δ_ω, the correlation between θ and ω, patience δ and the degree of risk aversion. The optimal choice of whether to invest in enterprise or not then depends on all these parameters, as well as the minimum scale of production $\underline{k}$.

Impact of a windfall

We ask the same question as before: Under what conditions will the cash windfall have a sustained effect on individuals' investment in enterprise and/or returns to investment in enterprise?

Even absent a credit constraint a windfall may induce some individuals to enter into enterprise, and change investment levels for those who do invest in enterprise, simply through the wealth effect. If utility displays constant absolute risk aversion (CARA) wealth would not have any effect on the optimal level of investment, and no effect on entry into enterprise. If, however, utility displays constant relative risk aversion (CRRA) then a windfall increases the level of wealth, which then increases the optimal level of investment in the risky enterprise.

Note that there will be individuals with either very low initial wealth, or very high risk aversion, who would not invest in the risky enterprise (because of $\underline{k}$) but would do so after receiving the cash windfall. Unless the amount of the windfall is very large (relative of the minimum scale $\underline{k}$) or the individual is very risk averse, we would not expect the windfall to have a large long-run average effect on investment across many indidivuals.

So long as both sectors are risky, for a windfall to result in high levels of investment and high returns, there must be some other form of imperfection on top of an environment with risk. Again, a credit constraint is a likely candidate in the setting described. This conclusion rests on the assumption that there are roughly similar levels of uncertainty in the two sectors. We turn to that assumption next.

Relative uncertainty

Intuitively, the relative volatility of traditional trade and enterprise matter for investments in enterprise. More importantly, the impact of their relative volatility depends on initial wealth, the degree of risk aversion, as well as the correlation between enterprise and traditional labor.

In general terms, if either enterprise or traditional labor is relative safe (i.e. either σ_θ or σ_ω is low), then investment in enterprise k falls as $\sigma_\theta/\sigma_\omega$ increases; and the more risk averse the individual is, the steeper the slope of the fall is. If σ_θ is low while σ_ω is high, the individual will very likely engage in the enterprise, as long as she is not bounded by a credit constraint. If σ_θ is high while σ_ω is low, the invidiual will likely not engage in the enterprise. In both cases, a windfall will have little impact on investments and earnings.

If, however, productivity in traditional labor and enterprise are both very volatile (σ_θ and σ_ω both high), then the relationship between k and $\sigma_\theta/\sigma_\omega$ would also depend on initial wealth a_0, the degree of risk aversion, as well as the correlation between θ and ω. First, consider the case where traditional labor and enterprise are uncorrelated or positively correlated. Holding everything else constant, if an individual has very low (or negative) initial wealth, then given a highly volatile income stream from traditional labor, the safety asset would be much more appealing to her than the enterprise even if the enterprise is less volatile than traditional labor. In this case, the individual may not enter into enterprise even if she faces no credit constraint – she might fear that she would never be able to repay the debt with her earnings. The same happens if the individual is very risk averse – she would not enter the enterprise and instead use the safety asset to smooth consumption over time. In both of these cases, a large windfall might pull the individual out of these sitations and allow her to invest in the enterprise. However, if the returns from traditional labor and enterprise are negatively correlated, then again k increases as $\sigma_\theta/\sigma_\omega$ falls, and the individual will likely invest in the enterprise as long as she is not bounded by a credit constraint. In this case, a windfall will have a long term effect on those with high levels of risk aversion and low levels of initial wealth . Again, this is because a windfall increases wealth and lead the risk averse to invest more in risky assets – the enterprise.

3 Cash windfalls and time-inconsistency

We introduce quasi-hyperbolic (β, δ) preferences to see what predictions they hold for investment and earnings. The problem becomes:

$$\begin{array}{rrcl} & \underset{c_t>0, l_t\geq 0, k_{t+1}\geq 0, a_{t+1}}{max} & & u(c_t, l_t) + \beta \sum_{s=t+1}^{\infty} \delta^s u(c_s, l_s) \\ s.t. & c_t + a_{t+1} + k_{t+1} & = & W_t \\ & l_t & = & l_t^E + l_t^T \leq 1 \\ & W_t & \equiv & (1+r_t)a_t + k_t + f^E(k_t, l_t^E, \theta) + f^T(l_t^T, \omega) \end{array}$$

We consider the case of a "naive" type, or "naif", who makes investment decisions under the false belief that future selves will act in the interest of the current self, and a "sophisticate" who knows exactly what her future selves' preferences will be.

3.1 Perfect financial markets

Optimal conditions 1 and 4 will now change into the general Euler equation for hyperbolic preferences:

$$\frac{u_c'(c_t, l_t)}{u_c'(c_{t+1}^P, l_{t+1})} = [\frac{\partial c_{t+1}}{\partial W_{t+1}}\beta\delta + (1 - \frac{\partial c_{t+1}}{\partial W_{t+1}})\delta] \cdot (1 + r) \tag{15}$$

and

$$\frac{u_c'(c_t, l_t)}{u_c'(c_{t+1}^P, l_{t+1})} = [\frac{\partial c_{t+1}}{\partial W_{t+1}}\beta\delta + (1 - \frac{\partial c_{t+1}}{\partial W_{t+1}})\delta] \cdot (1 + f_k^{E'}(k_{t+1}, l_{t+1}^E, \theta)) \quad if\ k_{t+1} > 0 \tag{16}$$

These resemble the Euler equations 1 and 4 under exponential discounting, except that the discount factor δ is replaced by the effective discount factor $\frac{\partial c_{t+1}}{\partial W_{t+1}}\beta\delta + (1 - \frac{\partial c_{t+1}}{\partial W_{t+1}})\delta$, a weighted average of the short-run and long-run discount factors $\beta\delta$ and δ where the weights are the next period marginal propensity to consume out of total wealth. Here W_t denotes total wealth at time t. c_{t+1}^P denotes the individual's predicted future decision about c_{t+1} at time t.

The differences between the naif and the sophisticate lie in the predicted consumption c_{t+1}^P and the marginal propensity to consume $\frac{\partial c_{t+1}}{\partial W_{t+1}}$. Sophisticates are aware of the time-inconsistency problem and will correctly anticipate future consumption. For them, $c_{t+1}^P = c_{t+1}$. Naifs, however, mistakenly believe that future selves will act as if their discount factor remains unchanged at all future dates. For them $c_{t+1}^P < c_{t+1}$. Time-inconsistency will affect both consumption and savings.

Time-inconsistency should not affect the optimal use of a cash windfall. For those with $\theta \geq \theta^*$, they will still invest until the returns to capital are equal between the enterprise and alternative financial options, or $f_k^{E'}(k, l^E, \theta) = r$. Note that consumption, hours and savings will all be different under time-inconsistency compared to our benchmark case without time-inconsistency. Thus threshold value of θ^* is different than in the benchmark case. However, the effect of a windfall will be similar to that in the benchmark case without time-inconsistency. This is because absent of any credit market imperfections, everyone will already be at their efficient scale.

3.2 Time-inconsistency with credit constraints

For a windfall to be invested and produce high average returns, some other constraint must be present. Similar to the case without time-inconsistency, credit constraints will suffice. To see this, we turn to the Euler equations again. Those who are credit constrained will put every additional dollar they get into consumption (not savings), because they are present-biased. Therefore $\frac{\partial c_{t+1}}{\partial W_{t+1}} = 1$ and the Euler equations become

$$\frac{u_c'(c_t, l_t)}{u_c'(c_{t+1}^P, l_{t+1})} = \beta\delta(1 + f_k^{E'}(k_{t+1}, l_{t+1}^E, \theta)) \quad if\ k_{t+1} > 0$$

for those who are bounded by the credit constraint, i.e. $a_{t+1} = 0$.

