CREDIT CONSTRAINTS, OCCUPATIONAL CHOICE, AND THE PROCESS OF DEVELOPMENT: LONG RUN EVIDENCE FROM CASH TRANSFERS IN UGANDA[*]

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Abstract:

How to stimulate employment and the shift from agriculture to industry in developing countries, with their young, poor, and underemployed populations? A widespread view is the poor have high returns to investment but are credit constrained. If so, infusions of capital should expand occupational choice, self-employment, and earnings. Existing evidence from established entrepreneurs shows that grants lead to business growth on the intrinsic margin. Little of this evidence, however, speaks to the young and unemployed, and how to grow employment on the extensive margin—especially transitions from agriculture to cottage industry. We study a large, randomized, relatively unconditional cash transfer program in Uganda, one designed to stimulate such structural change. We follow thousands of young adults two and four years after receiving grants equal to annual incomes. Most start new skilled trades. Labor supply increases 17%. Earnings rise nearly 50%, especially women's. Patterns of treatment heterogeneity are consistent with credit constraints being relieved. These constraints appear less binding on men, as male controls catch up over time. Female controls do not, partly due to greater capital constraints. Finally, we go beyond economic returns and look for social externalities. Poor, unemployed men are commonly associated with social dislocation and unrest, and governments routinely justify employment programs on reducing such risks. Despite huge economic effects, we see little impact on cohesion, aggression, and collective action (Peaceful or violent). This challenges a body of theory and rationale for employment programs, but suggest the impacts on poverty and structural change alone justify public investment.

JEL codes: J24, O12, D13, C93

1 Introduction

The process of development involves a shift in the structure of the economy from agricultural to non-agricultural pursuits (Clark, 1951; Fisher, 1945; Kuznets, 1966). This structural change, what ignites it, and what holds it back, has preoccupied generations of development economists and historians (Acemoglu, 2008; Herrendorf et al., 2013; Kuznets, 1973; Syrquin, 1988). Policymakers are interested in answers as well. A third of the world's population is 15 to 34 years old and lives in a developing nation. Most are underemployed, and their number is growing (World Bank, 2012). The problem of how to expand youth employment, especially non-agricultural work, in economies with few large firms, is a pressing global concern.

A chief explanation for underdevelopment is imperfect capital markets (Aghion and Bolton, 1997; Banerjee and Newman, 1993; Galor and Zeira, 1993; King and Levine, 1993; Piketty, 1997). In poor rural economies, credit constraints may slow investment in cottage industry or skills, as entrepreneurs must save to invest. If there are increasing returns to production, such as start-up costs, then credit constraints further slow or even trap the poor in subsistence farming or casual labor. If true, infusions of capital should increase occupational choice and spur investment and earnings, especially among poor new market entrants such as young people. This paper illustrates this claim with a simple model of occupational choice, and tests it with an experimental evaluation of a large cash transfer program to young men and women in Uganda.

Existing microeconomic evidence supports pieces of this theory, but direct evidence of credit constraints inhibiting new enterprise growth, occupational choice, and sector shifts is rather thin. A large literature has established that the poor have limited access to credit and insurance (Banerjee and Duflo, 2005; Karlan and Zinman, 2009; Townsend, 2011). Several experiments find high returns to grants of cash and in-kind capital to established business owners and farmers, especially among males (de Mel et al., 2008; Fafchamps et al., 2011; Udry and Anagol, 2006). These studies, however, mainly assess growth on the intensive margin. Growth in the size, productivity and formality of existing firms is a crucial element of modern economic growth (Kuznets, 1973), but it doesn't speak to the unemployed, to employment growth on the extensive margin, or to occupational change, especially the transition from farming to cottage industry.

There is a growing body of evidence on interventions targeting the poorest, but the impacts on income growth, occupational change, and sector shifts (and the role of credit constraints) are ambiguous. First, several microfinance experiments show, broadly speaking, increased borrowing

and investment in existing household enterprises (such as livestock raising), but little systematic effect on new enterprise growth, profits, or structural change studies (Angelucci et al., 2012; Attanasio et al., 2011; Augsburg et al., 2012; Banerjee et al., 2013; Crépon et al., 2011).

Second, several experiments study the impacts of giving ultra-poor rural people livestock, skills training, and income support. Generally speaking, over two to three years these programs increase assets and food security, but produce limited growth in agriculture, new businesses, or incomes (Banerjee et al., 2010; Goldberg, 2013). One exception is a study of Bangladeshi women by Bandiera et al. (2013). Alongside a rise in incomes, they see a shift in occupation within agriculture, from farm labor to rearing one's own livestock.

Finally, a large number of experiments study conditional cash transfer (CCT) programs, which tie transfers to child health and schooling obligations. Unfortunately, these seldom examine effects on investment or enterprise (Fizbein et al., 2009). The few that do reach opposing conclusions.[1] However, in at least one CCT program—*Progresa* in Mexico—there is evidence of an increase in self-employment as the transfers relieved financial constraints on credit and risk (Bianchi and Bobba, 2013; Gertler et al., 2012).

One could draw pessimistic conclusions on the potential for credit, asset, or cash transfers to change occupations and economic structure. It is possible that the role of credit constraints is overstated, or that other constraints bind the poor, such as self-control problems or social obligations and sharing norms (e.g. Banerjee and Mullainathan, 2009; Field et al., 2010). At the same time, these interventions may not offer an ideal test. With microcredit, for example, high interest rates, abrupt repayment plans, and low tolerance for default may mean that the credit constraint is not relaxed. Ultra-poor programs, meanwhile, constrain initial investments to livestock, which may not be the highest-return new enterprise, and may even impede sector change. Finally, it's possible that the framing and conditionality of CCTs constrain productive investment.

Ideally, we would like evidence on the effects of unconditional cash transfers on the poor, with attention to employment growth on the extensive margin—similar to the evidence on intensive growth with established entrepreneurs discussed above. To test the role of credit constraints, moreover, we can try to go beyond average treatment effects, and use rich pre-intervention data

[1] Two Nicaraguan experiments find no significant effect of the CCT on earnings and non-agricultural production (Macours et al., 2012; Maluccio, 2010) while a Mexican evaluation of the *Progresa* program estimates about a quarter of a conditional transfer was invested in informal enterprise (Gertler et al., 2012).

to identify the heterogeneous responses to treatment consistent with alternate constraints. Our model predicts, for instance, that if credit constraints bind then transfers should have the largest impacts on the poorest, the most able, the most patient, and those without an enterprise.[2]

We study an experimental program in Uganda's relatively underdeveloped north. In 2008 the government distributed cash transfers worth $382, roughly a year's income, to thousands of poor and underemployed youth aged 16 to 35. The government's explicit aims were to expand skilled non-agricultural employment, reduce poverty, and spur catch-up of the north with the rest of Uganda. The grants were unconditional, with two qualifications. First, the transfer was framed as an enterprise start-up program and (although it was understood there was no official monitoring of compliance) youth had to submit proposals for how they would invest in a trade. Second, partly for administrative efficiency, youth had to apply in small groups. Funds were distributed as a lump sum, which the group distributed among members, or spent together.

We collect data on a large sample over a long panel. From many thousand applicants, the government screened and selected 535 groups (about 12,000 youth, a third female). The average youth reported 19 hours a week and earned less than a dollar a day. The government randomly assigned 265 groups to the intervention. We survey a panel of 2,675 treatment and control youth three times: at baseline and two and four years post-intervention. While this is not very "long run" from the perspective of economy-wide structural change, it is longer run data than available from many program evaluations, and offers a window into the early stages of structural change.

This intervention offers several other advantages—large, relatively unconditional cash transfers; a long horizon; and a large sample of people of varying ages, poverty levels, and existing business ownership. In addition, our sample is in Africa, where we have little data of this nature.

In line with our model, treated youth invest most of the grant in skills and business assets. After four years, they are 65% more likely to practice a skilled trade, mainly small-scale industry and services such as carpentry, metalworking, tailoring, or hairstyling. They have much higher business capital stocks and earn high returns—real earnings are 49% greater than the control group after two years and 41% after four years. Real earnings continue to grow over the full four years. They are also about 40% more likely to keep records, register, and pay taxes.

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[2] De Mel et al. (2008) make a similar argument and finds some evidence to this effect among existing Sri Lankan entrepreneurs. Otherwise the evidence from heterogeneous treatment effects is fairly thin.

Further evidence of capital constraints comes from the fact that, in spite of the large income gains, labor supply increases 17%, almost entirely in these skilled trades. Farm work stays the same, and (at least after four years) these new skilled trades are mainly part-time occupations.

Patterns of treatment heterogeneity are also consistent with initial credit constraints, as the gains are largest among those with the fewest initial assets and access to loans, and the more patient and risk averse. Performance also improves with group's cohesion and homogeneity, suggesting that the group design could promote higher investment and returns.

In contrast to studies of existing entrepreneurs, however, women in this program earn high returns. In fact, the impact on females is actually greater in relative terms: after four years, treated female incomes are 84% greater than female controls (compared to a 31% relative gain for men). We also see evidence that credit constraints were more detrimental to young and unemployed women, and that without access to capital they are more likely than males to find themselves in a poverty trap—over the four years, males in the control group begin to catch up to their treated peers in investments and earnings, while females in the control group have largely stagnant capital stocks and earnings. The cash infusion produces a relative takeoff for women.

Finally, we examine non-economic impacts. Focusing on economic returns alone ignores potentially large social externalities to employment creation. Most governments (including Uganda's) have a second central motive for fostering employment: social cohesion and stability. Poor, unemployed young men are thought to weaken social bonds, reduce civic engagement, and heighten instability (e.g. Becker, 1968; Blattman and Miguel, 2010; Collier and Hoeffler, 1998; Goldstone, 2002; World Bank, 2012). We review a range of economic, political and psychological theories that support this view, theories bolstered by an abundance of evidence that incomes are correlated with social stability. Little of this evidence is causal, but if it's correct, it could justify larger public investments in occupational growth and change.

In spite of the large economic impacts, however, we see little experimental effect on our measures of social change. We collect a broad range of self-reported measures of family and community integration, community and national collective action, aggression, disputes with authorities, and attitudes toward and participation in violent protests. Although some of these are positively correlated with (non-experimental) changes in poverty and employment in our panel, the experimental treatment effects are small, close to zero, and not statistically significant.

Overall, these findings speak to a number of questions and literatures. First, we provide some of the first micro-level evidence for macro theories of development that emphasize how low income and credit constraints hold back occupational choice and economic structural change, in this case the transition from agriculture to cottage industry (Aghion and Bolton, 1997; Banerjee and Newman, 1993; Clark, 1951; Fisher, 1945; Galor and Zeira, 1993; Kuznets, 1966).[3]

Second, our evidence strengthens the case that women can be successful entrepreneurs. This is important because, while the existing evidence is optimistic that women invest income gains in children and pro-socially (e.g. Duflo, 2012), it is pessimistic about women's ability to invest aid to increase lifetime earnings (e.g. Cho et al., 2013; e.g. Fafchamps et al., 2011; Field et al., 2010). In this view, women's investment and occupational choice is constrained by social mores or pressure to share windfalls. Our results suggest a cash transfer is sufficient to create sustained growth in women's earnings. Our results could be a product of the context, or the fact that this is a poorer and unemployed (i.e. more constrained) population. Since the female impacts take four years to emerge, however, our findings may be a product of a longer-than-usual panel.

Third, returns are high in spite of the intervention's emphasis on investing in vocational training. The evidence on job training programs in the US, Europe and Latin America is famously pessimistic (e.g. Card et al., 2009), even in the very rare cases of low-income countries (Cho et al., 2013). Our evidence suggests that capital is the binding constraint and providing the means to purchase skills and tools was crucial to success in trades.

Finally, while the economic case is strong, the social externalities (at least the ones we measure) are not. It may be that the theoretical links between poverty and social instability are weaker than generally believed, or that third forces drive a spurious correlation between violence and poverty. Other experiments by one of the authors come to similar conclusions (Blattman and Annan, 2011; Blattman et al., 2013). Ultimately, more causally identified evidence is needed.

Other important questions for future research are the extent to which the framing and design of the intervention (an unenforced "pre-commitment" to invest funds) and the group nature of

[3] There is little evidence of growth on the extensive margin or non-traditional sectors, especially changes of these magnitudes, but (as noted above) three recent papers draw conclusions related to ours. Bandiera et al. (2013) show that livestock transfers shift occupations within the agricultural sector from labor to livestock-rearing. Bianchi and Bobba (2013) use observational data to show how CCTs enable a small shift to self-employment, though the sectoral shift is unclear. Macours et al. (2012) show that a grant on top of a CCT modestly increases non-farm earnings.

disbursement drove high levels of investment. We discuss related work in Uganda, however, that indicates monitoring and accountability has little impact on investment (Blattman et al., 2013).

Overall, the results suggest that aid or domestic redistribution can increase economic efficiency and growth if it is used to relieve constraints on the underemployed. Unconditional cash grants are not only an effective mechanism to do so, but are likely to be significantly cheaper to implement than CCTs or asset transfers. Unfortunately, we see few effects on social stability, so the case for such public investments is this economic one alone.

2 Intervention and experimental design

Uganda is a landlocked East African nation of roughly 36 million people. Growth took off in the late 1980s with the end to a major civil war, a stable new government, and reforms that freed markets and political competition. The economy grew an average of 7% per year from 1990 to 2009, putting per capita income 8.5% ahead of the sub-Saharan average (World Bank, 2009).

This growth, however, was concentrated in the south and central regions. The north, home to a third of the population, lagged behind, largely because it was plagued by insecurity from the 1980s until about 2006. In the north-central region, an insurgency lasted from 1987 to 2006. Conflicts in south Sudan and Democratic Republic of Congo also fostered insecurity in the northwest, while cattle rustling and armed banditry persisted in the northeast. In 2003, however, 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, began to disarm cattle-raiders, and increased control. The northern economy began to catch up.

The centerpiece of the government's northern development and security strategy was an cash transfer program hundreds of millions of dollars in size, the Northern Uganda Social Action Fund, or NUSAF (Government of Uganda, 2007). Starting in 2003, communities and groups could apply for cash transfers for either community infrastructure construction or income support and livestock for the "ultra-poor". After some years, however, the government decided to do more to boost non-agricultural employment and expand small industry and trades. To do so, in 2006 it announced a new NUSAF component: the Youth Opportunities Program (YOP).

2.1 The YOP Intervention

The intervention offered young adults aged roughly 16 to 35 the opportunity to apply to their district government for large cash grants. The express goal was to allow them to shift from agriculture and casual labor into semi-skilled "cottage" manufacturing and services—trades such as woodworking, metalworking, tailoring, and hairstyling.

These were largely unconditional transfers, with two exceptions. First, youth were required to form and apply in small groups. Groups ranged from 10 to 40 youth, with an average size of 22. One reason is administrative convenience: it was easier for the government to verify and disburse to a few hundred groups rather than thousands of people. Another reason is that, in the absence of official monitoring, group decision-making could act as a commitment device. We return to the theoretical effects of the group design below.

Second, groups had to submit a proposal that specified member names, a management committee of five members, the trade they proposed to train and practice in, the assets they would purchase, and a budget. "Facilitators"—usually a local government employee or civil society member—helped groups prepare proposals. In practice most groups proposed just one or two trades and selected their own training organizations, typically a local artisan or small institute.

Successful groups received a lump transfer to a bank account in the names of the management committee, with no government role thereafter. Groups were responsible for disbursing funds, accountable only to each other. There was no central monitoring or enforcement. As this is one of the largest, most well known aid programs in the country, in place for several years before this study, applicants were most likely aware of this absence of official accountability.

The average transfer was $7,497 per group, or $382 per member in 2008 dollars and exchange rates ($763 at PPP rates). This per capita grant is roughly equal to a youth's baseline annual income. Per capita grant size varied across groups, however, with 80% between $200 and $600. Variation is driven by differences in group size and amount sought (see Web Appendix A). No more than one group in a community was funded, and these small towns are large enough (typically hundreds or thousands of households) that general equilibrium impacts are likely small.

2.2 Participants

Tens of thousands of people applied. Half of all groups existed previously, as sports or religious or community youth clubs. While youth self-selected into the program, the government

also gave financial incentives to local leaders to mobilize and help youth to develop proposals. This resulted in a large number of poorer, less educated youth entering the pool. Hundreds of groups were funded before this study (all outside our sample of communities). In 2007 the government had funds for 265 more groups in 13 districts. It asked each district to nominate two to three times as many groups as there was funding, and audited submissions. We observe 535 groups after this screening. Figure I presents groups per parish (an administrative unit including about a dozen villages). Web Appendix A describes more selection and design details.

A third of group members are female. The average applicant was above the average wealth and education level in the north but most are poor by any measure—a quarter did not finish primary school, baseline incomes average about a dollar a day, and they report less than 19 hours of employment a week at baseline (full baseline statistics in Web Appendix B).[4]

Credit constraints are severe. Few formal lenders had a presence in northern Uganda at the time of the intervention. Village savings and loan groups were common, but loan terms seldom extend beyond two months with annual interest rates of 100 to 200%. Just 11% of the baseline sample had savings, $22 at the median. A third had loans, under $6 at the median, mainly from friends and family. Less than 10% borrowed from an institution, just $17 at the median.

Farming and animal husbandry are the most common occupation. For instance, by 2012, the control group spent 43% of all work hours in agriculture. Northern Uganda has relatively low levels of land and income inequality. Farming is rudimentary, a mix of subsistence and small cash crop production, on small rain-fed plots with little equipment or inputs. Otherwise, 22% of employment hours are in low-skill non-agricultural work (such as petty trade or casual labor), and 14% in skilled non-agricultural work (such as tailoring, carpentry, or salaried jobs like teaching). Web Appendix B lists detailed occupational statistics at baseline and the four-year endline.