With time inconsistency, all credit constrained individuals will invest less than if they were time-consistent. To see this, define τ such that $\frac{1}{1+\tau} = \beta\delta$, i.e. $\tau = \frac{1}{\beta\delta} - 1$. Since the sophisticates can correctly anticipate their future consumptions, in their steady state $c_{t+1}^P = c_{t+1} = c_t$, and the marginal rate of return will be $f_k^{E'}(k_{sophisticate}, l^E, \theta) = \tau$. Naifs will naively expect themselves to have more self-control tomorrow, and expect $c_{t+1}^P < c_t$. For them $\frac{u_c'(c_t, l_t)}{u_c'(c_{t+1}^P, l_{t+1})} < 1$ and $\rho < f_k^{E'}(k_{naive}, l^E, \theta) < \tau$. Therefore, for those who are credit constrained ($a_{t+1} = 0$), their steady state level of investment satisfies $\rho < f_k^{E'}(k_{naive}, l^E, \theta) < \tau = f_k^{E'}(k_{sophisticate}, l^E, \theta)$. They also work less and consume a larger portion of their income.

Somewhat counter-intuitively, given the levels of β and δ, the sophisticates invest even less than the naifs. This is because the naifs believe (incorrectly) that they will consume less tomorrow and eventually grow to $k = k^{**}$ just like a time-consistent type. Thus they think their average future marginal utility of consumption is low (i.e. high consumption) and therefore are willing to consume less than the sophisticates. In practice, however, we might expect β and δ to be positively correlated, or sophisticates to have both higher β and δ than the naive. In this case, sophisticates would invest more than naifs.

Impact of a windfall

The impact of a cash windfall is similar to the case with time-consistent preferences. Credit constraints (but not savings constraints) are needed in this simple model to expect investment and high returns. High investment and returns, moreover, will only be seen where people start below their steady state. The steady state levels of capital to which the time-inconsistent will move, however, are lower than the case without time inconsistency. Thus the average returns will be lower than the benchmark case, but still greater than r.

Recall, however, that in the time consistent case the average impact was expected to increase in patience (at least amongst those below their optimal steady state capital). With time inconsistency, holding patience constant, we expect the impacts to be larger among the more time-inconsistent. In practice, however, this comparative static will be difficult to identify, partly because β and δ may be correlated and partly because they may be difficult to measure separately.

More importantly, restricted windfalls with a flypaper effect have the potential to increase investment levels to k^*, at least temporarily. Eventually as long as they can divert, both types will return to their steady state level of investment.. However, if there is a commitment device, for example an in-kind transfer that cannot be diverted over time, then the sophisticates will more likely be the ones who apply for and use this in-kind transfer. Such a transfer will not only help some constrained individuals to enter into enterprise or get closer to their steady state level of investment, it will also change the steady state level of investment for the sophisticates from $k_{sophisticate}$ to k^{**}. A naive type, on the other hand, would not want to tie their hands to such a transfer; they would prefer a transfer that can be diverted over time. Intuitively, time inconsistency makes the sophisticates act like a person with very low discount rate $\beta\delta$ every period, when in fact their real discount rate for the far future is δ. So a windfall that also act as a commitment device could push them into a new equilibrium that it wouldn't do for someone who was time-consistent but merely impatient.

Appendix B: Supplemental empirical analysis

1 Correlates of attrition

Attrition rates approach 15% in 2010 and 18% in 2012, as discussed in Section 3.1 and Table I. Attrition is 7 pp greater in in the control group in 2012. To investigate how systematic is this attrition, Appendix Table 1 examines the baseline correlates of attrition. We regress an indicator for attrition on all baseline covariates in Table II, omitting the indicator for being unfound at baseline (since we are interested in the systematic relationship with other covariates). The regression includes district fixed effects but omits them from the table.

Overall, it's clear that attrition is at least somewhat systematic. The p-value on an F-test of joint significance of all covariates is less than 0.001. There are several covariates with substantively and statistically significant effects. For instance, attrition is less likely among older people, rural dwellers, farmers (especially those with greater hours), the completely unemployed, the illiterate, those with higher initial earnings and loan access, and group management committee members. Attrition is more likely among casual laborers and the risk averse.

With excess attrition in the control group, our treatment effects could be overstated if attrition was correlated with potential for success. We see some support for this. Our conceptual framework suggests that the returns from a grant are likely to be greatest among those with high ability and low initial wealth and access to credit. Our data suggest that impacts are much greater for the young. Since attrition is higher among the young and initially poorer, the average impact of treatment is predicted to be higher. At the same time, the more literate are more likely to be unfound and so this could depress their predicted returns from a grant.

2 Sensitivity of baseline balance to baseline non-response

Can baseline randomization imbalance be attributed to missing control groups? Approximately 6% of control group observations are missing in 2008 because the groups could not be located. A very small number of treatment group observations are also missing due to people who completed the survey but did not respond to a specific question. Appendix Table 2 looks at the sensitivity of randomization balance to alternate values for the missing control groups. The table examines four baseline covariates displaying randomization imbalance at baseline (see Table II).

All covariates are standardized and missing treatment data are imputed to the mean, or zero. Missing control group data are imputed to the mean (zero) plus 0.05, 0.10, 0.15, 0.20, or 0.25 SD of the covariate, thus gradually increasing the values of the covariates in the control group towards balance. Columns (1) to (6) report summary statistics (mean, SD, and number of observations) for the imputed treatment and control group values. Columns (7) and (8) recalculate treatment-control mean differences using an ordinary least squares regression of the covariate on assignment to treatment and district (randomization strata) fixed effects. The standard error in Column (8) is robust and clustered by group.

An imputed value of 0.05 SD for missing control groups is sufficient to bring the p-value on the treatment-control regression difference above 0.10 for all covariates except the ability to obtain a loan of UGX 100,000, which requires an imputation of 0.10 SD to become not statistically significant. In general, imputed values of 0.10 to 0.20 SD are sufficient to bring the regression differences to zero.

3 Comparison of the sample to the general population

We merge our study sample with a 2008 clustered population-based household survey, the Northern Uganda Survey (NUS). The Uganda National Statistics Bureau collected the NUS on behalf of Uganda's Office of the Prime Minister, in part to help the government assess the impacts of NUSAF on the north. The NUS was conducted in all NUSAF districts and focused on consumption, labor market activity, and health and education in the household. NUS sampling probabilities are estimates of the probability of being sampled in the full northern population.

The NUS and the YOP survey share several baseline characteristics from 2008, including: gender, age, urban status, marital status, school attainment, household size, durable assets (including 15 common measures of housing quality, livestock, and durable household items), main occupation, and district. We construct a wealth z-score that is the first principal component of the common durable asset measures. We compare our samples along these lines in Section 3.

In Appendix Table 3 we estimate a PATE by reweighting our sample to reflect the population distribution of shared, observed, baseline covariates discussed above. We estimate the PATE by reweighting our sample to reflect the distribution of these characteristics. Since 85% of our sample is aged 18 to 30, we first calculate a PATE for this age range alone, mainly adjusting for the fact that the general population is somewhat poorer and less educated. Investment and earnings treatment effects among 18 to 30 year olds are broadly similar in the population and the sample, though the PATE is marginally (but not statistically significantly) lower. If we consider the full age range, the PATE primarily reweights the treatment effects to older persons. Employment impacts are comparable and the estimated effect on durable assets increases, but the business asset and earnings PATEs are much smaller, and not statistically significant. We treat these results with caution, however, since young adults are selected into our sample because of unobserved initiative, connections or affinity for entrepreneurship. Thus the PATE probably overstates the true population average treatment effect.

4 Correlates of treatment non-compliance

Of the 265 groups assigned to treatment, 21 groups could not access funds due to unsatisfactory proposals/accounting, bank complications, or collection delays, and 8 groups reported they never received due to some form of theft or diversion. We do not have detailed data on each of these "not treated" cases. In order to assess whether the untreated are systematically different, in Appendix Table 4 we regress an indicator for not treated on baseline covariates (averaged within the group) and district fixed effects for the treatment groups alone, clustered by group. Columns 1 and 2 report coefficients and standard errors.

There are some patterns to treatment non-compliance but overall it is not very systematic. The p-value on an F-test of joint significance of all covariates is 0.199. Of 34 covariates, only three are significant at the 5% level or lower, and an additional 4 are significant at the 10% level. To provide a sense of magnitude, Column 3 reports the product of the regression coefficient by the standard deviation of the covariate. The probability that a group did not receive a grant is groups with larger group sizes and large grant amounts per member. One possibility is that these proposals invited greater scrutiny because of size, or breached district government norms of grant spending. They would also have been more attractive choices for theft, though the number of cases of theft is few. Poorer and less educated groups are less likely to receive their grants (significant at the 10% level). One possibility is that these groups were less likely to have irregularities in their applications. Finally, those groups distant from educational facilities were more likely to receive their grants. We do not have any explanation for this correlation.

5 Outcome means and raw treatment differences

Appendix Table 5 presents summary statistics on an extended list of outcomes, as well as simple intent-to-treat estimates, calculated with no baseline covariates except for the strata fixed effects. Additional outcomes in this table include more detailed employment measures and other economic outcomes such as savings and household transfers. As shown in the sensitivity analysis (Table VII in the main paper), intent-to-treat effects tend to be robust to removing the 2008 control vector.

6 Vocational skills training choices

What trades people choose in rural Africa is of general policy import, and useful or interpreting and understanding the treatment effects we observe. 68% of treatment group members and 15% of control group members report at least 16 hours of vocational training. Appendix Table 6 breaks down training incidence by skill type. It reports both whether any training was received in a skill (in the full sample, among those who trained at all, and among males and females who trained at all) as well as the proportion of hours where training was reported in each skill. The distribution of hours differs from incidence since some skills training programs (e.g. business skills) were much shorter than others.

7 Returns to different vocations

Could it be that heterogeneous impacts, especially gender differences, are driven by the fact that some choose profitable trades and others do not? This would be especially concerning if trades dominated by women have lower returns. In general, all the trades appear profitable, although male-dominated trades do have slightly higher returns. Average earnings and earnings per hour appear highest in male-dominated trades like carpentry and metalworking, followed somewhat closely by hairstyling and distantly by tailoring (both of which are mixed-gender trades).

Appendix Table 7 reports the mean and median earnings and earnings per hour, by trade, using earnings from that trade alone. We examine 2012 data only, for the full treatment sample and by gender. The table examines earnings and earnings per hour with everyone (including those reporting zero hours) and also among those who report positive hours worked in the trade only. Survey respondents were asked about the different business activities they were engaged in and the income they had received from each activity in the last four weeks. The data thus describe the businesses people are running, not those they were trained for or had originally selected to perform. Individuals may perform more than one activity including more than one trade.

8 How do economic outcomes vary with the size of the cash grant?

As described in Section 2.2 and Figure I, there was a sizable amount of potentially endogenous variation in average grant size per member in the group. Appendix Table 8 examines the (not causally identified) correlation between grant size per person and a selection of outcomes. We consider five outcomes—business assets, earnings, non-durable consumption, and savings, and durable wealth. Pooling both endlines, we regress each outcome on the grant amount for treatment groups only. The table reports standardized coefficients only. A 1 SD larger grant is associated with the following endline outcomes: a 0.08 SD increase in capital stocks (significant at the 10% level), 0.004 SD more earnings, 0.01 SD lower durable assets, 0.06 SD more savings, and 0.18 SD more non-durable consumption (significant at the 1% level).

9 Sensitivity analysis of treatment effects for additional outcomes

In Appendix Table 9 we perform the same sensitivity analysis as in Table VII, with additional outcomes. The conclusions are qualitatively similar to that described in the paper. In general these variables are even more robust to the sensitivity analysis than those presented in Table VII.

10 Social impacts

10.1 Disaggregated survey measures and treatment effects

The survey measures are largely drawn from the authors' prior studies of post-war social, political and community integration and mental health among northern Uganda youth.[1] The anti-social and aggression measures were rooted in psychological survey instruments on U.S. popula-

[1] Jeannie Annan et al., "Civil War, Reintegration, and Gender in Northern Uganda," *Journal of Conflict Resolution* 55 (2011): 875–906; Christopher Blattman and Jeannie Annan, "The Consequences of Child Soldiering," *Review of Economics and Statistics* 92 (2010): 882–898; Jeannie Annan et al., "Survey of War Affected Youth: Phase I & II Codebook" (Yale University, 2008), http://www.sway-uganda.org.

tions[2] and were adapted to the Ugandan context by the authors.[3] The use of pre-existing instruments strengthens credibility of measurement. All are self-reported rather than observed measures of behavior, however, leading to concerns of measurement error. But this should not bias our estimates, but rather increase standard errors.

Appendix Table 10 reports summary statistics and ITT estimates of the program impact on the individual components of the social impact family indices. In general, the disaggregated results are consistent with the interpretation of the family indices in the Section 6.6.

10.2 Heterogeneity analysis

The impact on aggression and anti-social behavior (ASB for short) is informative, but it is possible that the treatment has heterogeneous effects. For instance, the vast majority of nonaggressive people may have little room for improvement and so we should see treatment effects concentrated in those with the highest initial levels of aggression. Alternatively, treatment may be ineffective among the highly aggressive and so we may only expect to see effects on the less aggressive. There is little theory or empirical work to guide this analysis, and so these patterns are purely speculative. Nonetheless, they are plausible and deserve exploration.

In Appendix Table 11, we recalculate average treatment effects, by gender, adding the baseline level of aggression and an interaction term between treatment and aggression. In general, none of our main conclusions about the aggression impacts are affected by this analysis. We look at three dependent variables: the short aggression index we have for both the two- and four-year surveys, the extended ASB index, and the protest index we have for the four-year survey only. First, baseline aggression is generally positively correlated with all of the dependent variables, but the correlation is significant only for the two-year measures, suggesting that there is modest persistence of aggression over time. Second, the fall in male aggression and the rise in female aggression at two years is preserved, but the interaction coefficients suggest that the fall in male aggression is concentrated among those with the highest levels of initial aggression, but not so among the women who seem to increase aggression irrespective of baseline levels. Third, none

[2] Arnold H. Buss and Mark Perry, "The Aggression Questionnaire," *Journal of Personality and Social Psychology* 63, no. 3 (1992): 452–459.

[3] These were adapted by extensive pretesting by the authors and differ significantly from the original U.S. questionnaires. We are not aware of validated or standardized measures adapted to the African context.

of these patterns are apparent with the four-year measures of aggression, and we see little evidence of heterogeneity.

The weak association between baseline and four-year aggression is disappointing, however, and worth additional exploration. It may be possible to construct a better predictor of future aggression, however, and thereby investigate heterogeneity more thoroughly. In regressions not shown, we consider possible aggression correlates, including baseline aggression, risk aversion, and war experiences. We consider the three main dependent variables, pooling the 2010 and 2012 endlines for the short aggression index, and consider a quadratic transformation of each correlate. These variables explain a relatively modest amount of variation, significantly so in the case of the extended aggression and ASB index, and to a lesser extent the short aggression index.

To create a weighted average of these correlates, we use their value in the control group to predict endline aggression and ASB, and use the coefficients from this regression to weight the different measures, effectively creating a propensity score for aggression. If we evaluate the predictive power of this propensity score, generally the correlations between it and outcomes are positive but are small and are only statistically insignificant in one case (regressions not shown). In general, we conclude that aggression may not be very persistent or easy to predict. Alternate regressions with different functional forms or additional baseline correlates perform no better.

Nevertheless, we can use this aggression propensity score in a heterogeneity analysis, much like we did the baseline aggression measure above. We display these results in Panel B of Appendix Table 11. The 2010 impacts on men and women's aggression are unchanged, but now more aggressive males do not seem to respond disproportionately. We see no evidence of an average treatment effect on the other 2012 outcomes. The extended aggression and ASB index results, however, have a negative and significant coefficient on the interaction for men. At first this looks like evidence of a fall in aggression among the most aggressive treated men. However, if we add the treatment and interaction coefficients (to obtain the treatment effect on this subgroup) the linear combination of these two coefficients is negative but not statistically significant. Thus we have some evidence of a different effect of treatment on the more and less aggressive males, but the impact on these subgroups is still close to zero and insignificant, suggesting that our general conclusion—that there are small to no effect of the intervention on social outcomes, remains true.

11 Construction of summary indices

We have multiple baseline covariates that measure some dimension of working capital, ability, and time preferences. In order to have a single continuous summary measure of these characteristics, we construct a standardized index that is a weighted average of the multiple variables measured in the survey. Rather than arbitrarily giving each variable equal weight, or using factor analysis weights, we weight using regression estimates of their importance in predicting future economic success in the control group.