2.3 Experimental design and estimation

Given oversubscription, we worked with the government to randomly assign 265 of the 535 groups (5,460 individuals) to treatment and 270 groups (5,828 individuals) to control, stratified

[4] We compare our 2008 baseline data (described below) to representative surveys: the 2004 Northern Uganda Survey (NUS), the 2006 Demographic Health Survey (DHS), and the 2006 Uganda National Household Survey (UNHS). Compared to their age cohort, our sample were four times more likely to have had some secondary and 15 times less likely to have no education. They are also more likely to own assets like mobile phones and radios.

by district. Despite the scale of the intervention, we judge spillovers unlikely: The 535 groups were spread across 454 towns and villages, in a population of more than 5.4 million. Overall, baseline variables are balanced, although there is modest imbalance on baseline wealth and savings variables, with treatment group members slightly wealthier (details in Web Appendix B).

We define treatment compliance narrowly: all individuals in the group are coded as compliers, or "treated", if administrative records indicate the group received the transfer and if our survey indicates those funds were not diverted by district officials. 29 groups (11%) were not treated: 21 could not access funds due to unsatisfactory accounting, bank complications, or collection delays; and 8 groups reported they never received due to some form of diversion.

Our preferred ATE estimator is the average treatment effect on the treated (ATT), where being treated, T_{ij}, is instrument with the random assignment to treatment, A_{ij}. Non-compliance is small and unsystematic, so ATT and intention to treat (ITT) estimates are similar. We have data at baseline ($t = 0$), and the 2- and 4-year endlines ($t = 1,2$) for each person i in group j and district d. We estimate the 2-year ATE (θ_1) and the 4-year ATE ($\theta_1 + \theta_2$) jointly, as follows:

$$Y_{ijtd} = \theta_1 T_{ij} + \theta_2[T_{ij} \times 1(t = 2)] + \beta_1 Y_{ij0} + \beta_2 X_{ij0} + \beta_3 1(t = 2) + \alpha_d + \rho_j + e_i + \varepsilon_{ijt} \qquad (1a)$$

$$T_{ijtd} = \pi_1 A_{ij} + \pi_2[A_{ij} \times 1(t = 2)] + \delta_1 Y_{ij0} + \delta_2 X_{ij0} + \delta_3 1(t = 2) + \gamma_d + v_j + u_i + \mu_{ijt} \qquad (1b)$$

where Y_{ijtd} denotes an outcome variable at time t. X_{ij0} is a pre-specified set of baseline covariates (used to correct for any covariate imbalance), α_d and γ_d are stratum (district) fixed effects, ρ_j and v_j are group error terms (i.e. accounting for clustering), e_i and u_i are individual error terms (since there are two observations per individual for $t = 1,2$), and ε_{ijt} and μ_{ijt} are i.i.d. error terms. We will show alternative estimators have little material effect on the findings and conclusions.

3 Theory

In well-functioning markets, entrepreneurs choose their capital stock so that the marginal return equals the market interest rate. If they receive a cash windfall, investing it would drive marginal returns below that interest rate. The optimal strategy is to consume and save it. A windfall of capital goods forces suboptimal investment. Earnings rise until the entrepreneur divests. Thus transfers will not increase investment and profits unless there are market imperfections.

Market imperfections are commonplace, of course, especially credit markets. Our theory and tests focus on credit imperfections given the severe credit issues in northern Uganda highlighted above. Insurance market failure is also an important potential imperfection, and we discuss and analyze the role of risk below. We focus on credit imperfections, however, for two reasons. First, a single cash windfall is better suited to test credit market rather than insurance market imperfections (Bianchi and Bobba, 2013). Second, risk and imperfect insurance is most relevant when entrepreneurs choose between a safer, low-return activity and a riskier, high-return activity. In northern Uganda, however, skilled trades appear to have similar or lower risk than more traditional occupations. For instance, as discussed below, treatment slightly reduces the standard deviation of earnings compared to the full sample at baseline or the control group at endline.

3.1 A simple model of occupational choice with cash transfers under credit constraints

Setup—To structure our thinking we develop a simple two-period model of occupational choice in imperfect markets: there is no borrowing possible and production faces non-convexities (start-up costs) with inputs. Individuals have initial wealth w. Everyone can perform unskilled labor and earn a fixed y each period, or to become an entrepreneur, and earn $f(A, K)$, where f is a production function increasing in inherent ability, A, and capital stock, K. Becoming an entrepreneur has a fixed cost $F \geq 0$, which does not go into productive capital. Existing entrepreneurs have already paid F and have initial capital, $K_0 \geq 0$.[5]

Individuals save s at interest rate $1 + r$. To model credit constraints we assume $r = 0$ and that people cannot borrow. While a simplification, these assumptions are not farfetched: real interest rates on savings are often negative due to fees and inflation, and borrowing is prohibitively costly, as we note above. Adding borrowing at high rates would not change our conclusions.

In this setup, individuals choose s and K to maximize their (concave) utility function, $U = u(c_1) + \delta_i u(c_2)$, where c_t is consumption in period t and δ_i is individual i's discount rate. "Laborers" solve U s.t. $c_1 + s = y + w$, and $c_2 = y + s$. "Budding entrepreneurs" solve U s.t. $c_1 + s + F +$

[5] The model was developed jointly with Julian Jamison and is shared by a related study of poverty alleviation in Uganda (Blattman et al., 2013). It could be considered a two-period version of the one-period investment model in de Mel et al. (2008), and is influenced by a two-period microcredit model presented in an early unpublished version of Banerjee et al. (2013). Bianchi and Bobba (2013) develop a similar two-period model that more formally incorporates risk, and assumes that the non-entrepreneurial activity is less risky.

$K = y + w$, and $c_2 = f(A, K) + s$. Finally, "existing entrepreneurs" solve U s.t. $c_1 + s + K = f(A, K_0) + w$, and $c_2 = f(A, K + K_0) + s$. All are also constrained by $s \geq 0$, by our assumption.

Implications—Figures II-IV illustrate a stylized solution. Figure II ignores existing entrepreneurs and looks at initially low w individuals (w_L) who are laborers in period 1 and may choose to be entrepreneurs in period 2. Point E represents the endowment, and saving corresponds to the -45° line from E to the vertical axis. If they start an enterprise, they pay F and invest K, which pays $f(A, K)$ in period 2. We assume $f(\cdot)$ is concave (i.e. decreasing returns) and is increasing in both A and K. Figure II depicts a relatively high-ability entrepreneur with high potential returns.

Considering the w_L case, we can see that different indifference curves (corresponding to high and low discount rates, δ_H and δ_L) will lead to different choices between labor and enterprise, with entrepreneurship more likely among the patient. If δ and w are low enough, individuals will consume and produce at E. Entrepreneurship is more attractive with larger is A and smaller is F.

Next consider the higher wealth case, w_H. This could represent receipt of a cash windfall. Fixing A, there is a smaller range of δ for which the agent will choose to be a laborer: patience or ability would have to be especially low. Intuitively, everyone wants to smooth their consumption unless they're very impatient. The higher is w, the more individuals want to smooth, and capital investment typically gives a better return than saving (depending on A).

Figure III illustrates the difference between high and low ability (A_H and A_L). While magnitudes depend on the shape of production and utility, we still see a few general patterns. Patient individuals remain laborers if the returns to their ability are low enough. Generally, higher ability and patience people should come with a larger increase in period 2 earnings and consumption.

Figure IV considers existing versus budding entrepreneurs, focusing on high ability individuals. They have paid F and so their production function shifts right. Cash transfer impacts on period 2 earnings and consumption is lower for existing entrepreneurs, especially less patient ones.

Do larger grants result in more investment and earnings?—Recall that there are wide ranges in per person grants. We should not necessarily expect proportional increases in investment and earnings, however. Entrepreneurs invest until the marginal return to capital (MRK) equals the marginal rate of substitution (MRS) between periods (since we assumed $r = 0$). For small enough grants, MRK>MRS, and investment and income will increase with grant size. Once MRK=MRS, however, any additional windfall will be consumed or saved. Overall, we should expect to see

some relationship between grant size and returns (especially if the grant request and potential returns are positively correlated) but if the MRK falls quickly enough, this relation will be weak.

Risk—Another potential market imperfection is imperfect insurance. When individuals are risk averse, investment is less attractive because the certainty equivalent of uncertain income is less than the expected value. Given a cash windfall, more risk-averse individuals will be less likely to invest it if the new investment is riskier than the current occupation. Thus the impacts of cash transfers may decrease in risk aversion.[6]

Our data suggest that the standard deviation of log income for skilled professions is no greater than that of agricultural or low-skill occupations, comparing treated individuals before and after treatment, or comparing treatment and control post-treatment (not shown). This mitigates the effect of risk aversion. Nonetheless, to the extent people do not know their post-treatment returns, risk aversion should continue to play some of the predicted role.

3.2 Possible effects of group disbursement

Groups could play three possible roles. The first is negative: we may worry that leaders capture grants. Second, and more optimistically, groups may act as a commitment device in the spending of the windfall. For instance, the group commonly made payments for training and tools all together. Peer pressure may also promote investment. In our model, this could reduce the role of individual time preferences, δ, in the likelihood people pay F to become entrepreneurs. It is less likely, however, that the group affects longer-term re-investment of retained earnings. Over time, including our 4-year timeframe, patience should play a stronger role: low-patience individuals will be more likely to divest or let assets depreciate without reinvestment. Eventually they should resemble the low-patience existing entrepreneurs in Figure IV. This is the same position we would expect them to occupy if the windfall is large relative to F, in which case even low patience individuals have the incentive to pay F and become entrepreneurs.

Finally, groups may offer production complementarities. Most post-intervention enterprises are individual rather than group-based, so individual production functions probably remain the

[6] Proofs available on request. See de Mel et al. (2008) for an analogous one-period model that illustrates this point.

right framework for thinking about intervention impacts.[7] But some groups share tools and physical capital (e.g. a building, or high-value tools), which could increase returns.

It is easiest to test the elite/leader capture hypothesis, as we can test for disproportionate investments and profits (as well as ask other group members). The other hypotheses are not directly testable. Nonetheless, we can look for indirect evidence based on baseline data on group quality, cohesion and composition. In particular, we hypothesize that the extent to which groups act as effective commitment devices and effectively share tools and raise shared capital (and returns) is increasing in levels of group cohesion and quality.

3.3 Should we expect high returns from this intervention?

The government's program design raises several concerns. First, youth were constrained from proposing alternatives to a skilled trade (e.g. grain mills or mining equipment), and so trades might be suboptimal choices for some. Second, it is not clear the most common vocations—carpentry, hairstyling, and especially tailoring—yield high returns. In particular, women in Uganda tend to choose strikingly "gender stereotypical" trades, mainly hair salons and tailoring. Finally, even if a handful of tailors could make a living, can most of the small towns in our study support 20 new tailors? As we will see below, most groups trained in the same trade, often in small towns of 500 to 2000 households (though larger towns are in our sample). If true, all of these forces should depress returns to capital from the intervention, or increase incentives to deviate from the proposed budget to save and consume (especially the larger grants) or invest in non-vocational businesses. In Figures II-IV, this is analogous to reducing the slope of the production function, reducing investment, entrepreneurs, and period 2 incomes.

4 Data and measurement

The 535 eligible groups contained nearly 12,000 members. We survey a panel of 2,675 people (five per group) three times over four years. We first conducted a baseline survey in February

[7] After two years, 14% of the treated report coming together for income-generating activities on a daily basis, and 30% report coming together once a week for this purpose. Of those that come together daily, 75% report some shared tools while 85% of those that come together weekly report some shared tools.

and March 2008. Enumerators were able to locate 522 of the 535 groups.[8] They mobilized group members—typically about 95% were available—to complete a group survey that collected demographic data on all members as well as group characteristics. We randomly selected five of the members present to be surveyed and tracked them over future years.

The government disbursed funds July to September 2008. Groups began training shortly thereafter, and most had completed training by mid-2009. We conducted the first "2-year" endline survey between August 2010 and March 2011, 24-30 months after disbursement. We conducted a second "4-year" endline survey between April and June 2012, 44-47 months after.

We attempted to interview all 2,675 people each round. Initially, we made one to two attempts to find every person at their last known location. We then selected a roughly 50% random sample of unfound people (e.g. migrants) for intensive tracking, often in another district. The effective response rate is 91% after two years and 84% after four.[9] Though these attrition rates are low relative to most panels of this length, there is a slight correlation with treatment status: the treated were 5 percentage more likely to be unfound after two years (significant at the 1% level) but 3 percentage points less likely after four years (not significant). Attrition is slightly higher among males, but otherwise relatively uncorrelated with baseline data, suggesting that it is relatively unsystematic. Web Appendix C describes these levels and correlates of attrition in detail.

4.1 Main economic outcomes

We have three main, pre-specified outcomes of interest motivated by the model: investment; occupational choice/levels; and income. Summary statistics and simple treatment-control differences are presented in Table I. The survey also measured several pre-specified secondary outcomes, all of which are presented in Table I or the Appendices to avoid selective reporting.[10]

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[8] Across all three survey rounds we were unable to locate 12 of the 13 missing groups, suggesting they may have been fraudulent "ghost" groups that slipped through the auditing process. Unusually, all 13 missing groups had been assigned to the control group and so received no funding. This appears to be a statistical anomaly. District officials and enumerators did not know the treatment status of the groups they were mobilizing.

[9] We conduct two-phase tracking, where all respondents are sought in Phase 1 and Phase 2 selects a random sample of unfound respondents and makes three attempts them to track them to their current location. Phase 2 respondents receive weight in all analysis equal to the inverse of their sampling probability. This sampling technique optimizes scarce resources to minimize attrition bias (Gerber et al., 2013; Thomas et al., 2001). See Web Appendix A for a study timeline and Web Appendix C for analysis of attrition rates and patterns.

[10] The distinction between primary and secondary outcomes, and their pre-specification, comes from (i) the model, (ii) a pre-analysis plan created before designing the 2-year endline survey, and (iii) the formal analysis of 2-year

To measure investment, respondents self-report the *Hours of training received* between baseline and the 2-year endline (2Y) and their estimate of the *Real value of their business asset stock* (the sum of the value of all raw materials, tools and machines) in real 2008 Ugandan Shillings (UGX). We measure skills and capital separately and in different units because we could not obtain reliable individual estimates of the training cost in the endline survey, and because groups disbursed funds among members in diverse ways and seldom keep records.

To measure occupational choice and employment levels, we construct indicators for whether the person's *Main economic activity is non-agricultural*, and whether they are engaged in any *Skilled employment*—including trades and salaried higher-skill wage work (such as teaching) but excluding non-agricultural casual labor or petty business (See Web Appendix B breakdowns by occupation). We also look at levels of employment through the *Hours of agricultural work in the past 4 weeks*, *Hours of non-agricultural work in past 4 weeks*, and *Total employment hours*.

Our main income measure is monthly *Net earnings,* in real 2008 UGX. We ask respondents to estimate their gross then net earnings for each business activity and calculate the sum over all activities. Since earnings can be a noisy measure of permanent income, we complement it with three other measures. First, we construct an *Index of wealth* z-score using 70 measures of land, housing quality, and durable assets. At 4 years we also have a measure of *Short-term expenditures* in UGX based on 58 types of short-term non-durable expenses. Finally, we measure *Subjective well being* by asking respondents to place themselves (relative to the community) on a 9-step ladder of wealth. Web Appendix D has more measurement details on main outcomes and Web Appendix E describes secondary economic outcomes, some of which are listed in Table I.

4.2 Measurement error and ATE estimation

Since outcomes are self-reported, we will overestimate the ATE if the treated over-report 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

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results, all of which the authors documented in a World Bank discussion paper (Blattman et al., 2011). Thus the most strongly and officially pre-specified results are the 4-year ones.

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.

Another source of measurement error comes from extreme values. All our UGX-denominated outcomes have a long upper tail, and extreme outliers are highly influential in any ATE. We take three steps to minimize this problem: first, we top-code all UGX-denominated variables at the 99th percentile; second, we examine the ATE of the logged value.[11] Third, in the Web Appendix, we examine treatment effects at the median and other quantiles.

5 Results of the intervention

5.1 Investment

Most of the treated enroll in training, and a majority of the transfer is spent on fees and durable assets. Table II displays 2- and 4-year ATEs for investments, estimated using Equation 1. It also displays ATEs by gender. In each case we show the control group mean and the percentage change represented by the ATE.[12]

Skills training—Between baseline and the 2-year endline, 76% percent of the treated enrolled in technical or vocational training, compared to 15% of the control group. Treated males and females have similar enrolment levels. On average, being treated translates to 389 more hours of training than controls (Table II). The effect is almost identical for males and females.

Most groups (85%) train in a single skill, and most pursue the same few trades. Among the treated, 38% train in tailoring, 24% in carpentry, 13% in metalwork, and 8% in hairstyling. Women predominantly choose tailoring and hairstyling. Of the 15% of control group members who get training (in spite of not receiving a cash grant) most train in the same skills as the treated, though the trainings tend to be much shorter. About 40% of these pay their own way, and the rest receive training from a church, government extension office, or non-governmental organiza-

[11] Since some respondents report zero earnings or consumption, for a UGX-denominated variable x we actually calculate log(100 UGX + x), where 100 UGX is the smallest unit of Ugandan currency (about five cents). In Web Appendix I we show the results are robust to a standard log transformation or other non-linear transformations defined at zero, such as the inverse hyperbolic sine. We use log(100+x) in this paper mainly for its ease of interpretation.

[12] For logarithmic dependent variables with ATE estimate θ, this is calculated as $\exp(\theta - \frac{1}{2}\text{Var}(\theta)) - 1$.

tion (NGO). Thus, even among controls motivated enough to apply, just 6% could self-pay the vocational training absent a transfer. Web Appendix F has further analysis of training choices.

Capital investments—We also see a large initial increase in capital stocks, flattening out among the treated (or even declining slightly) between the 2- and 4-year endlines. Figure V illustrates cumulative distribution functions (CDF) for the natural log of the stock of business capital, including goods and tools. In Figure Va, we see that the distribution of capital is greater for treated males and females, but that there is some catch-up by the control group after 4 years (especially a fall in the number reporting no capital).

From Table II, the control group reports UGX 299,400 ($174) of business assets at the 2-year endline and 392,400 ($228) at the 4-year (larger among males than females). The treated report 470,950 ($274) more stock after two years, a 157% increase over the control group, and 200,641 ($117) more after four years, a 51% increase over the control group. These control means and level ATEs are pulled up by extreme values, however. Since capital stock is roughly log-normally distributed, we also look the log of the stock. We see a 1.84 log point increase in business assets after two years and a 1.033 log point increase after four.

What proportion of the grant was invested?—Treated groups reported that approximately a third of the YOP transfer was spent on fees for skills training. The ATE on business asset stock is 70% of the average per person grant. This capital stock includes reinvested earnings, however, and so overstates investment of the initial grant. Nonetheless, it indicates a majority of the grant reflected investment in becoming a skilled entrepreneur. Either self-control issues are less prevalent than often feared (at least with large transfers), or the intervention design—the proposal or group organization—may have acted as a commitment device. We return to this question below.

Other aid received—Finally, we check whether treatment or control group members were more likely to receive other forms of government or NGO aid. At the 2-year survey we asked respondents whether they had received other financial assistance or programs and its approximate value. We do not see a statistically significant difference (Table II).

5.2 Employment and occupational choice

With these investments, we see a shift in occupational choice towards skilled work and cottage industry (Table III). After 4 years, 29% of the control group reports their main occupation (by hours) is non-agricultural (unskilled or skilled). Treatment increases this proportion 11 percent-

age points (pp), or 38% relative to controls. This change is proportionally much greater among females (a 51% relative increase). 42% of the control group does any skilled non-agricultural work at all (i.e. trades or skilled salaried work), and treatment increases this by 27.5 pp, or 65%. Again, the proportional change is much larger among females (122% relative increase).

This shift does not come at the expense of agriculture. After 4 years, the both treated and control youth spend roughly 19 hours a week on some form of agriculture. On average, controls spent an additional 16.5 hours per week on all non-agricultural activities, but just 7 of these are in skilled trades or high-skill salaried work (see Web Appendix B for a detailed breakdown). Treatment has almost no effect on agricultural hours, but increases non-agricultural hours by 5 hours to a total of 22 hours per week (a 33% increase relative to controls), proportionally much greater among women (80%). This is entirely concentrated in high-skill activities, a 73% increase relative to controls. This ATE is roughly constant between the two- and four-year surveys.

While treatment is clearly tipping more people into high-skilled employment, this high-skilled employment is only occupying just 26% of the average treated person's employment per week. We also don't see evidence this amount is growing from two to four years (though, as we see below, incomes and hourly wages are growing from this activity over the same period, implying productivity is improving). A modest number are completely changing their occupations, but most are simply adding this new high-skill trade to their portfolio of income generating activities—a common practice in risky environments among the poor and nascent "middle class" (Banerjee and Duflo, 2008, 2007). It could also indicate constraints on further business growth, though four years is a still short horizon to evaluate this claim.

Another important observation is that the treatment group increases their labor supply in response to the increase in capital, both men and women. Overall, these increases are consistent with individuals being constrained before the grant, as labor supply increases occurs in spite of the rising desire for leisure that comes with increased earnings.

Finally, these enterprises are more likely to be formal. Treated individuals are 12 pp more likely to keep records (a 39% increase over controls), 5.1 pp more likely to register their business (a 34% increase) and 8.4 pp more likely to pay business taxes (a 40% increase). They are no more likely to have non-family employees, however. Web Appendix F describes these results.

5.3 Income and poverty

These investments translate into large earnings gains after two and four years. To see this, Figure Vb displays CDFs of log real earnings by gender and treatment status, Figure VI displays the levels and trends over time for real earnings in levels and logs, and Table IV calculates ATEs for all, including difference-in-difference estimates for earnings.[13]

Male earning levels are greater than female earnings in every period, the treatment effect appears to be large for both genders, and we see substantial catch-up by the control group between two and four years, primarily among males (Figure Vb). In the full sample, monthly real earnings increase by UGX 17,785 (about $10) after two years and 19,878 ($12) after four years, corresponding to 49% and 41% increases in income relative to the control group means (Table III). We cannot reject the hypothesis that the earnings ATE is equal at two and four years, or that it is the same for both genders. The ATEs on log earnings tell much the same story.

A few additional points are worth noting. First, as noted above, the "wage" (technically, average earnings per hour of employment) is 0.17 log points greater in the treatment group, indicating an increase in productivity. Second, we see large earnings and wage gains in spite of the potential program weaknesses: encouragement to invest in a narrow set of trades, a large number of people in one parish trained in a skill. Third, the different trades provide similar returns. Occupational choice of trade is endogenous to ability and other unobserved traits, and so trade-specific returns cannot be causally identified. Nonetheless, while earnings in male-dominated trades like carpentry are highest, tailoring and hairstyling still yield similarly high earnings whether a man or a woman is practicing (see Web Appendix G). Finally, a simple calibration exercise suggests that the bulk of the treatment effect is due to investments in physical capital (Web Appendix H).

We also consider the effects on consumption in Table IV. The results echo the earnings results: after 4 years, the treatment group's wealth index is 0.2 standard deviations (SD) greater than the control mean and short-term consumption increases by 14% relative to controls. Both treatment effects are greater for women (in absolute and relative terms) but not significantly so. Real savings also increase by 0.57 logs. Web Appendix F considers added economic outcomes.

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[13] Gross and net cash earnings were measured at the 2-year endline, but only gross earnings were measured at baseline. To approximate baseline net earnings, we apply the ratio of net to gross earnings at endline to the baseline data (roughly 0.75). Thus we must take Figure VI and the baseline differences-in differences estimate with some caution.

Finally, consistent with these income and wealth gains, the treated subjectively perceive themselves as doing economically better—14% more so after two years and 16% after four.

5.4 Return on investment

Overall, returns are high: The average treatment effect on net earnings in Table III represents a 40% annual return on the average transfer per group member.[14] This return may include added inputs, such as additional labor. If we adjust earnings to remove "wages" paid for hours worked, however, the treatment effect is larger on average.[15]

These returns are large relative to the real commercial lending rates of 10 to 30% that appear to be common among medium-size firms in Uganda. They also approach the high average returns to capital recorded from cash grants existing microenterprises in Sri Lanka, Mexico, and Ghana (de Mel et al., 2008; McKenzie and Woodruff, 2008; Udry and Anagol, 2006).

5.5 How do economic impacts vary with the size of the cash grant?

Recall there was a reasonable amount of (potentially endogenous) variation in grant size per person. In Table V we look at the (not causally identified) correlation between grant size per person and a selection of outcomes. We consider five outcomes—the logs of real capital stock, earnings, short-term expenditures, and savings, and a wealth index. Pooling both endlines, we regress each outcome on the log of the grant amount for treated groups only (controlling for demographic characteristics and baseline human and physical capital). A 1% increase in grant size is associated with an increase of 20% in capital stocks, 14% in earnings, 4% in short-tem expenditures, and 41% in savings, but a 0.54 SD decrease in wealth. The standard errors are wide, however, and so none of these estimates are statistically significant. One interpretation is that returns to investment are positive but diminishing in the size of the grant. It may also be that there are sig-

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[14] The average transfer amount was UGX 656,915 ($382) per group member and the monthly real earnings ATE is 17,785 ($10.33) after two years and 19,878 ($11.55) after four, all in 2008 real terms. These treatment effects are reasonably constant, so it might be fair to suggest the grant yields a constant real earnings impact of UGX 18,831 (the average of the two treatment effects). If we ignore heterogeneity in transfer amounts received, the ATE represents a monthly return of 2.87%—an annual rate of return of 40.4%.

[15] We do not have data on wages for all, and so we use data from the control group to predict a wage for each individual based on age, gender and education. We calculate a measure of *Adjusted earnings* for all treatment and control individuals by subtract from net earnings the product of the estimated wage and total employment hours (see Web Appendix F). The 2-year ATE for males and females is UGX 18,110 and the 4-year ATE is UGX 27,835.

nificant market barriers to expansion, or that grant recipients face other barriers to expanding their business. Alternatively, the results could reflect precautionary savings or for investment in alternative future businesses should this one fail.

5.6 Robustness and bounding

The asset stock and net earnings ATEs are robust to alternative specifications, including the omission of all control variables, an intent-to-treat ATE, weighting schemes, and relaxation of the top-coding of extreme values (Web Appendix I). Generally the size and the significance of results do not change. An exception is the change in top coding: eliminating top coding increases the ATE. Hence the estimates reported in Tables II to IV are the more conservative ones.

The same qualitative conclusions and statistical significance also hold for treatment effects at the median and other major quantiles. The median treatment effect on net earnings is UGX 8,200, approximately half the ATE (see Web Appendix I for details).

We also bound the treatment effects for potential attrition bias. Attrition is relatively unsystematic and uncorrelated with treatment (Web Appendix I). Nonetheless, the ATE can be overstated if unfound treatment individuals are possess lower potential returns than unfound controls. To bound the ATE, we impute outcome means for the unfound individuals at different points of the found outcome distribution. The most extreme bound, similar to Manski (1990), imputes the minimum value for unfound treated members and the maximum for unfound controls. Following Karlan and Valdivia (2011), we also calculate less extreme bounds for several variables, including net earnings. Detailed results are in Web Appendix I. In general, the ATE is robust to highly selective attrition, such as the assumption that attritors in the control group have the mean plus 0.25 SD better outcomes). Manski bounds include zero, however.

6 Impact heterogeneity: Lessons for credit constraints, gender, and groups

6.1 The role of credit constraints

To the extent credit constraints restrict our sample, our model suggests we should also observe the following patterns: (i) investment and earnings ATEs will be higher among the "most constrained"—those initially without a vocation, with low capital/wealth, and the more risk averse; and (ii) these ATEs will be higher among those with the highest ability (i.e. highest po-

tential returns), the more patient, and the least risk averse. Irrespective of credit constraints, we should also observe: (iii) investment and earnings increase with baseline capital/wealth, ability, and patience.[16] Impact heterogeneity is not identified, however, and can only provide suggestive support for (or against) the model. This experiment has four advantages, however: ex-ante predictions generated by past studies, a large sample size, a long horizon, and rich baseline data.

Table VI examines impact heterogeneity on logged real earnings (Web Appendix J shows very similar results with investment). We look at heterogeneity along five dimensions: (1) an indicator an *Existing skilled trade* at baseline; (2) a *Working capital z-score* summarizing initial asset wealth, savings and lending, and perceived credit access; (3) a *Human capital z-score* summarizing education, working memory, and health; (4) a *Patience z-score* summarizing 10 self-reported measures of patience, and (5) a *Risk aversion z-score* summarizing eight self-reported attitudes to risk.[17] We examine each form of heterogeneity individually and altogether. (Individually the test may be higher powered, while altogether is lower powered but less biased.) Column 1 considers the full sample, while the remaining columns look only at those without a skilled trade.

First, those with a vocation at baseline have 0.76 logs greater real earnings (Column 1). As predicted, treatment has a smaller impact on these existing entrepreneurs: the coefficient on the interaction is negative, large, significant and nearly as large as the ATE (-0.858 logs).

Second, as predicted, treatment effects are highest among the most credit constrained. Those with more capital have higher earnings: a one SD increase in initial working capital and credit access is associated with much higher earnings (0.517 logs, in Columns 1 and 2). But the coefficient on the interaction terms is large and negative: -0.195 logs in the full sample (Column 1) and -0.244 logs among those without an existing vocation (Column 2). These are statistically significant. When we include all interactions, however, the magnitude of the coefficient stays the same but we lose statistical power (Column 6).

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[16] These predictions parallel a one-period model of grants from de Mel et al. (2008) and a two-period model of microfinance by Banerjee et al. (2013). The former finds some support for their predictions.

[17] These indices are weighted averages of survey questions measured at baseline, with the exception of patience questions, which were asked at the 2-year endline (these exhibit no treatment effects, however, and so we treat the endline patience measure as time-invariant). Within the control group, these indices of working capital, ability, patience and risk all have the expected relationship with endline economic success. See Web Appendix B for details.

Third, the human capital results are only somewhat consistent with the model's predictions. A 1 SD increase in human capital is associated with 0.33 logs greater earnings, as one would expect (Column 3). But higher ability individuals are less responsive to treatment—the opposite of what the model predicts. The effect gets smaller and less significant when all interactions are included (Column 6) but remains negative. One possibility is that our measures of educational attainment, digit recall, and physical health are poor proxies for true entrepreneurial ability in Uganda.

Fourth, patterns of patience are somewhat consistent with our predictions. A 1 SD increase in the patience index is associated with 0.507 logs higher earnings (suggesting the measure captures some meaningful variation). Whether the more patient also respond more to treatment as predicted depends on whether we look at the patience interaction alone or with all other interactions. Alone, the interaction coefficient is -0.135 logs, but controlling for other interactions it has the opposite and expected sign, at 0.324 logs. Neither is statistically significant, however. If it were merely the case that patience is poorly measured, we would not expect the non-interacted term to be so significant. It may be that patience plays less of a role given the design of the program (proposals and group decision-making).

Finally, patterns of risk aversion are also consistent with our predictions. A 1 SD increase in risk aversion is associated with a 0.259 lower response to treatment, indicating the risk averse are less likely to make investments. Interestingly, higher risk aversion levels are associated with higher earnings overall.

6.2 Male versus female returns: Are females more constrained?

One of the most striking results is the difference in performance between men and women in the control group. We can see this pattern most clearly in in Figure VI, but also Tables II and IV. Male controls see their investment and earnings grow over time, more or less keeping pace with the treatment group. Treated males have a persistent lead in absolute terms, but this lead shrinks between two and four years in relative terms, largely because male controls accumulate capital stock and increase earnings. Specifically, looking at capital stock, treated males have a capital stock 167% greater than control males after two years but just 41% greater after four (Table II). Treated males also see sustained earnings growth, with the biggest increase in the first two years (Figure VI). Male controls keeps pace with the treated and may even be slowly closing the gap.

For males, this pattern is consistent with the idea that credit constraints slow investment as the capital must first be saved.

In contrast, women in the control group report stagnant capital stocks and earnings over the full four years. Treated females have a stock 108% greater than control females after 2 years and again 108% greater after four (Table II). We see no evidence of catch-up among the female control group. The log estimates suggest mild increases in stocks among female controls, but the control group catch-up still seems to be driven primarily by males.[18] Meanwhile, women in the control group actually crease their hours of non-agricultural employment in favor of agricultural work (Table III) and show almost no change in real earnings levels (Figure VIa and Table IV). If we use the log earnings figures (Figure VIb and Table IV) female control earnings look less stagnant, and more of the earnings growth is found in the first two years. But the pattern still suggests more divergence between treatment and control females than their male counterparts.[19]

What accounts for the difference between male and female controls? The theory and heterogeneity analysis suggests investment and earnings are increasing in an individual's initial access to capital and credit, patience and, to a lesser extent, stock of human capital. Our evidence suggests that women have less of all three.

We compare male and female access to capital and patience in Table VII, among all at baseline and controls at the 2-year endline. We regress each variable on a female dummy, controlling for district. The male mean is listed at the base along with the relative difference with women.

First, women have 0.274 SD lower levels of human capital, especially formal education. Women are 14 to 24% percent less likely than males to have access to small and large loans. At baseline women are also more likely to have outstanding debt, and their debts are larger when they have them (one reason they may be constrained from obtaining further credit). Their savings and wealth are not significantly lower at baseline, but their earnings are about 20% lower than

[18] The level ATE also shrinks because the estimated value of the treated group's capital stock falls between two and four years, from roughly UGX 770,000 to 593,000 (the sums of the control means and the ATE). This fall mainly reflects changes in a few influential observations, since the log of the treated group's stock shows no decline (the sums of the control means and the ATE increase 0.12 log points). There may also be substantive reasons for a fall, however; it could represent initial overinvestment and a correction over time, or limits on the entrepreneurs' ability to replace lumpy assets as they depreciate. Or it could indicate respondent errors in estimating asset values.

[19] The difference-in-difference estimate for female log earnings is positive between 2 and 4 years (not shown). While not statistically significant, it is a log point greater than the male estimate, and this gender gap is significant.

males at baseline—an important source of start-up and ongoing capital in the absence of credit. Higher female credit constraints could be due to lower levels of human capital and employment, or different age profiles. When we add these as controls (not shown), the results are similar.

Second, while differences in credit and capital appear to be important, women may also be more present-biased than males, and hence be less likely to make investments. 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 patience index is about 0.11 SD lower for control women than men. It is hard to believe, however, that this is the cause of laggard performance among control women. After all, women invest just as aggressively as men and earn just as high returns when treated, as we saw above. Whatever role patience plays in overall performance, unless "the patience constraint" women face is relieved through the treatment, patience differences alone cannot explain the laggard performance of female controls. While the group design of NUSAF could act as a commitment device for the initial investment, after four years we would expect the impatient to have divested or depreciated their business assets. Yet after four years women's capital has grown and continues to grow. Thus we are skeptical that female impatience is a binding constraint.

6.3 The effect of groups

So far our assessment of the effect of the group organization of the intervention has been largely speculative. Unfortunately this feature could not be varied, randomly or otherwise. But analysis of the heterogeneity provides some insights.

First, the evidence runs against concerns that group leaders or elites could capture the grants. Among non-leaders in treated groups, 90% 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 that imbalances or capture was minor. We can check these responses by examining whether group leaders received more training, have higher capital stocks, or greater earnings (after accounting for differences in human and physical capital, patience, and risk aversion). Members of the group management committee report roughly 20% greater training hours than non-leaders, but endline capital stocks and earnings are roughly the same (regressions not shown).