For instance, the human capital index is a weighted average of baseline measures of (i) educational attainment, (ii) a literacy indicator, (iii) an indicator for prior vocational training, (iv) performance on a digit recall test, measuring working memory, and (iv) a measure of physical disabilities assembled from responses how easily the respondent can perform a number of activities of daily life (ADLs). For weights, we use each variable's predictive power of economic success in the control group. We regress a composite measure of the economic impacts on the baseline measures of ability using the control group only. We use the estimated coefficients to predict a "score" for all treatment and control individuals, and standardize the score to have mean zero and unit standard deviation. Hence in the heterogeneity regressions, the level Index is correlated with the dependent variable by construction, but our interest is in the interaction between the Index and treatment.

The working capital index is a weighted average of baseline measures of (i) savings stock, (ii) the stock of loans outstanding, (iii) cash earnings, (iv) perceived access to a 100,000 UGX loan, (v) perceived access to a 1 million UGX loan, and (vi) indices of housing quality and assets (similar to the index of wealth endline measure). Weights are obtained in the same manner as ability.

The patience index is a weighted average of 2010 measures of 10 self-reported measures of impulsiveness and patience, including self-reported willingness to wait long periods for material goods, to spend money "too quickly", to put off hard or costly tasks, or to resist temptation. Weights for the z-score index are obtained in the same manner as ability. 2010 measures are used as no 2008 data are available, on the assumption that preferences are time-invariant and are not affected by treatment. As seen in Appendix Table 12, there is no appreciable difference in patience levels between treatment and control groups (mean difference of 0.01, p-value of 0.75), suggesting it is a time-invariant variable unaffected by treatment.

Finally, the risk aversion index is a weighted average of baseline measures of 8 self-reported measures of risky behavior, including (i) walking alone at night, (ii) engaging in unprotected sex,

(iii) investing in a risky business that could have high profits, and (iv) choosing not to sleep under a mosquito net, among others. Weights are obtained in the same manner as ability.

12 Role of the group design

The group design of the intervention could have positive effects on performance because they act as a commitment device in initial spending of the grant, or because there is learning, shared capital, or other production complementarities. Unfortunately the group aspect of the intervention could not be varied, randomly or otherwise. But analysis of the heterogeneity provides some speculative insights. Heterogeneity patterns suggest that members of the most functional groups at baseline have the highest investment and earnings—evidence in favor of positive group effects. In Appendix Table 13 we look at the effect of baseline group characteristics on investments and earnings: an indicator for whether the group existed prior to YOP; a quality of the group dynamic index (a z-score based on group members' assessments of the group's level of trust, cooperation, loyalty, inclusiveness, and equity); group size; proportion female; and a group heterogeneity index (a z-score of the standard deviation of group member education, wealth, and age). For each dependent variable, we first look at the correlation between these group characteristics and performance in those assigned to treatment (Columns 1, 3 and 5), then the treatment heterogeneity in the full sample (Columns 2, 4 and 6).

The most significant finding: A standard deviation increase in the dynamic is associated with an increase in the ATE of about 189,000 UGX (about $109 USD) in capital stocks and nearly 6,000 UGX (about $3.50 USD) for monthly earnings. Group size, prior existence, group heterogeneity, and female domination have little robust association with investment and earnings.

We cannot interpret this heterogeneity causally, as more able or forward-looking individuals may have formed higher-quality groups. That said, we account for these initial characteristics as best we can with our control variables. Thus, the association between more cohesive and cooperative groups and individual economic performance suggests the group design probably succeeded in acting as a commitment device and generating group learning and other complementarities.

13 How long to "pay back" a grant?

If the per capita grant had been a loan, how fast could it be repaid? For simplicity, we assume the amount to repay, A, is the average per capita grant of UGX 656,923, and the monthly repayment, P, is equal to the full treatment effect. For the full treatment effect we use the average of the 2010 and 2012 earnings treatment effects, so that P = UGX 16,396. The number of months to repay A is equal to $-\log[1 - (r/12 \times A/P)] / \log(1 + r/12)$, where r is the real interest rate. This is equal to 3.3 years for $r = 0$, 3.7 years for $r = 0.05$, 4.7 years for $r = 0.15$, 7.3 years for $r = 0.25$, and infinity for $r = 1$.

Appendix Table 1: Correlates of attrition

	Dependent variable: Indicator for attrition					
	2010 endline			2012 endline		
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline covariate, 2008	Coefficient	SE	Effect of 1 SD change in covariate	Coefficient	SE	Effect of 1 SD change in covariate
Assigned to treatment	0.020	[0.020]	.	-0.050	[0.023]	.
Grant amount applied for, USD	0.000	[0.000]	-0.030	0.000	[0.000]	0.000
Group size	0.000	[0.005]	0.003	-0.005	[0.004]	-0.034
Grant amount per member, USD	0.000	[0.000]	0.035	0.000	[0.000]	-0.010
Group existed before application	-0.016	[0.024]	.	-0.030	[0.025]	.
Group age, in years	0.001	[0.005]	0.002	-0.002	[0.006]	-0.003
Within-group heterogeneity (z-score)	0.013	[0.011]	0.013	0.026	[0.013]**	0.026
Quality of group dynamic (z-score)	0.008	[0.013]	0.008	-0.009	[0.016]	-0.009
Distance to educational facilities (km)	0.002	[0.002]	0.015	0.000	[0.003]	-0.002
Age at baseline	-0.003	[0.002]*	-0.018	-0.006	[0.002]***	-0.030
Large town/urban area	0.081	[0.030]***	.	0.143	[0.036]***	.
Risk aversion index (z-score)	0.039	[0.011]***	0.039	0.046	[0.012]**	0.046
Management committee member	-0.044	[0.018]**	.	-0.042	[0.024]	.
Chairperson or vice-chairperson	0.013	[0.027]	.	0.023	[0.036]	.
Weekly work hours: Casual labor	0.003	[0.002]*	0.017	0.003	[0.003]	0.013
Weekly work hours: Own business	0.001	[0.001]	0.005	-0.001	[0.002]	-0.010
Weekly work hours: Skilled trades	0.002	[0.001]*	0.018	0.000	[0.002]	0.004
Weekly work hours: High-skill wage labor	0.001	[0.009]	0.001	-0.017	[0.010]	-0.014
Weekly work hours: Other non-ag work	0.003	[0.003]	0.013	-0.002	[0.002]	-0.007
Weekly work hours: All agricultural work	-0.005	[0.001]***	-0.056	-0.005	[0.001]	-0.052
Weekly household chores, hours	-0.001	[0.000]	-0.012	-0.001	[0.001]	-0.017
Zero employment hours in past month	-0.134	[0.032]***	.	-0.149	[0.034]	.

Continued next page

	Dependent variable: Indicator for attrition					
	2010 endline			2012 endline		
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline covariate, 2008	Coefficient	SE	Effect of 1 SD change in covariate	Coefficient	SE	Effect of 1 SD change in covariate
Main occupation is non-agricultural	-0.171	[0.037]***	.	-0.094	[0.047]	.
Engaged in a skilled trade	-0.061	[0.036]*	.	-0.043	[0.053]	.
Currently in school	-0.083	[0.034]**	.	-0.067	[0.052]	.
Highest grade reached at school	-0.002	[0.003]	-0.007	0.000	[0.004]	0.000
Able to read and write minimally	0.065	[0.021]***	.	0.048	[0.026]	.
Received prior vocational training	-0.034	[0.030]	.	-0.051	[0.037]	.
Digit recall test score	-0.008	[0.004]**	-0.016	0.016	[0.006]***	0.033
Index of physical disability	-0.006	[0.002]***	-0.014	-0.002	[0.003]	-0.004
Durable assets (z-score)	0.016	[0.011]	0.017	-0.008	[0.012]	-0.009
Savings in past 6 mo. (000s 2008 UGX)	0.000	[0.000]	0.011	0.000	[0.000]***	0.035
Monthly cash earnings (000s 2008 UGX)	0.000	[0.000]*	-0.014	0.000	[0.000]	-0.017
Can obtain 100,000 UGX ($58) loan	-0.024	[0.020]	.	-0.011	[0.022]	.
Can obtain 1,000,000 UGX ($580) loan	-0.014	[0.028]	.	0.005	[0.037]	.
Observations		2,323			2,111	
Mean of dependent variable		-0.146			-0.179	
p-value on F-test of joint significance, all covariates		<0.001			<0.001	