Second, groups could have positive effects on performance, either because they act as a commitment device in initial spending of the grant, or because there is learning, shared capital, or

other production complementarities. It is difficult to test these propositions individually, but 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 Table VIII 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 the treated (odd-numbered columns), then the treatment heterogeneity ion the full sample (even-numbered columns).

The most significant finding: A standard deviation increase in the dynamic is associated with an increase in the ATE of 0.408 logs for capital stock and 0.297 logs for earnings after two years. Moreover, the capital and earnings ATEs are lower in more heterogeneous groups (though the results is not significant for earnings). Group size, prior existence, 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.

6.4 The effect of the proposal and potential monitoring

The fact that groups had to put together a proposal on how they would use the funds adds an additional complication to this study, one that may make the transfer seem somewhat conditional. There was no official monitoring in the program design. The NUSAF program had been operating for several years at the time of the study, and had a reputation for misused funds and poor compliance, and so the absence of official monitoring should have been widely understood. Nonetheless, the act of writing the proposal may have created a individual, group or community norm of compliance, and a reluctance to violate it. Can this account for the high rates of investment? We are unable to evaluate this possibility in the NUSAF YOP program. However, a sub-

sequent cash grants evaluation in northern Uganda, by one of the authors, randomly varied whether foreknowledge of official monitoring and follow-up by the granting organization changes investment levels or patterns. For the most part, it does not. Monitoring induces a small decrease in the purchase of durable assets and a small increase in spending on business assets., and no effect on consumption of the grant or unspent funds (Blattman et al., 2013).

7 Is there evidence of social externalities?

Idle hands do the devil's work, the saying goes. This folk wisdom is pervasive in policy, and so enhancing social cohesion and stability is a common rationale for poverty programs (Kristof, 2010; World Bank, 2012). Indeed, it was one of the three core aims of NUSAF.

A large social, political and economic literature supports this folk wisdom, suggesting that poverty alleviation has social externalities. First, empirical work associates employment with social cohesion. In the U.S., incomes and employment have long been associated with participation in civic life (e.g. Verba and Nie, 1972). Within and across developing countries, moreover, there is a also correlation between employment and levels of trust, civic participation, and support for democracy (Altindag and Mocan, 2010; Wietzke and McLeod, 2012; World Bank, 2012). This literature has two challenges, however. One, it is weakly theorized. Moreover, there is little evidence these correlations are causal. Nonetheless, the view is pervasive.

Second, a range of theories argues that poverty and unemployment raise the risk of social instability. In the canonical economic model, poverty lowers the opportunity cost of participation in riots, crime and conflict (e.g. Becker, 1968; Collier and Hoeffler, 1998; Ehrlich, 1973; Grossman, 1991). Another body of theory posits that poverty, especially relative poverty, provokes a sense of injustice and frustrated ambitions, and with it a desire to retaliate.[20] Finally, a psychology literature argues that under stress, including economic stress, people are more likely to respond to bad stimuli with aggression (Berkowitz, 1993). While these theories operate at dif-

[20] Much of this evidence comes from historical and ethnographic studies of conflict (e.g. Gurr, 1971; Scott, 1976; Wood, 2003). Related is the sociological concept of anomie, whereby poverty and underemployment increases alienation and a lack of purpose, and hence leads to deviance, delinquency and crime (Durkheim, 1893; Merton, 1938). This view is consistent with experimental economic evidence that people will pay to punish perceived inequity or slights (Fehr and Schmidt, 1999), as well as theories of 'expressive' collective action, which hypothesize that the collective action problem is solved when people place intrinsic value on action itself (e.g. Downs, 1957).

ferent levels and articulate different mechanisms, all predict an inverse causal relationship between poverty and social cohesion, aggression, and stability.

Unfortunately the existing literature offers no causal evidence on the social impacts of poverty relief or job creation, especially at the micro level. Experimental poverty programs seldom measure these behaviors, and the few that do have failed to find a causal link (Berman et al., 2011; Blair et al., 2013; Blattman and Annan, 2011). A number of regional and cross-country analyses link economic shocks to national-level violence, but the generalizability of these results is uncertain (Blattman and Miguel, 2010). Northern Uganda, which only recently emerged from two decades of political instability, is a useful setting to study such social returns. Measuring potential social externalities was one of the main and more novel objectives of this study.

7.1 Data and measurement

The survey collected data on more than 50 socio-political variables, largely drawn from the authors' prior studies of post-war social, political and community integration and mental health among northern Uganda youth (Annan et al., 2011, 2009; Blattman and Annan, 2010). 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.

To simplify analysis and avoid cherry-picking of results, we collect the variables into pre-specified conceptual "families" and create additive indices standardized as z-scores (Kling et al., 2007). Each family index has zero mean and unit standard deviation. Web Appendix K describes measurement and summary statistics for the individual components.

First, we consider an *Index of kin integration* that is an additive index of four survey measures of marriage, family support, household in-fighting, and relations with elders.[21] Second, we capture broader social integration by an *Index of community participation* based on 10 measures of associational life, namely participation in community groups, meetings, collective action, and leadership. At four years we also have an *Index of contributions to community public goods* based on seven different types of public goods.

[21] Low levels of integration at this kin level could reflect the sociological concept of anomie discussed above. It could have more direct economic origins as well. In Africa, young adults who cannot contribute to the household or kin may find themselves dislocated from these networks, which could reduce constraints on anti-social behavior.

Third, we create an *Index of aggression and disputes* based on eight forms of self-reported hostile or aggressive behavior and disputes with neighbors, community leaders, and police. At the 4-year survey, we expand data collection and collect 18 additional self-reported anti-social or aggressive behaviors. These measures were rooted in psychological survey instruments on U.S. populations (Buss and Perry, 1992) and were adapted to the Ugandan context by the authors.[22]

Finally, also at four years, we have measures of peaceful and non-peaceful political attitudes and participation. We measure an *Index of electoral participation* based on 6 forms of political action around the 2011 election (such as registering and voting) and an *Index of partisan political action* based on four forms of express party support (such as attending a rally). Last, we have an *Index of protest attitudes and participation* based on 7 measures of participation in and attitudes around the largely violent post-election protests in Uganda (discussed further below).

Several of these measures have small, positive, non-experimental correlations with earnings, wealth and employment in our panel, consistent with the correlations found in the broader literature (see Web Appendix K for details). The large economic impacts of this program, however, offer a more convincing causal test.

7.2 Results

Overall, we see limited and weak evidence of a positive social impact on males after two years, and none whatsoever after four years. Table IX presents ATEs for the main outcome families. The point estimates are typically close to zero, and standard errors on these z-scores are typically small (less than 0.10 or 0.05 SD) suggesting we can rule out medium to large changes.

First, treatment is associated with little change in kin integration: a 0.012 SD decline after two years and a 0.048 SD increase after four, with neither change statistically significant. Looking at individual components (Web Appendix K), there is no significant change in family support or marriage, but a slight (0.05 SD) yet significant reduction in family disputes and a slight (0.07 SD) but only weakly significant decline in relations with elders.

Turning to community participation, treatment is associated with a small and temporary increase: a 0.097 SD increase after two years (significant at the 10% level) and an insignificant

[22] 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.

0.01 SD increase after four. The effect at two years is mainly driven by a small and weakly significant increase in whether the youth is a community leader (a mobilizer) or speaks out at community meetings. None are apparent after four years. We also have a measure of contributions to community public goods (roads, water, schools, etc.) at the four-year mark. The treatment effect is positive (0.02 SD) and not statistically significant.

We next examine interpersonal aggression and disputes. As we would expect, aggression is not common in the sample: the incidence of any one type of dispute (e.g. with neighbors, community leaders, or police) or of specific self-reported hostile or anti-social behaviors (e.g. threatening others, use of abusive language) is low (Web Appendix K). Roughly 5% of the sample, however, report frequent and high levels of aggression across a number of measures. Looking at males and females together, aggression declines 0.073 SD after two years and rises 0.05 SD after four, but neither effect is significant. When we disaggregate by gender, however, we see an unusual pattern at two years: a 0.20 SD decline among males, and a 0.18 SD increase among females, both significant at least at the 5% level. If we look ATEs for the individual components, the ATE is driven mainly by fewer reported disputes with neighbors, community leaders, and police (Web Appendix K). This two-year finding motivated in-depth investigation of aggression and disputes after four-years. Using the same measures and index, however, the pattern disappears at four years—the point estimate for both males and females is small but positive.

We also expanded our measures of aggression to include a wider number of aggressive and anti-social behaviors. This expanded index shows close to a zero ATE for either gender. One interpretation is that the male decline and female rise in aggression was temporary. A second is that the two-year finding is anomalous, especially because it was only observed in subgroups, and because few of the other measures showed any change at two years of any magnitude.

Finally, we consider political participation and behavior, related to the 2011 election and protests. The four-year survey took place a year after a contentious national election, where incumbent President Museveni was reelected for a third term. Voting itself was relatively and peaceful but the campaign was marred by intimidation of the opposition (European Union, 2011). Electoral participation tends to be high, with 91% saying they voted in the Presidential election and 87% in the district election, and roughly half saying that they attended gatherings to discuss the issues. We see little impact of treatment: overall, the treated have an index 0.04 SD higher than controls. This impact increases slightly to 0.10 SD among males, significant at the 10% level

(Table IX). Looking at the individual components, this weak effect seems to be driven mainly by a slightly higher propensity to have voted in the district-level election.

We see little change in levels of political participation. In a separate paper, we explore the effect of the program on support for Museveni and his government, and levels of partisan political action (Blattman et al., 2012). In brief, the treated are no more likely to support the government.

Finally, we see little difference in protest attitudes and behavior. Following the 2011 elections, as well as the Arab Spring protests, the main opposition leader called for nationwide protests. Marches were held in the capital and many towns, including several in the north. Some turned into mild to major rioting and looting, most conspicuously in Gulu, the largest northern town. Police repressed the rioters and marchers, with some loss of life (Human Rights Watch, 2012). The protests themselves tended to be held in major towns, whereas NUSAF was more rural, so only 2-3% of our sample 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. We assemble these protest attitudes and participation into an index, and the treated have a mere 0.008 SD lower index than controls. Looking across seven component measures, all of the ATEs are close to zero in absolute and relative terms and none are statistically significant (including actual participation). Thus we see no evidence that these large changes in well-being—changes directly attributed to a government program—reduce the likelihood of participating in anti-government protests or holding anti-government attitudes.

8 Discussion and conclusions

This paper explores a longstanding question about the process of development: whether capital market imperfections hold back occupational choice, income and employment growth, and the shift from agriculture to non-agriculture. It does so by looking at a surprisingly controversial development intervention: the cash transfer. In *Making Aid Work*, Banerjee (2007) laments that, "it is an item of faith in the development community that no one should be giving away money".

This paper's results imply this bias against cash is unwarranted. In Uganda, grants are typically invested and yield high returns in new, skilled trades. High returns have been noted elsewhere, but mainly among existing entrepreneurs and mostly males. We show the same is possible

among the poor and unemployed. Skilled trades are still only a fraction of their overall activities after four years, but represent their major source of earnings and productivity growth.

Another notable finding is the high returns to cash even among poor, unemployed and relatively uneducated women. This contrasts with the emerging literature that finds returns among female entrepreneurs to be low. While the context and population in this study are different, we believe our longer horizon is important: the treatment effects on females are greatest after four years. Hence the importance of longer term data.

Our results also stress the importance of credit constraints in poverty and occupational choice, supporting a large body of theory (Banerjee and Newman, 1993; Levine, 1997). Note, however, even the best microfinance institutions are unlikely to replicate our impacts: even if entrepreneurs accept the risk, loan terms are seldom longer than a few months, and microfinance interest rates (unlike commercial rates) often exceed the rates of return we observe by an order of magnitude. The expansion of existing finance models will not spur informal sector growth unless transaction costs fall, lending periods lengthen, and repayment schedules allow some delay.

Finally, our results present an optimistic picture of vocational training in low-income countries. Looking across the dozens of evaluations, most conclude that job-training seldom passes a cost-benefit test (Card et al., 2009; Cho et al., 2013; Heckman et al., 1999). The training programs most studied in poor countries relate to business skills and financial literacy, but these non-technical skills appear to yield tepid results (McKenzie and Woodruff, 2012). The biggest difference between this intervention and most others is the availability of capital for start-up costs. Our theory and evidence suggest this capital was essential.

Finally, we see little evidence of social externalities from this intervention. Our assessment has limitations, however. 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 any change on the individual margin runs counter to many expectations, including opportunity cost theories of conflict. It suggests that the case for these public investments should be made on the economic returns alone.

We see several priorities for future research. First is long-term tracking of experimental samples. Second is the question whether high investment levels were a product of the program design of NUSAF: pre-commitment to invest the funds (however unenforced), and disbursement to

groups. A separate study in Uganda by one of the authors suggests, however, that there is little change in investment patterns due to supervision and accountability (Blattman et al., 2013).

A third important is whether the absence of social externalities generalizes to other contexts, especially to "high-risk" populations. This too is being explored with ongoing experiments, including studies with ex-combatants being remobilized for warfare (Blattman and Annan forthcoming), and street youth mobilized for crime and rioting (Blattman, Jamison and Sheridan forthcoming). Preliminary results, however, suggest little poverty-violence causal link.

In the meantime, cash grant programs are becoming more prevalent. Besides a growing number of government and World Bank programs, there are examples such as GiveDirectly, an international NGO founded by economists, which distributes unconditional cash via mobile phone to poor households. It has been rated as the #2 "top charity" in the world as one of the most effective ways to donate aid. "Strong evidence," they argue, "indicates that cash transfers lead recipients to spend more on their basic needs (such as food) and may allow recipients to make investments with very high returns, with no evidence of large increases in spending on items like alcohol or tobacco" (GiveWell, 2012). Our results suggest this enthusiasm is warranted, a change in perspective that, if true in general, could and should provoke one of the most dramatic transformations of foreign aid and state poverty alleviation in generations.

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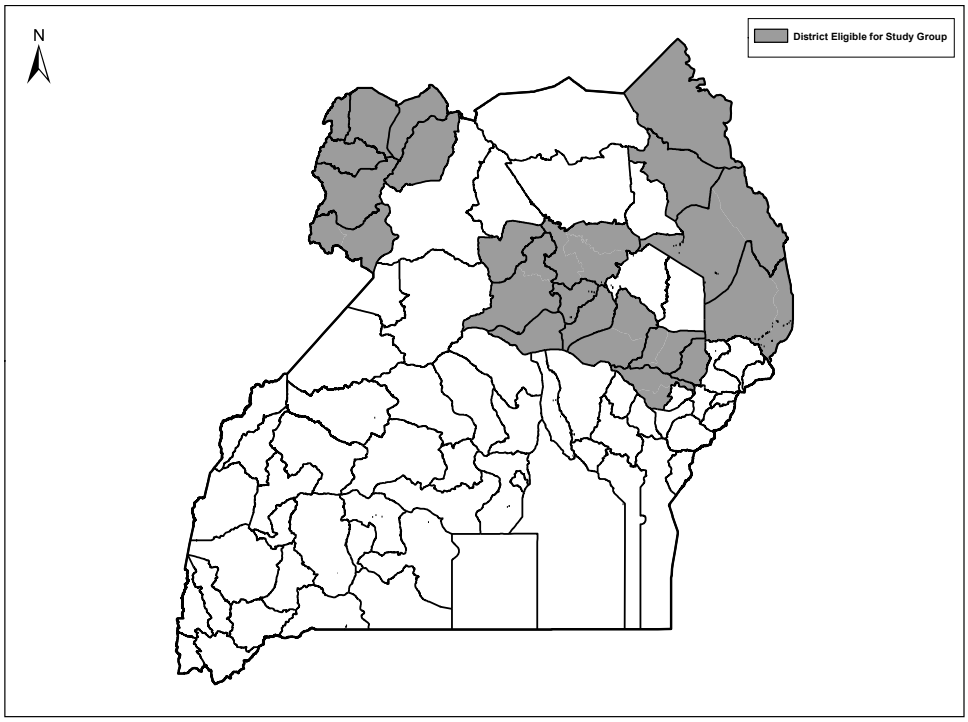
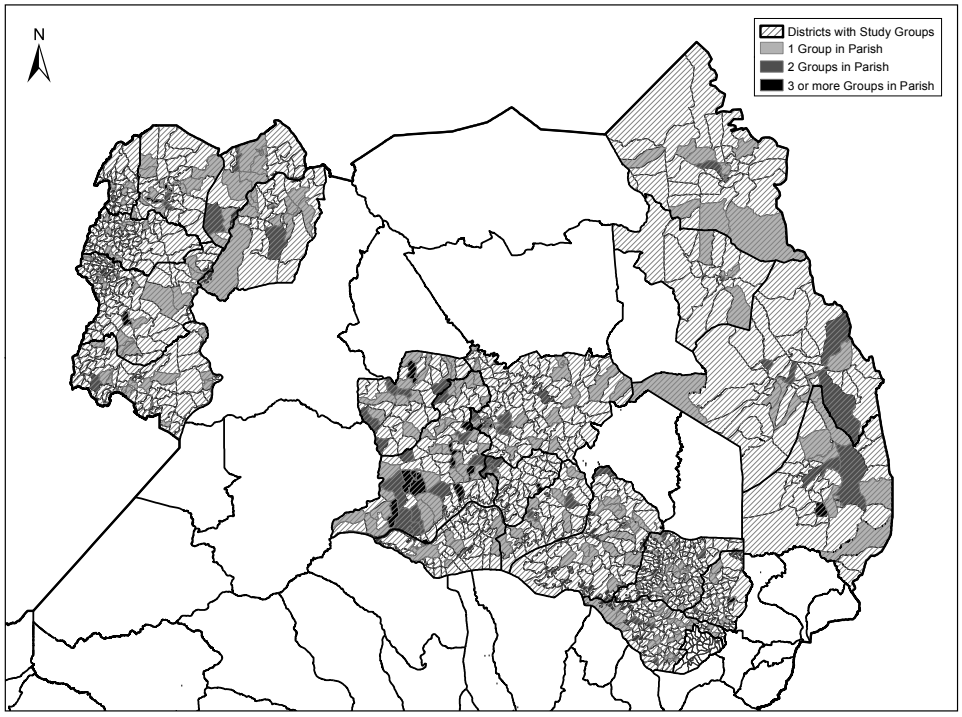
Figure I: Location of study communities

i. Districts participating in the study (2006 boundaries)

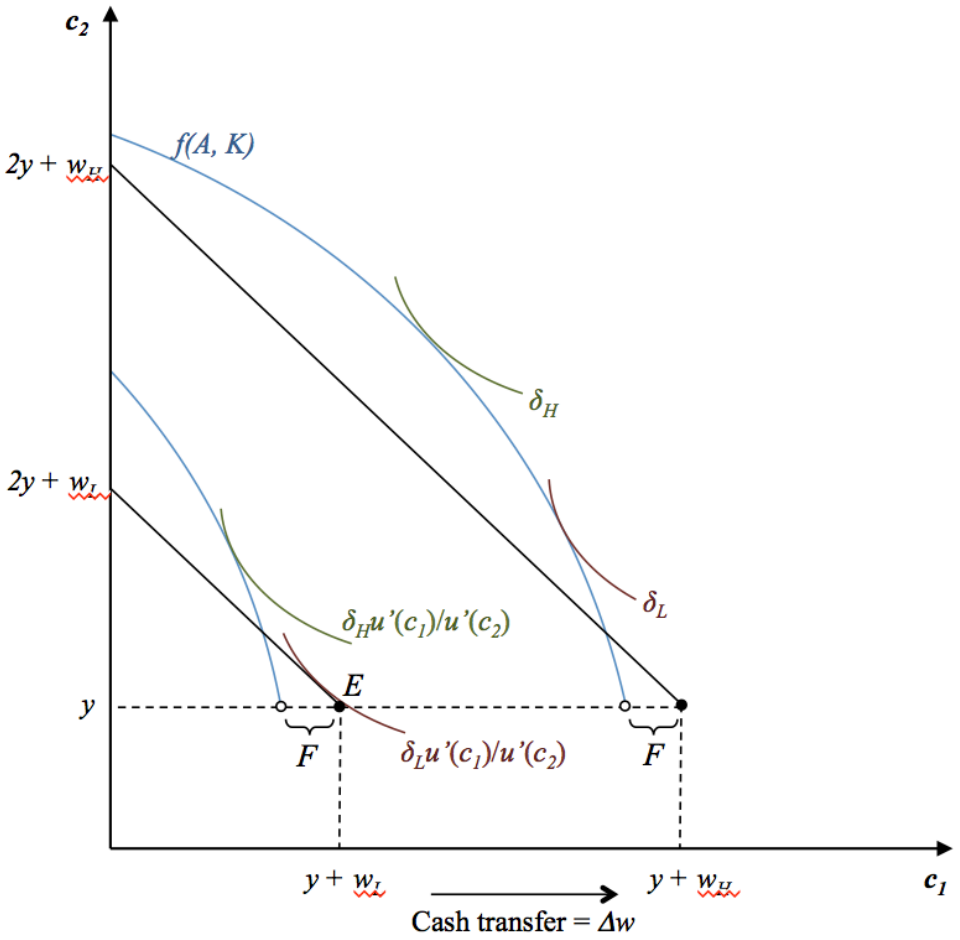


ii. Number of study communities (treatment and control) per parish



Notes: 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.

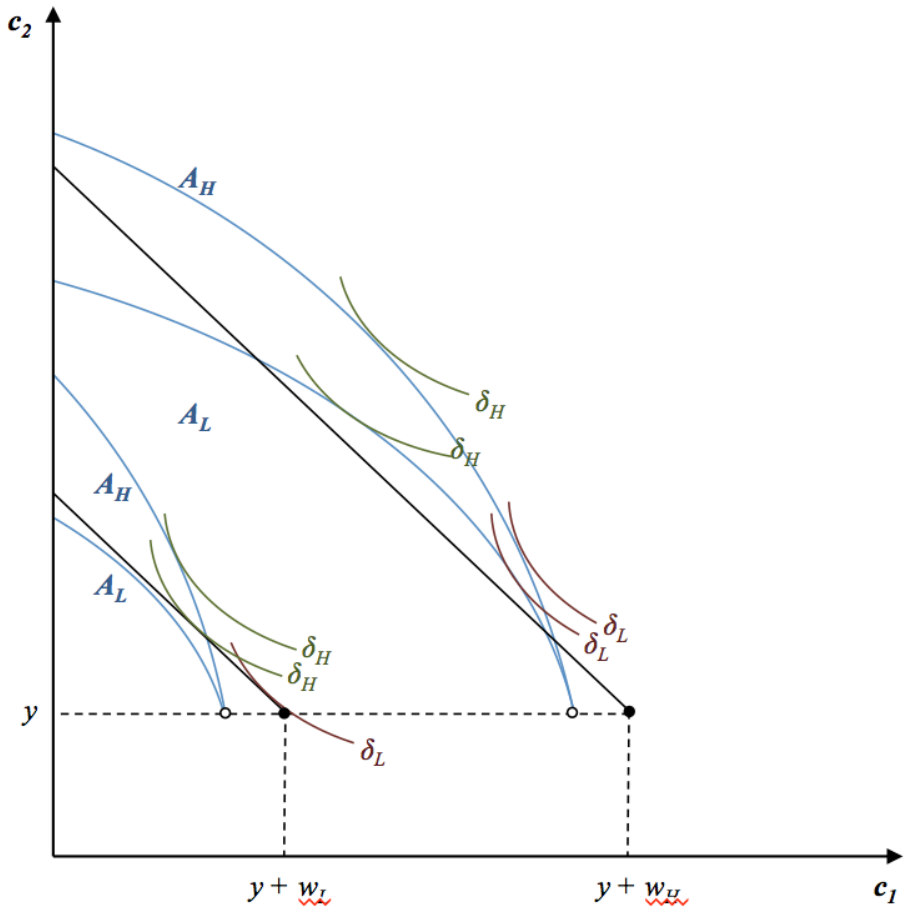
Figure II: Impact of cash transfers on occupational choice (No existing entrepreneurs)



At w_L, more patient and higher ability people become entrepreneurs while others remain laborers. Highly impatient laborers will have a corner solution at E.

For small F (relative to Δw) patient and impatient cash transfer recipients become entrepreneurs. But investment and period 2 income are generally increasing with patience.

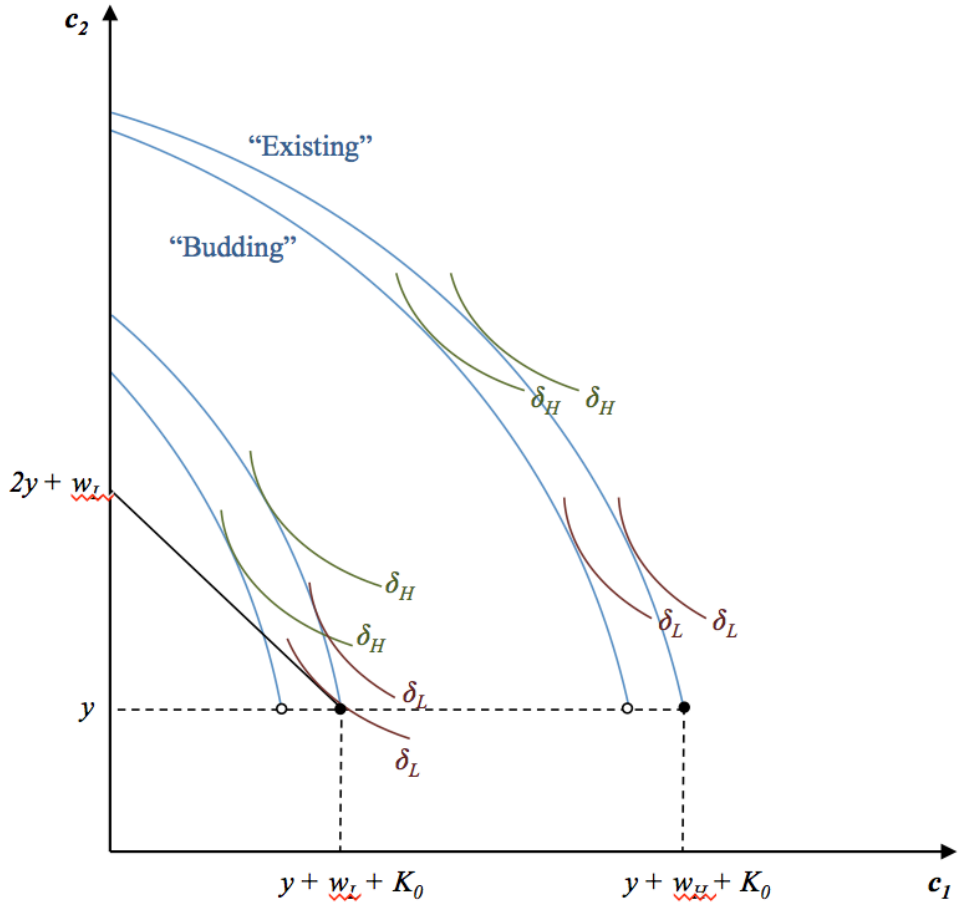
Figure III: High versus low ability individuals (No existing entrepreneurs)



The impact of a cash transfer is larger among higher ability and more patient individuals. Ability and patience positively interact.

Only highly impatient or very low ability individuals (those who do not have high return earning opportunities) would remain laborers after a cash transfer.

Figure IV: Existing versus budding entrepreneurs, with equal levels of starting capital



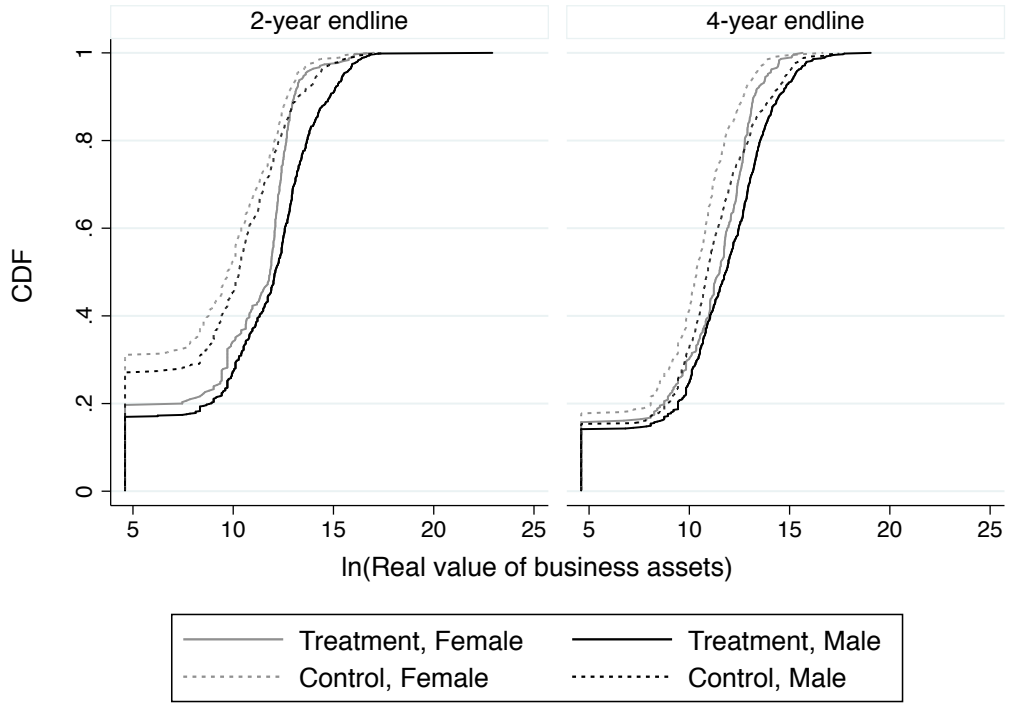
For illustrative simplicity we assume first period entrepreneur income is equal to labor income: $f(A,K_0) = y$.

The impact of cash transfers on investment and profits is larger among budding entrepreneurs than existing entrepreneurs.

The larger thee fixed cost of becoming an entrepreneur, the more impactful the transfer will be on profits (relative to existing entrepreneurs)

Figure V: Cumulative distributions of capital and earnings, by treatment status and gender

i. Real value of business assets



ii. Real net earnings

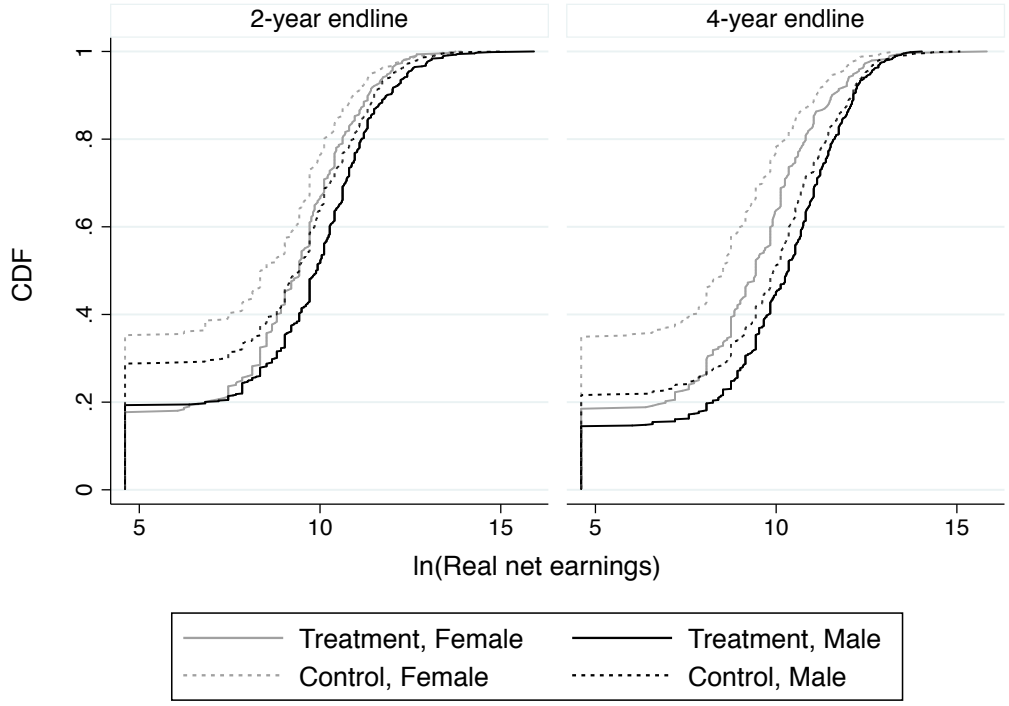
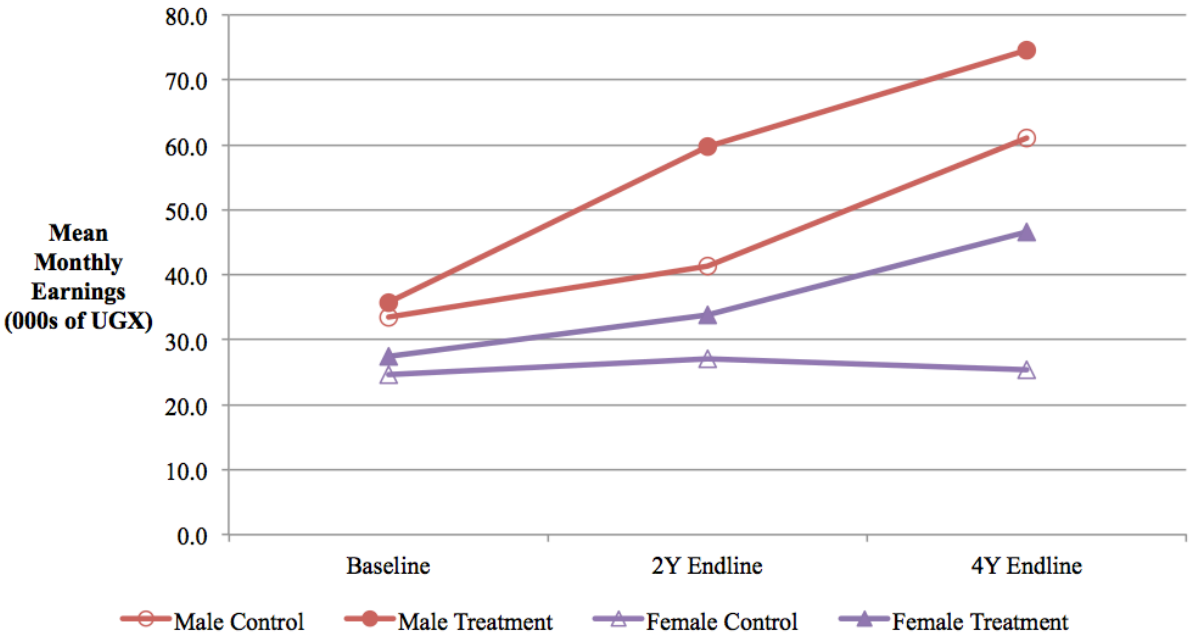