Notes: Columns (1)-(2) and (4)-(5) report the coefficients and standard errors from a weighted least squares regression of an indicator for attrition on the baseline covariates used in all treatment effects regressions and listed in Table II (exlcuding the indicator for unfound at baseline). Weights are the inverse of the probability of selection into endline tracking. To provide a sense of magnitude, columns (3) and (6) report the product of the standard deviation of the baseline variable (in Table II) and the coefficients in Columns (1) and (4), with the exception of indicator variables. Robust standard errors are clustered at the group level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2: Sensitivity of baseline randomization balance to imputation of missing control group data

Baseline covariate exhibiting treatment imbalance (transformed into z-scores)	Missing control group data imputed to the mean plus:	Balance statistics with imputed control group data							
		Control group			Treatment group			Regression difference	
		(1) Mean	(2) SD	(3) Obs	(4) Mean	(5) SD	(6) Obs	(7) Coeff.	(8) p-value
Durable assets	+0.05 SD	-0.01	0.95	1,352	0.05	1.06	1,325	0.03	0.49
	+0.10 SD	0.02	0.97	1,352	0.05	1.06	1,325	0.01	0.90
	+0.15 SD	0.05	1.00	1,352	0.05	1.06	1,325	-0.02	0.69
	+0.20 SD	0.07	1.05	1,352	0.05	1.06	1,325	-0.05	0.39
	+0.25 SD	0.10	1.10	1,352	0.05	1.06	1,325	-0.08	0.20
Prior vocational training	+0.05 SD	0.02	0.97	1,352	0.02	1.04	1,325	0.04	0.39
	+0.10 SD	0.05	0.99	1,352	0.02	1.04	1,325	0.01	0.83
	+0.15 SD	0.08	1.02	1,352	0.02	1.04	1,325	-0.02	0.70
	+0.20 SD	0.11	1.07	1,352	0.02	1.04	1,325	-0.05	0.36
	+0.25 SD	0.13	1.12	1,352	0.02	1.04	1,325	-0.07	0.17
Can obtain 100,000 UGX loan	+0.05 SD	-0.02	0.96	1,352	0.09	1.02	1,325	0.09	0.03
	+0.10 SD	0.01	0.99	1,352	0.09	1.02	1,325	0.06	0.15
	+0.15 SD	0.04	1.02	1,352	0.09	1.02	1,325	0.03	0.45
	+0.20 SD	0.07	1.07	1,352	0.09	1.02	1,325	0.01	0.89
	+0.25 SD	0.09	1.12	1,352	0.09	1.02	1,325	-0.02	0.68
Savings in past 6 mo.	+0.05 SD	-0.02	0.82	1,352	0.06	1.16	1,325	0.06	0.11
	+0.10 SD	0.01	0.84	1,352	0.06	1.16	1,325	0.03	0.39
	+0.15 SD	0.03	0.88	1,352	0.06	1.16	1,325	0.01	0.87
	+0.20 SD	0.06	0.94	1,352	0.06	1.16	1,325	-0.02	0.65
	+0.25 SD	0.09	1.00	1,352	0.06	1.16	1,325	-0.05	0.33

Notes : This table recalculates balance for four baseline covariates displaying randomization imbalance at baseline, in Table II. Approximately 6% of control group observations are missing and a very small number of treatment group observations are missing (people who completed the survey but did not respond to a specific question. All covariates are standardized and missing treatment data are imputed to the mean, or zero. Missing control group data are imputed to the mean plus 0.05, 0.10, 0.15, 0.20, or 0.25 SD of the variable, thus gradually increasing the values of the covariates in the control group. Columns (1) to (6) report summary statistics (mean, SD, and number of observations) for the imputed treatment and control group values. Columns (7) and (8) recalculate treatment-control mean differences using an ordinary least squares regression of the covariate on assignment to treatment and district (randomization strata) fixed effects. The standard error in Column (8) is robust and clustered by group.

Appendix Table 3: Sample and population average treatment effects, 2012

	Average treatment effect (ITT controlling for baseline covariates)				
	All ages	Adults aged 16-35 only		Adults aged 18-30 only	
	(1)	(2)	(3)	(4)	(5)
Dependent variable	Within sample	Within sample	Reweighted to population	Within sample	Reweighted to population
Training hours	340.450	340.775	313.025	351.882	319.876
	[22.521]***	[22.989]***	[29.334]***	[24.059]***	[27.016]***
Real value of business assets (000s of 2008 UGX)	224.986	219.843	139.876	210.282	154.698
	[62.601]***	[63.400]***	[102.234]	[67.063]***	[62.554]**
Average weekly employment hours in skilled trade	3.776	3.714	3.593	3.839	5.039
	[0.548]***	[0.571]***	[0.968]***	[0.594]***	[0.888]***
Real net monthly earnings (000s of 2008 UGX)	18.186	17.818	9.940	18.691	19.747
	[4.898]***	[4.984]***	[8.500]	[4.944]***	[4.642]***
Durable assets (z-score)	0.181	0.152	0.181	0.189	0.331
	[0.055]***	[0.055]***	[0.109]*	[0.058]***	[0.083]***
Observations					

Notes: Column (1) lists the sample ITT estimate from Table III for each dependent variable. Column (2) recalculates the ITT for people aged 16-35 in the sample only (dropping older and younger respondents). Column (3) recalculates the estimate in Column (2), weighting for the inverse probability of selection into the sample from the population (rescaled to have a mean of one) in addition to the usual inverse probability of selection into endline tracking. Columns (4) and (5) do the same for people aged 18-30. All treatment effects are calculated 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 characteristics reported in Table II. Robust standard errors are clustered at the group level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4: Correlates of treatment non-compliance, treatment group only

	Dependent variable: "Not treated" (i.e. Group did not receive a grant)		
	(1)	(2)	(3)
	OLS with district fixed effects		Effect of 1 SD change in covariate
Baseline covariate, group average	Coefficient	SE	
Grant amount applied for, USD	0.0000	[2.68e-05]	-0.056
Group size	0.0184	[0.00729]**	0.132
Grant amount per member, USD	0.0007	[0.000354]**	0.123
Group existed before application	-0.0186	[0.0432]	.
Group age, in years	0.0207	[0.0124]*	0.037
Within-group heterogeneity (z-score)	-0.0269	[0.0187]	-0.029
Quality of group dynamic (z-score)	0.0312	[0.0176]*	0.032
Distance to educational facilities (km)	-0.0099	[0.00365]***	-0.055
Age at baseline	0.0039	[0.00565]	0.013
Female	0.0736	[0.0820]	.
Large town/urban area	-0.0060	[0.0627]	.
Risk aversion index (z-score)	-0.0141	[0.0367]	-0.010
Highest grade reached at school	-0.0277	[0.0143]*	-0.054
Able to read and write minimally	0.0561	[0.0888]	.
Received prior vocational training	0.1000	[0.134]	.
Digit recall test score	-0.0083	[0.0192]	-0.008
Index of physical disability	-0.0239	[0.0145]	-0.028
Durable assets (z-score)	-0.0136	[0.0296]	-0.009
Savings in past 6 mo. (000s 2008 UGX)	-0.0002	[0.000300]	-0.015
Monthly cash earnings (000s 2008 UGX)	-0.0004	[0.000222]*	-0.034
Can obtain 100,000 UGX ($58) loan	0.0560	[0.0777]	.
Can obtain 1,000,000 UGX ($580) loan	0.1530	[0.115]	.
Weekly employment, hours: Casual labor, low skill	-0.0020	[0.0115]	-0.005
Weekly employment, hours: Own business, low skill	0.0116	[0.00825]	0.042
Weekly employment, hours: Skilled trades	0.0011	[0.00678]	0.005
Weekly employment, hours: High-skill wage labor	0.0052	[0.0303]	0.003
Weekly employment, hours: Other non-agricultural work	0.0149	[0.0150]	0.023
Weekly employment, hours: All agricultural work	0.0083	[0.00554]	0.052
Weekly household chores, hours	-0.0012	[0.00265]	-0.012
Zero employment hours in past month	0.0962	[0.119]	.
Main occupation is non-agricultural	0.1970	[0.168]	.
Engaged in a skilled trade	-0.0480	[0.154]	.
Currently in school	-0.2910	[0.184]	.
Observations		265	
Mean of dependent variable		0.109	
p-value on F-test of joint significance of all covariates		0.199	