Figure VI: Earnings trends, by treatment status and gender

i. Real net earnings (top-coded at 99th percentile) over time



ii. ln(*Real net earnings*) *over time*

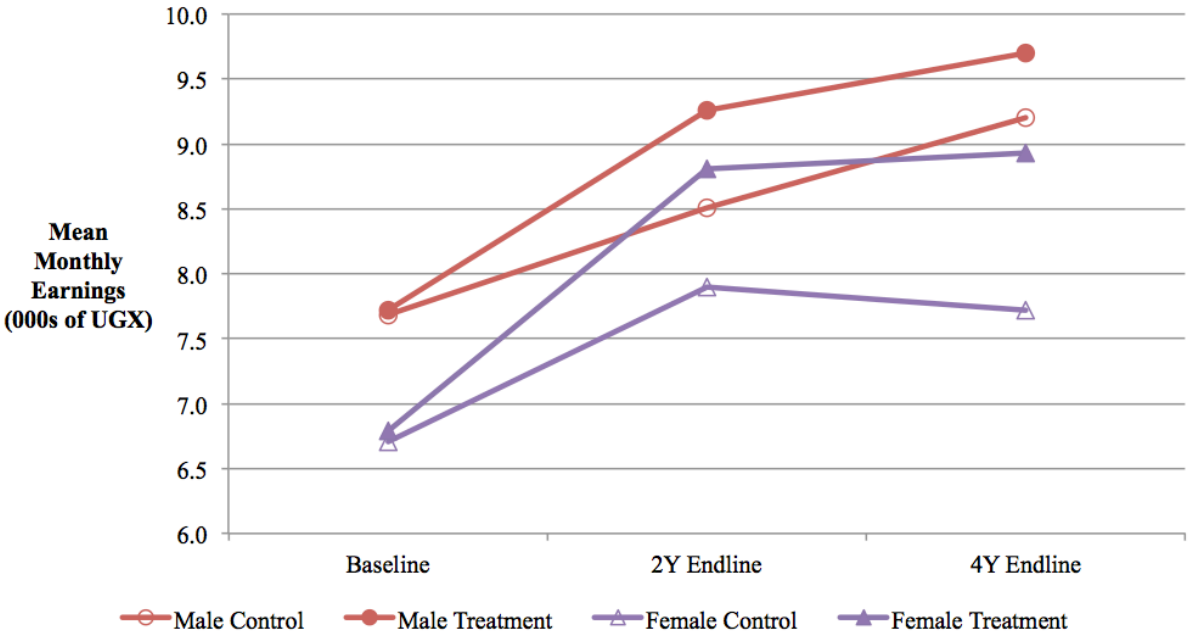


Table I: Key outcome summary statistics and treatment-control differences

	(1)	(2)	(3)	(4)	(5)	(6)
	2-year endline (N=2006)			4-year endline (N=1869)		
Main outcomes	**Control**	**Treatment**	**Difference†**	**Control**	**Treatment**	**Difference†**
Investments:						
Hours of training received	48.94	378.32	338.345	--	--	--
	214.47	*474.17*	*[22.680]****	--	--	--
Real value of business asset stock (000s of UGX)	299.39	737.14	402.508	392.45	607.82	245.228
	1,166.54	*1,898.32*	*[84.823]****	*1,175.50*	*1,438.57*	*[67.146]****
Occupation and employment:						
Main economic activity is non-agricultural	0.32	0.43	0.094	0.29	0.38	0.085
	0.47	*0.50*	*[0.026]****	*0.45*	*0.49*	*[0.025]****
Skilled employment indicator	0.34	0.63	0.301	0.42	0.67	0.240
	0.48	*0.48*	*[0.026]****	*0.49*	*0.47*	*[0.025]****
Avg of agricultural work per week	14.26	13.53	-0.880	19.21	18.98	0.222
	16.75	*19.09*	*[0.898]*	*20.00*	*20.51*	*[1.103]*
Avg hours of non-agricultural work per week	14.59	20.03	5.757	16.50	21.48	4.844
	24.39	*24.58*	*[1.311]****	*25.40*	*27.05*	*[1.426]****
Avg employment hours per week	39.92	42.28	2.405	46.47	50.59	4.942
	29.34	*30.18*	*[1.465]*	*31.27*	*32.86*	*[1.602]****
Earnings and poverty:						
Real net cash earnings (000s of UGX)	36.10	51.40	15.680	47.90	65.43	19.744
	80.30	*97.48*	*[4.376]****	*92.27*	*106.67*	*[5.414]****
Index of wealth (z-score)	-0.05	0.04	0.104	0.16	0.33	0.168
	0.95	*1.01*	*[0.054]**	*1.05*	*1.07*	*[0.064]****
Short term expenditures (000s of UGX)	--	--	--	51.67	55.72	6.358
	--	--	--	*34.19*	*38.11*	*[1.901]****
Subjective well being (1-9 scale)	2.73	3.05	0.376	3.29	3.73	0.415
	1.55	*1.65*	*[0.078]****	*1.68*	*1.81*	*[0.089]****
Select secondary outcomes						
Log of real value of other state/NGO transfers (000s of UGX)	5.56	5.81	0.124	--	--	--
	2.30	*2.73*	*[0.120]*	--	--	--
Average hours spent on chores per week	9.86	7.68	-2.379	10.08	8.75	-0.853
	17.02	*14.13*	*[0.846]****	*17.61*	*16.29*	*[0.993]*
Avg. hours spent on high-skill activities per week	6.78	11.83	5.404	7.23	11.99	4.714
	17.80	*19.03*	*[0.959]****	*17.14*	*19.68*	*[1.060]****
Log of value of all current savings	7.70	8.26	0.570	--	--	--
	3.45	*3.55*	*[0.172]****	--	--	--
Keeps records of expenses and revenues	0.31	0.41	0.120	0.26	0.39	0.119
	0.46	*0.49*	*[0.025]****	*0.44*	*0.49*	*[0.025]****
Formally registered a business	0.15	0.19	0.057	0.11	0.17	0.064
	0.36	*0.39*	*[0.020]****	*0.32*	*0.37*	*[0.020]****
Pays business taxes	0.21	0.28	0.081	0.22	0.31	0.087
	0.41	*0.45*	*[0.025]****	*0.42*	*0.46*	*[0.024]****
Has employees	0.49	0.46	-0.014	--	--	--
	0.50	*0.50*	*[0.027]*	--	--	--
Log of average earnings per hour ('000s of UGX)	-1.59	-1.50	0.095	-1.58	-1.46	0.173
	1.42	*1.35*	*[0.086]*	*1.33*	*1.39*	*[0.079]***

† Calculated by a regression of the outcome on assignment to treatment and randonimzation strata (district) fixed effects, with robust standard errors clustered at the group level

All UGX-denominated outcomes were censored at the 99th percentile to contain potential outliers

**** p<0.01, ** p<0.05, * p<0.1*

Table II: Average treatment effects on skill and capital investments, overall and by gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours of training received	**Real value of business asset stock (000s of UGX)**		**ln(Real value of business asset stock)**		**Log of real value of other state/NGO transfers**
	2Y	**2Y**	**4Y**	**2Y**	**4Y**	**2Y**
ATE (All)	389.2	471.0	200.6	1.842	1.033	0.149
Std. Err.	[23.798]***	[91.784]***	[74.421]***	[0.197]***	[.181]***	[0.135]
Control mean	48.9	299.4	392.4	9.236	10.17	5.564
ATE as % of mean	795%	157%	51%	519%	176%	15%
Male ATE	384.3	601.3	217.9	1.92	0.889	0.182
Std. Err.	[24.325]***	[125.665]***	[104.847]**	[0.241]***	[.223]***	[0.160]
Control mean	40.7	360.7	532.3	9.396	10.5	5.588
ATE as % of mean	944%	167%	41%	563%	137%	18%
Female ATE	399.0	207.8	165.8	1.685	1.31	0.083
Std. Err.	[44.226]***	[102.086]**	[65.275]**	[.342]***	[.291]***	[.231]
Control mean	62.4	192.6	153.2	8.937	9.61	5.523
ATE as % of mean	640%	108%	108%	409%	255%	6%
Female - Male ATE	14.7	-393.5	-52.1	-0.235	0.421	-0.099
Std. Err.	[46.473]	[160.109]**	[118.927]	[0.418]	[.358]	[0.273]
Observations	1997	1996	1865	1996	1865	2003

Robust standard errors in brackets, clustered by group and stratified by district.
All ATEs are averagre treatment effect on the treated (ATT)
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
**** p<0.01, ** p<0.05, * p<0.1*

Table III: Average treatment effects on employment and occupational change, overall and by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(9)	(10)
	Main occupation is non-agricultural		**Skilled employment indicator**		**Total hours spent on agricultural activities in past week**		**Total hours spent on non-agricultural activities in past week**		**Total number of hours worked in high-skill activities in the past week**		**Total number of hours worked in the past week**	
	2Y	**4Y**	**2Y**	**4Y**	**2Y**	**4Y**	**2Y**	**4Y**	**2Y**	**4Y**	**2Y**	**4Y**
ATE (All)	0.108	0.098	0.337	0.275	-1.080	-0.435	6.468	5.381	5.927	5.265	3.616	5.254
Std. Err.	[0.029]***	[.028]***	[0.029]***	[.029]***	[1.006]	[1.193]	[1.449]***	[1.54]***	[1.046]***	[1.102]***	[1.614]**	[1.819]***
Control mean	0.309	0.280	0.344	0.424	14.26	19.21	14.59	16.50	6.781	7.228	39.92	46.47
ATE as % of mean	35%	35%	98%	65%	-8%	-2%	44%	33%	87%	73%	9%	11%
Male ATE	0.103	0.082	0.314	0.234	-0.807	-0.657	6.516	3.841	5.698	5.452	4.627	4.433
Std. Err.	[0.034]***	[.033]**	[0.034]***	[.033]***	[1.204]	[1.504]	[1.805]***	[2.018]*	[1.358]***	[1.466]***	[1.891]**	[2.091]**
Control mean	0.327	0.299	0.403	0.505	15.30	21.42	16.19	20.21	8.578	9.148	35.93	44.98
ATE as % of mean	31%	27%	78%	46%	-5%	-3%	40%	19%	66%	60%	13%	10%
Female ATE	0.12	0.132	0.383	0.354	-1.631	-0.012	6.378	8.361	6.388	4.906	1.578	6.832
Std. Err.	[.049]**	[.047]***	[.049]***	[.049]***	[1.606]	[1.889]	[2.223]***	[2.237]***	[1.425]***	[1.359]***	[2.826]	[3.375]**
Control mean	0.322	0.246	0.242	0.291	12.55	15.54	11.78	10.43	3.620	4.069	46.79	49.01
ATE as % of mean	37%	54%	158%	122%	-13%	0%	54%	80%	176%	121%	3%	14%
Female - Male ATE	0.017	0.05	0.069	0.12	-0.824	0.645	-0.138	4.52	0.690	-0.546	-3.049	2.399
Std. Err.	[0.058]	[.055]	[0.057]	[.057]**	[1.915]	[2.392]	[2.782]	[2.99]	[1.916]	[1.925]	[3.311]	[3.917]
Observations	2598	2598	1996	1861	1996	1861	1996	1861	1996	1861	1996	1861

Robust standard errors in brackets, clustered by group and stratified by district.
All ATEs are averagre treatment effect on the treated (ATT)
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
All UGX denominated variables censored at the 99th percentile.
**** p<0.01, ** p<0.05, * p<0.1*

Table IV: Average treatment effects on earnings and poverty, overall and by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Real net cash earnings (000s of UGX)		**ln(Real net cash earnings)**		**Log of average earnings per hour ('000s of UGX)**		**Index of wealth (z-score)**		**Short term expenditures (000s of UGX)**	**Subjective well being (1-9 scale)**	
	2Y	**4Y**	**2Y**	**4Y**	**2Y**	**4Y**	**2Y**	**4Y**	**4Y**	**2Y**	**4Y**
ATE (All)	17.8	19.9	0.916	0.846	0.145	0.171	0.1	0.202	7.277	0.369	0.528
Std. Err.	[4.790]***	[5.551]***	[0.151]***	[.154]***	[0.090]	[.085]**	[0.055]*	[.064]***	[2.033]***	[0.087]***	[.1]***
Control mean	36.1	47.9	8.285	8.653	-1.593	-1.584	-0.0516	0.16	51.67	2.727	3.292
ATE as % of mean	49%	41%	147%	130%	-9%	-11%			14%	14%	16%
Male ATE	23.2	19.1	0.847	0.598	0.146	0.174	0.133	0.179	6.943	0.474	0.611
Std. Err.	[6.512]***	[7.318]***	[0.178]***	[.18]***	[0.106]	[.104]*	[0.070]*	[.071]**	[2.543]***	[0.106]***	[.116]***
Control mean	41.3	61.0	8.509	9.205	-1.536	-1.471	-0.0267	0.188	53.76	2.75	3.298
ATE as % of mean	56%	31%	130%	79%	-10%	-12%			13%	17%	19%
Female ATE	6.9	21.4	1.058	1.329	0.141	0.163	0.035	0.248	7.923	0.157	0.368
Std. Err.	[6.643]	[8.224]***	[.271]***	[.261]***	[.161]	[.146]	[.086]	[.119]**	[3.209]**	[.149]	[.187]**
Control mean	27.1	25.5	7.898	7.715	-1.707	-1.83	-0.0986	0.11	48.24	2.688	3.287
ATE as % of mean	25%	84%	178%	265%	-8%	-9%			16%	6%	11%
Female - Male ATE	-16.3	2.4	0.211	0.731	-0.005	-0.011	-0.098	0.069	0.98	-0.317	-0.243
Std. Err.	[9.531]*	[11.058]	[0.321]	[.308]**	[0.189]	[.179]	[0.109]	[.136]	[4.023]	[0.181]*	[.221]
Observations	1996	1861	1996	1861	1484	1466	1983	1848	1859	1994	1858

Robust standard errors in brackets, clustered by group and stratified by district.
All ATEs are averagre treatment effect on the treated (ATT)
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
All UGX denominated variables censored at the 99th percentile.
**** p<0.01, ** p<0.05, * p<0.1*

Table V: Grant size per person as treatment

	(1)	(2)	(3)	(4)	(5)
	Log of real value of business asset stock	**Log of real net cash earnings**	**Log of real value of short term expenditures**	**Log of real value of current savings**	**Index of wealth**
ln(Grant size per person)	0.202	0.137	0.412	0.412	-0.054
	[0.240]	[0.167]	[0.352]	[0.352]	[0.067]
Female	-0.522	-0.547	-0.441	-0.441	-0.003
	[0.198]***	[0.143]***	[0.269]	[0.269]	[0.063]
4Y endline dummy	0.137	0.380			0.314
	[0.170]	[0.123]***			[0.056]***
Human capital index	-0.025	0.165	1.007	1.007	0.337
	[0.153]	[0.108]	[0.186]***	[0.186]***	[0.046]***
Working capital index	0.142	0.315	0.470	0.470	0.357
	[0.145]	[0.116]***	[0.191]**	[0.191]**	[0.058]***
R-squared	0.141	0.080	0.197	0.197	0.217
Control Mean	9.698	8.466	7.611	7.611	0.0528
Treatment Effect Percentage	2%	2%	5%	5%	--
Surveys	2Y & 4Y	2Y & 4Y	4Y	4Y	2Y & 4Y
Obs	1684	1682	871	871	1674

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic and district indicators
All ATEs are averagre treatment effect on the treated (ATT)
*** p<0.01, ** p<0.05, * p<0.1

Table VI: Treatment heterogeneity in earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Log of real net earnings					
	Full sample	**Those without an existing skilled trade**				
Treated	0.940	1.006	1.035	1.037	1.030	1.028
	[0.154]***	[0.158]***	[0.158]***	[0.157]***	[0.158]***	[0.156]***
Female	-0.731	-0.694	-0.640	-0.660	-0.708	-0.630
	[0.102]***	[0.104]***	[0.108]***	[0.107]***	[0.106]***	[0.109]***
Treated X 4Y endline	-0.037	-0.161	-0.156	-0.178	-0.185	-0.225
	[0.211]	[0.222]	[0.223]	[0.222]	[0.223]	[0.221]
4Y endline	0.383	0.430	0.425	0.421	0.434	0.446
	[0.140]***	[0.147]***	[0.148]***	[0.148]***	[0.148]***	[0.147]***
Treated X Skilled trade	-0.858					
	[0.368]**					
Existing skilled trade	0.755					
	[0.225]***					
Treated X Working capital index	-0.195	-0.244				-0.230
	[0.101]*	[0.107]**				[0.183]
Working capital index	0.517	0.517				0.456
	[0.097]***	[0.102]***				[0.130]***
Treated X Human capital index			-0.197			-0.079
			[0.106]*			[0.181]
Human capital index			0.331			0.144
			[0.099]***			[0.128]
Treated X Patience index				-0.135		0.324
				[0.108]		[0.235]
Patience index				0.507		0.245
				[0.129]***		[0.164]
Treated X Risk Aversion Index					-0.259	-0.266
					[0.108]**	[0.245]
Risk Aversion Index					0.461	0.501
					[0.184]**	[0.219]**
R-squared	0.095	0.090	0.085	0.087	0.085	0.096
Obs	3846	3532	3532	3531	3502	3502
Control Mean	8.466	8.376	8.376	8.376	8.376	8.376

Robust standard errors in brackets, clustered by group and stratified by district Omitted regressors include an age quartic, district indicators, and baseline measures of employmnet and human and working capital
**** p<0.01, ** p<0.05, * p<0.1*

Table VII: Male-female differences in access to credit and other determinants of investment and returns

	Can get a loan of UGX 100,000 ($58)		Can get a loan of UGX 1 million ($580)		Total value of outstanding loans (000s of UGX)		Total value of outstanding loans, conditional on a loan	
	Baseline, full sample	2Y survey, controls	Baseline, full sample	2Y survey, controls	Baseline, full sample	2Y survey, controls	Baseline, full sample	2Y survey, controls
Female dummy	-0.076 [0.016]***	-0.082 [0.034]**	-0.029 [0.011]***	-0.066 [0.029]**	6.497 [3.790]*	-11.937 [9.672]	37.885 [12.339]***	-102.431 [64.387]
Male mean	0.391	0.583	0.121	0.309	16.65	49.13	91.76	244.5
Female dummy as % of male mean	-19%	-14%	-24%	-21%	39%	-24%	41%	-42%
R-squared	0.069	0.032	0.039	0.053	0.023	0.040	0.074	0.103
Obs	5196	1111	5196	1100	5130	1128	1674	295

	Savings (000s of UGX)		Index of wealth		Real net cash earnings (000s of UGX)		Human capital index	Patience index
	Baseline, full sample	2Y survey, controls	Baseline, full sample	2Y survey, controls	Baseline, full sample	2Y survey, controls	Baseline, full sample	2Y survey, controls
Female dummy	-2.879 [5.366]	-36.002 [12.091]***	0.049 [0.037]	-0.050 [0.068]	-14.134 [5.073]***	-11.842 [5.311]**	-0.274 [0.032]***	-0.112 [0.027]***
Male mean	26.61	83.91	-0.125	-0.0657	70.95	39.26	0.651	0.0679
Female dummy as % of male mean	-11%	-43%	--	--	-20%	-30%	--	--
R-squared	0.059	0.040	0.103	0.092	0.026	0.037	0.105	0.798
Obs	2574	1126	5196	1116	5196	1125	2598	1294

Robust standard errors in brackets, clustered by group and stratified by district. Omitted regressors includedistrict indicators.
**** p<0.01, ** p<0.05, * p<0.1*

Table VIII: Investments and performance by group characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	2 Year Endline		**Pooled Endlines**			
	Hours of training received since baseline		**Log of real value of business asset stock**		**Log of real net cash earnings**	
	Treated	**All**	**Treated**	**All**	**Treated**	**All**
Treated		217.230		2.556		0.803
		[91.128]**		[0.537]***		[0.448]*
Treated × Group existed prior to YOP		84.454		-0.309		0.156
		[47.908]*		[0.273]		[0.222]
Group existed prior to YOP (indicator)	77.426	1.613	0.031	0.284	0.206	-0.001
	[36.270]**	[22.926]	[0.189]	[0.166]*	[0.135]	[0.145]
Treated × Group dynamic index		23.484		0.408		0.297
		[17.883]		[0.123]***		[0.101]***
Group dynamic index	-9.013	-16.491	0.154	-0.145	-0.000	-0.236
	[16.257]	[8.954]*	[0.093]*	[0.093]	[0.070]	[0.080]***
Treated × Group size		2.340		-0.019		0.011
		[3.223]		[0.020]		[0.016]
Group size	-3.047	-1.949	-0.016	0.001	-0.002	-0.004
	[2.664]	[1.482]	[0.015]	[0.014]	[0.011]	[0.011]
Treated × % of group female		205.646		-0.474		-0.603
		[85.771]**		[0.519]		[0.421]
% of group female	92.267	-92.868	-0.543	-0.014	-0.457	0.270
	[87.134]	[44.611]**	[0.391]	[0.342]	[0.292]	[0.288]
Treated × Group heterogeneity index		-36.984		-0.253		-0.093
		[23.535]		[0.128]**		[0.103]
Group heterogeneity index	-28.215	-3.994	-0.127	0.013	-0.041	0.012
	[19.557]	[11.104]	[0.089]	[0.092]	[0.071]	[0.081]
4Y endline			0.212	0.896	0.359	0.384
			[0.159]	[0.165]***	[0.119]***	[0.139]***
Treated x 4Y endline				-0.772		-0.016
				[0.261]***		[0.210]
Inequality of grant distribution	-12.565		-0.062		-0.115	
	[16.958]		[0.093]		[0.062]*	
Level of leader capture	-0.947		0.119		-0.143	
	[49.886]		[0.310]		[0.215]	
R-squared	0.141	0.277	0.136	0.168	0.095	0.100
Obs	968	1979	1883	3825	1881	3821
Control Mean	48.94	48.94	9.698	9.698	8.466	8.466

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic, district indicators, and basel
*** p<0.01, ** p<0.05, * p<0.1

Table IX: Average treatment effects on social outcome families

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Kin integration outcomes (Z score)		**Community participation outcomes (Z score)**		**Community public good contributions (Z score)**	**Anti-social behavior and dispute outcomes (Z score)**		**Anti-social behavior and dispute outcomes extended (Z score)**	**Electoral participation outcomes (Z score)**	**Protest attitudes and participation (Z score)**
	2Y	**4Y**	**2Y**	**4Y**	**4Y**	**2Y**	**4Y**	**4Y**	**4Y**	**4Y**
ATE (All)	-0.012	0.048	0.097	0.010	0.021	-0.073	0.050	0.010	0.040	-0.008
Std. Err.	[0.059]	[0.053]	[0.053]*	[0.057]	[0.058]	[0.054]	[0.051]	[0.052]	[0.056]	[0.050]
Male ATE	0.042	0.008	0.085	0.084	0.059	-0.201	0.044	0.005	0.103	0.005
Std. Err.	[0.065]	[0.058]	[0.060]	[0.069]	[0.070]	[0.064]***	[0.061]	[0.063]	[0.061]*	[0.063]
Female ATE	-0.121	0.124	0.12	-0.133	-0.053	0.183	0.062	0.021	-0.085	-0.035
Std. Err.	[.108]	[.106]	[.1]	[.088]	[.093]	[.087]**	[.089]	[.094]	[.108]	[.098]
Female - Male ATE	-0.163	0.116	0.035	-0.217	-0.112	0.384	0.018	0.016	-0.188	-0.040
Std. Err.	[0.122]	[0.120]	[0.115]	[0.107]**	[0.113]	[0.103]***	[0.107]	[0.113]	[0.122]	[0.124]
Observations	2003	1865	2003	1865	1865	2003	1865	1865	1865	1865

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
All ATEs are averagre treatment effect on the treated (ATT)
**** p<0.01, ** p<0.05, * p<0.1*

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A Further intervention and experimental details

From 2003 to 2010, the centerpiece of Uganda's northern security and development plan was a decentralized development program, the Northern Uganda Social Action Fund. Starting in 2003, communities and groups could apply for cash transfers for (1) community infrastructure construction, or (2) income support and livestock for groups of ultra-poor men and women.

In 2006 the government announced a third NUSAF component: the Youth Opportunities Program (YOP). An approximate timeline of events is as follows:

2006	YOP program announced, applications received
2006-7	Hundreds of YOP applications funded
8-12/2007	YOP funds remain for 265 groups in 14 districts; District governments nominate 600+ groups from the 2006 application pool; Central government screens and approves 535 groups

2/2008	Baseline survey with 5 people per group
7-9/2008	Government transfers funds to treatment groups
10/2010	"2-year" endline survey runs through 2/2011
3/2012	"4-year" survey runs through 6/2012

17 districts were eligible for NUSAF support, including YOP. In 2008, 13 districts had YOP funds remaining and could participate in this study (see Figure 1 in main paper).

Village and subcounty officials passed applications up to the district, which screened and nominated applications for funding, by forwarding them to the central government. Specifically, local government officials, called NUSAF District Technical Officers (NDTOs), were instructed to verify applications for the minimum set of technical criteria required for eligibility, and conduct field appraisals on a multiple of the total number of projects that would be selected for funding. However it was created, this list of eligible and verified projects was sent to a central government project Management Unit (NUMU).

This government screening and selection took two main forms. First, the government asked that 22 groups of underserved populations (e.g. Muslims, orphans) be funded and excluded from the experiment, and so these are not in our sample. Second, since a town could seldom expect more than one proposal to be selected, local governments deliberated over proposals, and these decisions may have been informed by strategic calculus or by political and personal ties.

Finally, the central NUMU reviewed proposals again for completeness and validity. A NUMU audit team also attempted to visit and verify each group in the field, to minimize corruption. In January 2008 they provided the researchers with a final list of 535 groups that were eligible for the intervention and could be randomized. They also provided the researchers with a budget per district. Our baseline data reflect only those who apply, were nominated by local governments for the final round of YOP funding, and then are deemed eligible and real by central government review.

To randomize, the researchers took a list of eligible proposals in each district and ordered it with a uniform random variable in Microsoft Excel 2003. The projects were assigned to treatment is descending order, until the funds for that district were exhausted. Between 30% and 60% of the eligible groups in each district were selected for funding.

Group cash transfers averaged roughly UGX 12.9 million ($7,497) per group. Groups could request amounts up to roughly $10,000. The average transfer size was UGX 656,915 ($382) per member. Given the variation in group size and requests, however, transfer size per official group member varied from UGX 200,000 to more than 2 million across groups. Appendix Figure 1a displays the distribution of group sizes from the group roster at baseline, and Appendix Figure 1b displays the distribution of transfers in US dollar equivalents.[1]

[1] A note on calculation of the grant size and grant size per person. The original proposals contain (a) total grant size, and (b) number of group members. Both data have some challenges, however, and so we make two major changes. First, the grant listed in the proposal may not match the amount actually received by the group because (1) a small number of funds were not transferred because of accounting or administrative issues at the central government o the receiving bank and group, and (2) a small number of groups had some of their funds stolen or diverted. This is dis-

B Baseline summary statistics and balance

B.1 Descriptive statistics

Appendix Table 1 lists group and individual characteristics at baseline. Group data are based on a baseline group roster and survey, as well as administrative data. This includes the male-female breakdown of the group, as well as the group size, how long the group had existed before the intervention, and grant size information. Here and elsewhere, all estimates are within-sample estimates. That is, 5 people were selected from each group, and we do not reweight the estimates to make inferences about the full group (which can vary in size). Each five are weighted equally.

Appendix Table 3 displays occupational choice at baseline for the full sample and at the four-year survey for the treatment and control groups.

B.2 Baseline covariate balance

Appendix Table 1 presents control and treatment means for 6 group-level variables and 22 individual control variables. We focus on key variables specified by our theory and heterogeneity analysis (excluding the Patience index and components, which were collected at endline). The table also displays the regression estimate for the treatment-control difference, with p-values (controlling for district as there are different probabilities of selection within these strata). We cluster standard errors at the group level.

The majority of the variables have a p-value well above 0.10. Four related economic variables display some imbalance, however: prior vocational training, initial working capital, savings and credit access are all slightly higher among the treatment group at baseline with a p-value of 0.10 or smaller.

If we analyze the full range of 300 baseline variables we find a modest degree of imbalance: 8.7% of the mean differences have a p-value less than .05, 13.3% have $p<.10$, and 51.3% have $p<.50$. Figure A2 plots the cumulative distribution function (CDF) for the p-values for all 300 tests of balance. The CDF is only slightly above the 45-degree line.

Overall, the treatment group is slightly wealthier than the control group. This does not appear to be a flaw in the randomization but rather a statistical anomaly. It is one reason we control for these baseline outcomes in our main regressions.

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cussed in the paper under treatment compliance. We adjust the grant amount received downwards if any of these two things occur—a rare situation, in fewer than 10% of cases. The second major change is the number of group members. We found the administrative data from the proposals (often submitted in 2006) to be different from the actual composition of the groups in 2008, by the time funding was available. To calculate group size per person, we use information from the group roster in 2008 plus retrospective endline data on the "effective" size of the group (the number of people who meaningfully participated in the group) to estimate a new group size, for treatment group members only, on the number of group members who participated in the grant. Details available from authors.

B.3 Indices of baseline working capital, ability, time and risk preferences

Index construction

For the heterogeneity analysis (and also as baseline variables) we have multiple variables/proxies that measure some dimension of baseline working capital, ability, time preferences, or risk 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. All are displayed in Appendix Table 1.

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 *endline* 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. Appendix Table 2 lists summary statistics for each. Weights for the z-score index are obtained in the same manner as ability. Endline measures are used as no baseline data are available, on the assumption that preferences are time-invariant and are not affected by treatment. As seen in Appendix Table 2, there is no appreciable difference in patience levels between treatment and control groups (mean difference of 0.00, p-value of 0.95), suggesting it is a time-invariant variable unaffected by treatment.

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.

Correlation with endline economic performance

Our theoretical model and the main heterogeneity analysis hypothesize that human capital, working capital, time preferences, and existing vocations determine investment decisions and future income levels. Thus we may ask, independent of the treatment, do these characteristics actually correspond with economic success in the panel?

Appendix Table 4 looks at the impact of various baseline measures on a composite measure of success (comprised of income and the stock of durable assets). For each measure, we look at the relationship with key indicators of human and physical capital as well as our constructed indices. As predicted by the simple theory presented in the main paper, which says that greater ability and capital will lead to better economic outcomes, there is a large, positive and significant correlation between working capital and human capital and both success and wealth. Working capital is also strongly positively correlated with wages, though human capital seems to have a small but insignificant positive effect. We do not see any of the predicted correlation between the economic success measures and patience.

Initial assets and savings have a strong positive correlation with success, wealth, and, to a lesser extent, profits, supporting the "wealth begets wealth" conjecture. Higher levels of baseline education also correlate significantly with wealth and the success measure.

Females see less economic success and lower profits than males in both groups, though this is not reflected in the wealth index, probably because the wealth index is a household rather than an individual measure.

C Survey non-response and panel attrition

Tracking strategy

We follow a two-phase tracking design. All respondents are sought in Phase 1, where at least one attempt is made to find respondents at their last known location.

In Phase 2, we select a random sample of unfound respondents and intensively track these (in general, at least three further attempts). Those not found in Phase 1 who are randomly selected no to be tracked in Phase 2 receive zero weight (and so are effectively dropped from the analysis. Those selected to be tracked in Phase 2, whether they are found or unfound, receive greater weight in all analysis. This weight is equal to the inverse of their Phase 2 sampling probability. This sampling technique is designed to use scarce resources to minimize attrition bias.[2] From the perspective of assessing attrition bias, it means we focus not our attention not on overall attrition but the "effective" attrition from those who were (randomly) tracked.

Attrition rates – Overall and effective

Appendix Table 5 displays overall and "effective" attrition rates. The effective response rate (ERR) is a weighted average of the response rates in phases 1 and 2: ERR = RR1 + RR2 × (1-RR1).[3] In the 2-year endline, RR1 was 63%. We drew a 53% random sample of unfound people, and RR2 for the subsample was 75%, for an ERR of 91%. In the 4-year endline, RR1 was 61%.

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[2] Thomas, D., E. Frankenberg, et al. (2001). "Lost but Not Forgotten: Attrition and Follow-up in the Indonesia Family Life Survey." The Journal of Human Resources **36**(3): 556-592; Gerber, A., D. Green, et al. (2011). "Addressing missing outcome data in randomized experiments: A design-based approach." Unpublished working paper.

[3] Orr, L., J. D. Feins, et al. (2003). Moving to Opportunity Interim Impacts Evaluation. Washington, DC, U.S. Department of Housing and Urban Development.

We drew a 39% random sample of unfound people, and RR2 for the subsample was 59%, for an ERR of 84%.

Correlates of attrition and potential bias

Overall, attrition is relatively unsystematic. Appendix Table 6 displays a linear regression of an attrition indicator on treatment status, district fixed effects (omitted from the table) and eleven demographic characteristics, lagged dependent variables, or baseline of capital and ability used in the main analysis. The regression is clustered at the individual level.

Attrition appears to be slightly correlated with treatment in the first endline but not in the second. Groups assigned to treatment are 5.6% more likely to not be found at the first endline, compared to 3.4% less likely to be found in the second endline (the former is statistically significant, the latter is not).

Second, collectively the explanatory power of the baseline variables is low. The regression has an R-squared of 0.07 in the first endline and 0.17 in the second, even including district fixed effects. Most coefficients are small and not statistically significant. We only observe several substantive and statistically significant differences. Unsurprisingly, younger and male respondents are more likely to be unfound across both endlines. In the second endline, we observe some correlation between certain group characteristics and attrition: those in larger groups and groups with higher levels of group inequality were more likely to be unfound, as well as those in groups with higher levels of heterogeneity.

D Main economic outcomes: Additional information

The main paper describes the measurement of key investment and economic outcomes. This section describes additional measurement decisions and details.

For *Hours of training received*, we omit any training lasting less than 16 hours. This tends to exclude minor community-based trainings by extension officers or NGOs. Respondents could report multiple trainings, and we report the sum of all hours trained.

The *Stock of business capital* is self-reported. Respondents are asked to self-assess the value of their raw materials, 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.

Monthly *Net earnings* are also self-reported by activity. While subject to recall and other bias, evidence from experimental microenterprise profit measurement suggests self-reported profits may be the least biased measure of income, imperfect as it may be.[4]

The *Index of wealth* z-score is constructed using 7 measures of housing quality, 55 household and business assets, 5 types of landholdings, and 3 measures of personal appearance. The index is the standardized score from the first principal component of these assets—shown to be a rela-

[4] Suresh de Mel, David McKenzie, and Christopher Woodruff, "Measuring Microenterprise Profits: Don't Ask How the Sausage Is Made," *World Bank Policy Research Working Paper Series* 4229 (May 2007), http://ideas.repec.org/p/wbk/wbrwps/4229.html.

tively reliable proxy for full consumption aggregates.[5] We measure the same assets at baseline and both endlines, and use the same weights in every round. These weights are derived from the principal components analysis of all baseline and endline assets pooled.

Our *Short-term expenditures* measure comes from an abbreviated expenditures survey, focused mainly food and household and personal goods and services rather than durables, and based on self-reported spending per category. It is not a comprehensive measure of expenditure. Such abbreviated expenditure surveys are a reliable approximation to a full expenditure survey, which was too lengthy to perform in this instance.[6]

E Secondary economic outcomes: Summary statistics and ATEs

In order to avoid selection bias in presenting of results, we report all measured survey outcomes, including the pre-specified secondary outcomes. Appendix Table 7 lists control means and treatment effects for 5 secondary investment outcomes and Appendix Table 8 and Appendix Table 9 list 13 secondary economic outcomes. In general, these are outcomes about which we did not have strong theoretical priors (i.e. the main model in the paper does not make direct predictions about them), and we did not pre-specify as core outcomes of interest, but the direction and magnitude of the effects are of interest.