Notes: The dependent variable, "Not treated", is equal to 1 if the government did not disburse a grant to the group (21 cases), or if the group reported that they did not receive the grant due to some form of diversion or theft (8 cases). Column (1) lists the coefficients on baseline covariates in an unweighted ordinary least squares regression of "Not treated" on covariates and district fixed effects. The coefficients on district fixed effects are omitted from this table. Column (2) reports robust standard errors in brackets, clusted by group. To provide a measure of substantive magnitude, Column (3) reports the product of the coefficient and the standard deviation of the covariate (from Table II), except for indicator variables. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 5: Descriptive statistics and simple intent-to-treat effects

	2010 (2-year endline)				2012 (4-year endline)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control	Treatment	ITT, no 2008 covars.		Control	Treatment	ITT, no 2008 covars.	
	Mean	Mean	Coeff.	SE	Mean	Mean	Coeff.	SE
Transfers								
Treated (Group received YOP cash transfer)	0.00	0.89	0.884	[0.020]***	.	.	.	.
Received non-YOP transfer or program	0.16	0.18	0.016	[0.019]	0.02	0.05	0.025	[0.009]***
Value of non-YOP program (000s 2008 UGX)	23.00	101.70	64.967	[19.266]***	.	.	.	.
Thinks "likely" to receive a program in future	.	.	.	.	0.76	0.76	0.025	[0.021]
Investments								
Enrolled in vocational training	0.15	0.68	0.535	[0.024]***	.	.	.	.
Returned to primary or secondary school	0.10	0.12	0.018	[0.017]	.	.	.	.
Hours of vocational training received	48.98	378.32	340.768	[23.731]***	.	.	.	.
Business assets (000s 2008 UGX)	290.24	725.77	394.269	[80.627]***	392.79	607.82	228.352	[67.129]***
Employment								
Average employment hours per week	24.91	28.91	4.151	[1.074]***	32.24	37.64	5.204	[1.306]***
Agricultural	13.90	13.02	-0.767	[0.839]	18.77	18.88	0.384	[1.036]
Non-agricultural	11.01	15.89	4.918	[0.914]***	13.48	18.76	4.821	[1.098]***
Casual labor, low skill	1.51	1.66	0.016	[0.374]	2.27	2.19	-0.236	[0.415]
Petty business, low skill	5.33	4.93	-0.344	[0.622]	5.44	5.62	-0.094	[0.706]
Skilled trades	2.92	7.34	4.500	[0.626]***	2.82	6.42	3.558	[0.569]***
High-skill wage labor	1.24	1.97	0.746	[0.350]**	1.84	2.64	0.816	[0.493]*
Other non-agricultural	0.00	0.00	0.000	[0.000]	1.11	1.89	0.777	[0.421]*
No employment hours in past month	0.10	0.09	-0.017	[0.016]	0.05	0.02	-0.019	[0.009]**
Main occupation is non-agricultural	0.16	0.22	0.045	[0.020]**	0.19	0.23	0.019	[0.022]
Engaged in any skilled trade	0.17	0.44	0.266	[0.026]***	0.22	0.48	0.256	[0.027]***
Works ≥30 hours per week in a skilled trade	0.04	0.09	0.049	[0.013]***	0.03	0.07	0.035	[0.012]***
Average hours of chores per week	9.69	7.60	-1.741	[0.656]***	9.98	8.63	0.015	[0.735]

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Appendix Table 5 (continued): Descriptive statistics and simple intent-to-treat effects

	2010 (2-year endline)				2012 (4-year endline)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control	Treatment	ITT, no controls		Control	Treatment	ITT, no controls	
	Mean	Mean	Coeff.	SE	Mean	Mean	Coeff.	SE
Migration and urbanization								
Has changed parish since baseline	0.23	0.28	0.046	[0.025]*	0.35	0.28	-0.083	[0.028]***
Lives in large town or city	0.18	0.15	-0.001	[0.027]	0.17	0.15	0.005	[0.027]
Lives in Kampala	0.00	0.01	0.007	[0.005]	0.01	0.00	-0.004	[0.005]
Business formality								
Maintains formal records	0.30	0.41	0.113	[0.024]***	0.26	0.39	0.116	[0.024]***
Enterprise is formally registered	0.15	0.18	0.053	[0.019]***	0.11	0.17	0.060	[0.020]***
Pays business taxes	0.21	0.28	0.077	[0.024]***	0.22	0.31	0.087	[0.024]***
Income								
Monthly cash earnings (000s 2008 UGX)	35.25	49.96	14.344	[4.271]***	47.85	65.31	17.429	[5.424]***
Durable assets (z-score)	-0.06	0.04	0.101	[0.054]*	0.15	0.33	0.185	[0.061]***
Non-durable consumption (z-score)	.	.	.	.	-0.01	0.10	0.182	[0.057]***
Subjective well being, 1 to 9 scale	2.73	3.05	0.375	[0.077]***	3.29	3.73	0.402	[0.089]***
Other economic outcomes								
Savings (000s 2008 UGX)	76.89	99.85	23.723	[11.980]**	.	.	.	.
Net household transfers (000s 2008 UGX)	6.80	-1.11	-6.651	[4.705]	.	.	.	.

Notes: Columns (1)-(2) and (5)-(6) report summary statistics for the treatment and control groups 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 the impact of program assignment at each endline. These standard errors are heteroskedastic-robust and clustered by group. The full sample ITT is calculated via a weighted least squares regression of the dependent variable on a program assignment indicator and randomization strata only (13 district fixed effects). *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 6: Training incidence and hours, by skill

	Percentage who received at least 16 hours of training in the skill (can exceed 100%)								Percentage of hours trained in each skill (among those who received at least 16 hours of training in a skill)					
	Full sample		Among those who received any training											
	Both genders		Both genders		Men		Women		Both genders		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Skill	T	C	T	C	T	C	T	C	T	C	T	C	T	C
Tailoring	26%	5%	38%	31%	23%	16%	69%	61%	13%	22%	14%	11%	12%	53%
Carpentry	16%	2%	23%	12%	31%	17%	6%	2%	16%	8%	16%	9%	10%	6%
Metal Work	10%	1%	13%	5%	17%	6%	4%	2%	13%	6%	12%	7%	13%	4%
Salon	5%	1%	8%	4%	4%	4%	15%	3%	9%	20%	11%	25%	8%	20%
Business	4%	6%	5%	27%	5%	30%	6%	20%	4%	5%	4%	6%	3%	3%
Repairs	3%	1%	4%	7%	5%	9%	2%	2%	10%	7%	10%	8%	9%	6%
Mechanic	3%	1%	4%	5%	5%	7%	2%	0%	16%	20%	13%	21%	30%	0%
Brick	2%	1%	3%	4%	4%	4%	0%	3%	6%	3%	6%	4%	4%	2%
Agribusiness	1%	1%	2%	7%	2%	8%	1%	7%	2%	2%	2%	2%	1%	2%
Other	15%	5%	20%	21%	20%	24%	22%	15%	12%	6%	12%	8%	11%	4%
No training	29%	80%												
Total	114%	104%	120%	123%	116%	125%	127%	115%	100%	100%	100%	100%	100%	100%

Notes: Columns (1) to (8) report the percentage of people who reported at least 16 hours of training in a particular skill, first in the full sample treatment (T) and control (C) groups and subsequently only among those who received any training. Since people could report more than one training program, the totals can exceed 100%. Columns (9) to (14) report the distribution of hours of training among those with at least 16 hours of training in a particular skill.

Appendix Table 7: Earnings patterns by trade in 2012 (using earnings from that trade only)

Panel a: Earnings from each trade in the past 4 weeks (000s UGX), treated indidivuals only

	Full sample			Male				Female			
Skill	Mean	Median	SD	Mean	Median	SD	N	Mean	Median	SD	N
Carpentry	59.7	26.5	83.0	60.2	28.0	83.2	117	0.0	0.0	.	1
Metalworking	52.7	30.0	85.7	53.8	30.0	86.3	47	0.0	0.0	.	1
Tailoring	20.1	10.0	34.7	21.5	10.0	31.6	68	19.0	10.0	36.8	100
Hairstyling	36.5	19.0	51.6	47.9	20.0	61.3	19	24.3	13.5	36.9	18

Panel b: Earnings from each trade in the past 4 weeks (000s UGX), treated individuals only, Zero values excluded

	Full sample			Male				Female			
Skill	Mean	Median	SD	Mean	Median	SD	N	Mean	Median	SD	N
Carpentry	67.7	34.5	85.3	67.7	34.5	85.3	104	.	.	.	.
Metalworking	56.2	36.0	87.4	56.2	36.0	87.4	45	.	.	.	.
Tailoring	21.9	10.0	35.7	23.6	12.5	32.4	62	20.7	10.0	37.9	92
Hairstyling	38.5	20.0	52.3	50.6	30.0	62.0	18	25.8	15.0	37.5	17

Panel c: Earnings per hour worked from each trade in the past 4 weeks (000s UGX), treated individuals only

	Full sample			Male				Female			
Skill	Mean	Median	SD	Mean	Median	SD	N	Mean	Median	SD	N
Carpentry	1.6	1.0	1.9	1.6	1.0	1.9	112	.	.	.	.
Metalworking	1.4	0.6	2.6	1.5	0.7	2.6	47	0.0	0.0	.	1
Tailoring	0.7	0.3	1.3	0.7	0.4	0.9	66	0.7	0.3	1.5	99
Hairstyling	1.4	0.5	1.8	1.0	0.3	1.5	19	2.0	1.7	1.9	18

Panel d: Earnings per hour worked from each trade in the past 4 weeks (000s UGX), treated individuals only, Zero values excluded

	Full sample			Male				Female			
Skill	Mean	Median	SD	Mean	Median	SD	N	Mean	Median	SD	N
Carpentry	1.7	1.2	1.9	1.7	1.2	1.9	102	.	.	.	.
Metalworking	1.5	0.7	2.7	1.5	0.7	2.7	45	.	.	.	.
Tailoring	0.8	0.4	1.3	0.8	0.4	0.9	60	0.8	0.3	1.5	91
Hairstyling	1.5	0.6	1.8	1.0	0.3	1.6	18	2.1	2.0	1.9	17

Appendix Table 8: Association between grant size and economic outcomes, treatment group only

	Dependent variable (standardized z-score), pooled endline surveys				
	(1)	(2)	(3)	(4)	(5)
	Business asset stock (000s of 2008 UGX)	Monthly cash earnings (000s of 2008 UGX)	Durable wealth index	Current savings (000s of 2008 UGX)	Non-durable consumption index
Grant size per person (standardized z-score)	0.0839	0.0044	-0.0125	0.0600	0.1770
	[0.046]*	[0.039]	[0.038]	[0.054]	[0.068]***
Endline surveys	2010, 2012	2010, 2012	2010, 2012	2012	2012
Observations	1683	1683	1672	811	866

Notes: Columns (1) to (5) report the coeffcients on grant size per person in a weighted least squares regression of each depedent variable on assignement to treatment and all baseline covariates in Table II, as well as district fixed effects and an indicator for the 2010 survey. Weights are the inverse probability of selection into the endline samples. Robust standard errors are clsutered at the group level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 9: Sensitivity analysis of treatment effects to alternate models and missing data scenarios

	Program impact under alternative models			Sensitivity of ITT estimate to systematic attrition scenarios				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Impute missing dependent variable with mean + (-) *X* SD for missing control (treatment) indidivuals				"Worst case" Manski
Dependent variable	ITT with controls	TOT with controls	ITT without controls	0.1 SD	0.25 SD	0.5 SD	1 SD	bound
Enrolled in vocational training								
2010	0.532	0.606	0.535	0.521	0.506	0.48	0.428	0.347
SE	[0.023]***	[0.023]***	[0.024]***	[0.02]***	[0.02]***	[0.021]***	[0.022]***	[0.025]***
Hours of training received								
2010	340.450	387.430	340.208	328.506	315.147	292.882	248.352	19.294
SE	[22.521]***	[23.332]***	[23.590]***	[19.399]***	[19.433]***	[19.645]***	[20.615]***	[38.56]
Non-durable consumption (z-score)								
2012	0.180	0.208	0.190	0.137	0.091	0.014	-0.14	-0.795
SE	[0.051]***	[0.058]***	[0.057]***	[0.046]***	[0.046]**	[0.048]	[0.053]***	[0.096]***
Avg. weekly hours non-farm work								
2010	5.306	6.127	5.060	4.74	4.1	3.032	0.898	-6.377
SE	[0.867]***	[0.968]***	[0.914]***	[0.77]***	[0.775]***	[0.79]***	[0.847]	[1.385]***
2012	5.097	6.111	5.182	4.653	3.744	2.23	-0.798	-13.29
SE	[0.998]***	[1.137]***	[1.115]***	[0.858]***	[0.868]***	[0.895]**	[0.993]	[1.884]***
Subjective well-being								
2010	0.368	0.417	0.381	0.314	0.254	0.156	-0.042	-0.56
SE	[0.075]***	[0.085]***	[0.078]***	[0.066]***	[0.067]***	[0.068]**	[0.074]	[0.107]***
2012	0.407	0.462	0.409	0.375	0.294	0.159	-0.111	-0.921
SE	[0.085]***	[0.097]***	[0.088]***	[0.072]***	[0.072]***	[0.074]**	[0.082]	[0.131]***

Notes: Column (1) replicates the main 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 control variables listed in Table II, but keeping the district fixed effects (since treatment probabilities vary by these strata). CIn Columns (5) to (9), we re-estimate 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 (4) to (8) impute the mean of the control (treatment) group plus (minus) "X" standard deviations of the group's distribution (SD), for X=0.1, 0.25, 0.5, and 1. Column (8) imputes the highest value in the distribution for controls and the lowest for treatment, often called the worst-case Manski bound.

Appendix Table 10: Summary statistics and program impacts for social outcomes

	2010		2012	
	ITT, with controls		ITT, with controls	
	(1)	(2)	(3)	(4)
Outcome	Coeff	SE	Coeff	SE
Social integration				
Family is caring (4-point scale)	-0.02	[0.031]	0.002	[0.025]
Family has few disputes (4-point scale)	-0.007	[0.029]	0.047	[0.018]***
Lives with a partner	0.028	[0.021]	0.029	[0.022]
Quality of rlder relations (4-point scale)	-0.016	[0.034]	-0.064	[0.032]**
Community participation and engagemennt				
Number of group memberships	0.272	[0.091]***	--	--
Attends community meetings	0.027	[0.023]	0.025	[0.045]
Community mobilizer	0.037	[0.022]*	-0.032	[0.023]
Community leader	0.008	[0.024]	--	--
Speaks out at community meetings	0.044	[0.023]*	--	--
Member of village government	--	--	0.024	[0.017]
Member of village decision-making committee	--	--	-0.013	[0.023]
Interested in participating in village government	--	--	-0.008	[0.024]
Got together with others to raise an issue	--	--	0.018	[0.026]
Contributed to the following public goods in past year				
Road maintenance	--	--	-0.013	[0.026]
Community water construction and maintenance	--	--	-0.015	[0.025]
Community building construction and maintenance	--	--	0.021	[0.014]
School fundraising	--	--	0.009	[0.025]
Community latrines construction and maintenance	--	--	0.005	[0.018]
Community fundraising	--	--	0.014	[0.022]
Funeral expenses of someone outside of the family	--	--	-0.018	[0.020]
Aggressive and hostile behaviors (each a 4-point scale)				
Is quarrelsome	0.011	[0.030]	0.04	[0.028]
Takes things without permission	-0.005	[0.024]	0.003	[0.014]
Curses or uses abusive language	-0.004	[0.020]	0.005	[0.021]
Threatens to hurt others	-0.018	[0.025]	0.007	[0.018]
Has disputes with neighbors	-0.018	[0.026]	0.034	[0.018]*
Has disputes with community leaders	-0.027	[0.015]*	-0.002	[0.005]
Has disputes with police	-0.036	[0.013]***	-0.008	[0.007]
Is involved in physical fights	0.006	[0.013]	0.006	[0.012]

Continued next page

	2010		2012	
	ITT, with controls		ITT, with controls	
	(1)	(2)	(3)	(4)
Outcome *(continued from previous page)*	Coeff	SE	Coeff	SE
Aggressive and hostile behaviors (extended)				
Yells at others when they annoyed you	--	--	0.033	[0.044]
React angrily when provoked	--	--	-0.021	[0.042]
Get angry when frustrated	--	--	0.055	[0.055]
Damage things because you feel mad	--	--	-0.017	[0.023]
Become angry when prevented from doing things	--	--	-0.003	[0.053]
Get angry when others threaten you	--	--	-0.015	[0.053]
Feel better after hitting or yelling at someone	--	--	-0.016	[0.030]
Hit others to defend yourself	--	--	0.034	[0.038]
Damage things for fun	--	--	-0.008	[0.021]
Use physical force to get others to do what you want	--	--	-0.031	[0.025]
Use force to obtain money or things from others	--	--	-0.003	[0.019]
Get others to gang up on someone else.	--	--	-0.014	[0.019]
Carry a weapon to fight	--	--	-0.01	[0.012]
Yell at others so they will do things for you	--	--	0.012	[0.030]
Protest Attitudes				
Protest attendance index (4-point scale)	--	--	-0.001	[0.011]
Feel protests were justified	--	--	0.004	[0.019]
Feel violence and destruction during protests was justified	--	--	-0.002	[0.014]
Feel police and the military were justified in having a violent response to the protests	--	--	-0.016	[0.018]
Wishes there would have been a protest in their district	--	--	-0.009	[0.013]
Would go now if there was a similar protest in their district	--	--	-0.001	[0.014]
If the protest turned violent, would stay to participate in the violence	--	--	0.01	[0.012]

Notes: Columns (1)-(4) report the intent-to-treat (ITT) estimates and standard errors of the impact of program assignment at each endline. These standard errors are heteroskedastic-robust and clustered by group. The full sample ITT is calculated 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 characteristics 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. Weights are the inverse of the probability of selection into endline tracking. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 11: Aggression heterogeneity analysis

Panel A: Using baseline aggression	Dependent variable							
	Aggression index				Aggression (extended)		Protests	
	2010		2012		2012		2012	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Covariate	Male	Female	Male	Female	Male	Female	Male	Female
Treated	-0.207	0.208	0.044	0.069	0.013	0.020	0.006	-0.042
	[0.065]***	[0.088]**	[0.061]	[0.092]	[0.061]	[0.096]	[0.063]	[0.100]
Treated X Aggression index	-0.172	-0.042	-0.047	0.082	-0.012	0.046	0.015	0.002
	[0.073]**	[0.086]	[0.064]	[0.094]	[0.066]	[0.106]	[0.070]	[0.093]
Aggression index	0.114	0.079	0.023	0.034	0.057	0.036	-0.060	-0.008
	[0.056]**	[0.048]*	[0.045]	[0.049]	[0.051]	[0.061]	[0.051]	[0.061]
Observations	1,328	667	1,228	627	1,228	627	1,228	627

Panel B: Using aggression p-score	Dependent variable							
	Aggression index				Aggression (extended)		Protests	
	2010		2012		2012		2012	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Covariate	Male	Female	Male	Female	Male	Female	Male	Female
Treated	-0.204	0.200	0.031	0.080	-0.011	0.032	0.002	-0.048
	[0.066]***	[0.085]**	[0.065]	[0.085]	[0.063]	[0.092]	[0.064]	[0.096]
Treated X Aggression p-score	-0.094	0.056	-0.094	-0.184	-0.163	-0.194	-0.060	0.014
	[0.065]	[0.072]	[0.069]	[0.136]	[0.070]**	[0.122]	[0.070]	[0.118]
Aggression p-score	0.106	-0.002	0.189	0.326	0.274	0.265	-0.027	0.114
	[0.072]	[0.097]	[0.087]**	[0.131]**	[0.082]***	[0.133]**	[0.077]	[0.137]
Observations	1,319	665	1,217	626	1,217	626	1,217	626

Notes: Columns (1) to (16) 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<0.01, ** p<0.05, * p<0.1

Appendix Table 12: Patience measures and treatment-control differences

	Control		Treatment		Regression difference	
	(1)	(2)	(3)	(4)	(5)	(6)
Covariate, measured in 2010	Mean	SD	Mean	SD	Coeff.	p-value
Patience index (z-score)	0.02	0.99	-0.02	1.01	-0.01	0.75
Patience index components (0-3 scale):						
Good at resisting temptation	2.4	0.7	2.3	0.7	-0.01	0.57
Would spend an afternoon waiting for a free medical exam	1.6	0.9	1.6	0.9	0.01	0.70
Take warnings now for many years in advance	2.2	0.8	2.3	0.8	0.02	0.42
Sometimes not able to stop doing something that is wrong	1.8	1.0	1.8	0.9	0.00	0.92
Keep postponing activities	1.8	0.9	1.7	0.9	-0.02	0.57
If you get money, you spend it too quickly	1.7	1.0	1.7	0.9	-0.03	0.35
Sometimes act quickly and not think about results of actions	1.9	0.9	1.8	0.9	-0.02	0.55
Regret many choices you have made in the past	1.1	0.8	1.1	0.8	-0.01	0.60
Easy task first or hard task first	1.4	0.9	1.4	0.9	0.01	0.72
Medicine today vs. medicine in one week that will cure you	1.1	1.0	1.1	1.0	0.02	0.57

Notes : Columns (1) to (4) list summary statistics for the aggregate patience index and its components. Columns (5) and (6) report the results of a regression of the covariate on assignment to treatment and randomization strata (district) fixed effects, with robust standard errors clustered by group.

Appendix Table 13: Investments and performance by group characteristics

	Dependent variable		
	2010	2010 and 2012 pooled	
	(1)	(2)	(3)
Independent variable	Hours of training received since baseline	Real value of business asset stock (000s of 2008 UGX)	Real net cash earnings (000s of 2008 UGX)
Assigned to treatment	322.253	721.933	30.352
	[76.984]***	[242.658]***	[13.100]**
Assigned × Group existed prior to YOP	35.117	-9.573	1.129
	[42.040]	[102.864]	[6.350]
Group existed prior to YOP	26.669	-4.045	-0.150
	[21.926]	[63.608]	[4.364]
Assigned × Group dynamic index	29.459	189.007	5.766
	[16.960]*	[42.836]***	[2.996]*
Group dynamic index (z-score)	-24.876	-85.873	-5.347
	[8.591]***	[30.942]***	[2.391]**
Assigned × Group size	-0.919	-7.989	-0.234
	[2.954]	[7.832]	[0.455]
Group size	-4.503	2.664	-0.060
	[2.413]*	[7.574]	[0.469]
Assigned × % of group female	56.059	-363.071	-28.322
	[77.671]	[229.339]	[11.560]**
% of group female	-57.324	125.685	14.876
	[40.533]	[128.904]	[7.681]*
Assigned × Group heterogeneity index	-17.119	20.857	-0.249
	[23.407]	[48.310]	[3.222]
Group heterogeneity index (z-score)	3.671	-12.392	2.377
	[10.685]	[32.735]	[2.425]
2012 endline		109.023	13.953
		[55.277]**	[3.839]***
Assigned x 2012 endline		-234.858	2.743
		[101.551]**	[6.066]
R-squared	0.232	0.084	0.136
Observations	1997	3869	3869
Control Mean	48.98	340.7	41.45