F Skills training choices with and without the intervention

There is little data on training programs and choices in Africa. In addition to describing training and investment opportunities and decisions by the treated, we contrast their choices to control group members. In the absence of any intervention, these control group decisions give insight into the counterfactual training choices and opportunity.

F.1 What skills training are undertaken by treated and control individuals?

Endline group survey responses indicate that roughly 35% of the grants were spent on training. 74% of the treated (i.e. 68% of the full treatment group) invest in vocational training, or an average of 413 training hours. 15% of the control group, however, still obtains training in the absence of government support.

The treatment group also trains more intensively, receiving an average of 239 more hours of training than the control group members who received any substantive training.

This section briefly examines the major types of training undertaken among those who report at least 16 hours of training, by treatment status. First, the training type seems to primarily have

[5] Deon Filmer and Kinnon Scott, "Assessing Asset Indices," *World Bank Policy Research Working Paper Series* 4605 (2008).

[6] Kathleen Beegle et al., "Methods of Household Consumption Measurement Through Surveys: Experimental Results from Tanzania," *Journal of Development Economics* 98 (2012): 3–18, doi:10.1016/j.jdeveco.2011.11.001.

been selected by the entire group. Approximately 85% of groups had only one type of training for the entire group. About 12% had two different types, and just 3% selected 3 to 5 different types.

Second, most treated groups train in the same three or four trades. Among the treated over a third of the training was in tailoring (38%), followed by carpentry (24%), metalwork (13%), salon/hairstyling (8%), and business or management (5%). The remainder consists of other small trades, with only 2% receiving trading in any kind of agribusiness (whether farming, beekeeping, or fish farming). Among treated women who get any training, 70% are trained in tailoring and 16% in salons. Just 14% report training in a carpentry, metalwork, repairs or mechanics. Among treated males who get any training, just 23% report training in tailoring and 5% in salons, while 59% report training in a carpentry, metalwork, repairs or mechanics.

Third, 15% of control group members obtain training, but the mix of training is different. Tailoring (31%) and trades like carpentry (12%) and metalworking (5%) remain important, but lower than among the treated. Various forms of business and management courses are relatively more common (27%) as is agribusiness or farm training (7%). Among males, we see a larger incidence of business training (31%) and less of an emphasis on carpentry and metal work. Control group females do not have a large difference in training incidence from the treated group, though they, like the control men, have a higher incidence of business training.

The above breakdown has one problem: a short training receives just as much weight as a long and intensive one. This brings us to a sixth finding: that if we account for the intensity of the training, all of the above patterns are exaggerated. Looking at training intensity (incidence times the median number of hours trained), tailoring, carpentry, metalwork, and salon training make up 77% of hours among the treated and 70% in the control group. The control distribution resembles slightly more the treated distribution, with shorter business and management trainings a much smaller share of the distribution, and tailoring an especially large proportion of the distribution (47%). Most striking, though, is the difference in intensity of training in tailoring between the women's treated and women's control group. Though the incidence of tailoring training between the two groups was similar, the control group saw 91% of training intensity spent in tailoring versus only 65% in the treatment group. This result is driven primarily by the fact that the women's control group spent very little time on any other type of training.

Overall, results indicate that the funding not only enabled more people to afford lengthier and potentially more thorough training, but allowed them to train in more technical subjects that require more intensive training (like tailoring and construction trades). It is unclear whether this technical training was accompanied by any entrepreneurship or "soft skills" training, or why tailoring and construction in particular seem to dominate the types of training chosen.

F.2 Cost and delivery of skills training

Of the 15% that undertake training in the control group, the vast majority reported that received some training through governmental or non-governmental support. Few are self-funding. Self-supported individuals make different investments, with a greater emphasis on trades like tailoring and carpentry while government-sponsored training (for instance) places more emphasis on business/finance.

We do not have detailed data on the institutes themselves, other than the self-reported data, but there are likely to be as many providers of training as there are groups. Across northern Uganda, there are innumerable small artisans and formal and informal TVET (technical and vocational education and training) institutes. Training may thus resemble an apprenticeship with an emphasis on practice rather than class/technical learning (though programs often have elements of both, especially those in TVET institutions). Outside these artisans and institutes, government extension officers also provide training to communities and groups. The capacity of these institutions varies dramatically, and we lack data on these.

Control group individuals were much more likely to report training by District extension officers and somewhat less likely to report TVET trainers and local artisans. The emphasis on district extension probably reflects the increased reliance on free or government-supported agricultural and business training in the control group, and hence the shorter nature of these programs.

We do not have detailed information on the institutes themselves or quality criteria and details. We only have subjective assessments of quality of the training and the trainer on a 1 to 10 scale by trainees themselves. Trainees in both the treated and control groups rate the training fairly highly, with a third overall giving the training a score of 7/10 or higher (the median is 8/10 for both treated and control). We might expect the training quality to be higher among more formal institutions with higher tuitions. But treated individuals do not seem to subjectively rate the training significantly higher than control individuals. This could reflect higher expectations of more formal and expensive programs. We unfortunately do not have the data to say.

F.3 Patterns of take-up of training and skilled occupations

Take-up of skills training

Appendix Table 10 looks at determinants of training among the treated and in the control group. It examines ordinary least squares (OLS) regressions of an indicator for engaging in training since baseline on various baseline measures of demographics, education and prior vocational training, wealth, and patience. District fixed effects are included but not displayed. 74% of the treated and 15% of the control group invested in training.

Given that so many of the controls who received training received short, free training, initiative, ability and wealth may not be expected to play a large role. We see that receiving any training is increasing in assets, though the coefficient is small and significant at the 10% level only. Meanwhile, the probability of training *decreases* somewhat significantly with credit access—those who say they could obtain a 1 million UGX loan are nearly 9 percentage points less likely to have received training.

The theory from the main paper would predict that those with more human capital, higher working capital and higher levels of patience would be more likely to make these forward-looking investments. That is, those people who are higher ability, less capital-constrained, and more patient and future-oriented would be better prepared to make investments with long-term payoffs. We do see positive correlations, but they are not that significant. It may be that the control group is too constrained to invest in training, or perhaps vocational training is not the optimal investment for most youth, so they do not undertake it in the absence of the subsidy.

We see a similar story if we look at the association between baseline characteristics on self-supported types of training, although we do not see the same positive correlation with the human capital index.

Investment in business assets

Since receiving funds, treated individuals group have also invested a significantly greater amount of money into their businesses in comparison to the control groups. Treated individuals report a value of 940,000 UGX worth of raw materials, tools and machines owned, which is roughly three times more than the control group mean of 350,000 UGX. The median difference is even greater: 220,000 UGX among the treated versus just 20,000 UGX in the controls, an eleven-fold increase. We see a similar patter with asset acquisitions.

G Returns to Different Vocations/Trades

Mean and median returns by trade (using earnings from that trade only)

Women tend to choose strikingly "gender stereotypical" trades—mainly hair salons and tailoring. Could lower female returns overall be due to low returns to these specific trades? Overall, we see that returns to male-dominated trades like carpentry and metalworking seem to be greatest, but that tailoring and salon returns are in the middle of the pack in terms of earnings, with women tailors and salon workers earning similar returns to males in the same professions.

The businesses started by the YOP participants compromise a number of different skills. This section describes the differential returns across these business types. The data was collected in a detailed economic activity module in the first and second endlines. Business owners were asked about the different business activities they were engaged in and the income they had received from each activity in the last 4 weeks. The data below thus describes the businesses people are running, not those they were trained for or had originally selected to perform. Individuals may perform more than one activity.

Appendix Table 11 describes the average and median profits from each trade, as well as the standard deviation and number of individuals from the data set that are engaging in each activity, looking only at treated individuals. The table also examines how these profits break down by gender across activities. Women consistently have lower returns than men across activities, and the few women that engage in male-dominated activities, such as brickmaking and carpentry, have significantly less returns than men. For more traditionally female friendly activities such as tailoring and hair salons, the difference is not very significant.

Panel (a) presents results for the full sample. There are a number of activities, however, with a median income of zero. This is due to the fact that a number of individuals report an activity, but no income from it in the period. This is normal in this region as most activities are highly variable, with zero incomes in the last month a common outcome (because the cash flow was delayed, or because profits were zero). Brickmaking, carpentry and metal fabrication have the highest reported returns. Tailoring, a traditionally female activity and the most popular skill for women, has relatively much lower returns. Tailoring also compromised the highest number of people engaged in each activity.

Panel (b) describes the same group of individuals, but with those reporting zero income excluded. The results are very similar to those in Panel (a), suggesting that zero income earners do not dramatically alter the differences across businesses.

To explore the role that time spent in activities plays in the differential returns, Panels (c0 and (d) look at wages (or, more accurately, average hourly return) with and without 0s. This is total reported income divided by total reported time spent in the activity. Wages are universally low, with average wages between 500 to 3000 USH per hour ($0.20 to $1.20). The exception is the few women engaged in brick making, who report hourly wages of 24,000. This though appears to be an anomaly due to the low sample size.

Brick making for men also has the highest hourly return, with carpentry and metal fabrication close behind. Women in general earn just slightly less than men in more male dominated activities, except with hair salons, where women perform significantly better. Looking at wages thus suggests that women are not in fact doing much worse than men, only that they are not able to put as much time into their businesses. This is potentially due to pressure put on women to perform a number of household tasks in addition to their business activities.

Aggregate earnings by main occupation

Appendix Table 12 reports a regression of the natural log of aggregate net earnings (from all activities) for different businesses. The regression allows for a more accurate comparison of the returns across different business types, after accounting for age, gender and different starting human and physical capital. The relevant coefficient is the one on the indicator for working in a particular trade. All of these coefficients are positive, as the compare practitioners of that trade to all other people in the sample, including those with no trade and those with no employment whatsoever. To judge (roughly) the relative returns of each trade we can compare the size of these coefficients across different trades. The coefficients on construction trades appear to be among the highest in terms of total net earnings. As can be seen, tailoring is no longer the worst performing activity in general. Instead, it performs in approximately the middle of all activities, with metal fabrication, hair salon and carpentry performing the best.

We also look at differential returns for women for the three most common female activities, weaving, tailoring and hair salon. None of the coefficients on the trade/female interaction are statistically significant, though the size of the difference between men and women for weaving is very large at about half. The difference in male/female tailoring earnings, however, seems to be fairly negligible.

H Calibration: The relative contribution of physical and human capital

We test for the relative contribution of human and physical capital in our earnings average treatment effects. We do so in two ways. First, we estimate a simple production function that includes training and asset accumulation, along with a number of controls. Second, we also test a calibrated model by employing the same estimation strategy on the control group only and use the estimated coefficients to predict treatment outcomes. The calibrated treatment method suggests that the production function predicts two-year outcomes with relative precision, though the results for four-year outcomes are not well estimated. Both the direct estimation and calibration methods produce similar-sized coefficients. We find that physical capital was a major component

of the results obtained, with a coefficient three times the size of human capital. The methods employed in this note follow the literature on business model returns estimation.[7]

We first fit a standard Cobb-Douglas production function, $Y = AK^{\alpha}H^{\beta}$, where each firm produces net earnings, Y, with inputs of productivity, A, physical capital, K, and human capital, H. The return to physical capital is denoted as α, and the return to human capital as β. In order to estimate the model, it can be modified using the natural log function, becoming, $y = a + \alpha k + \beta h + \gamma X$. Lower case letters denote the natural log of each input, and X indicates a vector of demographic variables. We can estimate this equation and, by comparing the results obtained for α and β, we have an approximate (albeit unidentified) estimate of the relative importance of human and physical capital investment in producing earnings.

In addition to this direct test, we also estimate the model using the control sample only. The results obtained for α, β and γ are then used to estimate the outcomes for the treated group by interacting them with treatment group variables. This estimate is then compared to the actual outcome. This allows for a potentially less biased test of the model, as the treatment and control groups were selected at random. Since few of the individuals in the control sample are engaged in the same businesses (vocations), however, this method could also introduce an additional bias if the control group activities are not immediately comparable to the treatment group.

To estimate this model, we use our main net earnings measure. We measure physical capital using the real value of business assets acquired since baseline. The value of human capital is derived from the hours of training received times the average estimated value of an hour of training. The value of physical capital accumulation is available for individuals as it was asked in both of the endline surveys. The value of human capital accumulation is not observed as respondents were seldom aware of the cost of the trainings they received. We do have some information to help back out this value though. Some individuals were able to report the cost of the training they received. We use this value to estimate the minimum, maximum and average cost of training by hour. We also know the amount of hours of training everyone received. We multiply these two values to come up with an aggregate estimate of investment. We also consider a bounding exercise where minimum and maximum values are employed, and the results do not vary much. Finally, an additional constraint in the estimation of human capital is that we collected this information only in the two-year endline, but did not ask for investment between years two and four. This could explain the difficulty of calibrating the model in the four-year endline if individuals invested in human capital between these years.

We estimate the models in Appendix Table 13. Demographic controls (omitted from the table) include: age, age squared and age cubed, hours worked in the last 4 weeks capped at 99%, education, literacy, vocational training, numeracy measures, wealth index at baseline, savings in the last 6 months, and ability to access loans.

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[7] Jeremy Lise, Shannon Seitz, and Jeffrey Smith, *Equilibrium Policy Experiments and the Evaluation of Social Programs* (National Bureau of Economic Research, 2004); Petra Todd and Kenneth Wolpin, "Using a Social Experiment to Validate a Dynamic Behavioral Model of Child Schooling and Fertility: Assessing the Impact of a School Subsidy Program in Mexico" (2003).

Panel A estimates the simple production function on the full sample. The first column displays the results for the full, pooled sample, while columns 2 and 3 are for the 2- and 4-year follow-ups only. Column 4 is the pooled results with a control for the 4-year data. The results are fairly consistent across all of the specifications: the contribution of physical capital to business outcomes is between 0.11 and 0.14, while the contribution of human capital is between 0.30 and 0.35, approximately one-fourth the contribution of physical capital.

We also test different specifications for the value of human capital accumulation. As discussed above, estimates of per hour cost for training vary between 2,000 USH and 6,000 USH. Columns 1-4 present the results of the average specification, while columns 5-8 assume 2,000 USH and 9-12 assume 6,000 USH per hour. As this is a linear change, the results for physical capital do not change, while the coefficients for human capital do change, but by very little. The assumption for cost of training is robust, and the contribution of human capital stays consistent at about one-fourth of physical capital.

In addition to running the model on the full sample, we also run it on the control group only and use these results to predict outcomes for the treatment group. Panel B presents these results. The estimated coefficients are not substantially different for the contribution of physical capital in either of the follow-ups. There is also no difference in human capital for the 2-year follow-up. For the 4-year follow-up there is though a large decrease in the contribution of human capital. Human capital accumulation is no longer a significant indicator of success.

This lack of importance of human capital in the long-run may be due to a number of factors. It may be that human capital has decreasing returns. It may also be due to measurement error during the 4-year follow-up data collection. The results for the long-run analysis should thus be interpreted with caution.

Finally, the calibration results are presented in Panel C. These results are obtained by taking the point estimates obtained in Table X for h, k, and x and interacting these with the average values for the treatment group in the respective categories. A calibrated y is then obtained, representing an estimated average output for the treatment sample. Panel C first presents the difference between the observed and calibrated y for the 2- and 4-year results. The calibrated result at 2-years is well estimated, as it is only 11% lower than the observed outcome. The 4-year result is relatively poor, with calibrated outcomes 72% lower than the observed ones. The pooled results for the first and second endline are off by 41%, which lies exactly half-way between the two survey results, as we would expect.

I Robustness checks and alternate estimators

I.1 Specification and model changes

The main treatment effects specification in the paper is an instrumental variable estimate of the average treatment effect on the treated (ATT), one that controls for various baseline characteristics and weights each of the five group members equally, regardless of the size of the group. Appendix Table 14 examines the sensitivity of our ATEs to this approach. In general, the results are highly robust to a variety of alternative ATE calculations.

We consider the sensitivity of the ATE for five of our key outcomes: The real value of business assets in linear and log form, real net earnings in linear and log form, and the anti-social behavior index (a z-score). Column 1 displays the ATT from the main paper. Column 2 estimates the simple intent-to-treat effect (ITT) using controls and standard weights. Columns 3 and 4 estimate the ATT and ITT without baseline controls (including only district dummies, which are necessary for identification because the probability of selection varied by these strata). Column 5 estimates the ATT where individuals are weighted by the probability of selection from their group, so that people from larger groups get more weight.

In general, the point estimates change very little and statistical significance is not affected. The method of calculating ATEs is nearly immaterial.

Finally, recall that our linear real value of business assets and real net earnings measures were censored at the 99th percentile, to eliminate extreme values. The log versions of these variables, moreover, are actually $\ln(1+x)$ to (erroneously) account for zero asset and income values.

I.2 Quantile treatment effects

Quantile analysis will mitigate bias from outliers. Appendix Figure 3 maps the quantile treatment effects (QTEs) for real net earnings at 2- and 4-years. The median control group member had just 8,300 UGX ($3.50) of income in the last month after 2 years and 12,400 UGX ($5.16) after four. The top quantile control income earners are doing much better, with 86,800 UGX and 149,000 UGX at the 2- and 4-year follow-ups respectively.

In both follow-ups, below the 20th percentile, treated group members have nearly identical incomes, but the two groups diverge sharply from that point onwards. AT the 2-year follow-up, the median QTE for earnings is UGX 8,200, and at the 70th and 90th percentiles the QTE rises to more than UGX 15,800 and 40,000 respectively. These levels are very similar at 4-years. The results suggests substantial gain from the program at the median, and some relative reduction at the upper tail. Also, it is worth noting that the income of control individuals is rising quickly in this time, which may account for the lower longer term treatment effects: in the long run, those with high investment opportunities may begin to reach, however slowly.

I.3 Bounding main treatment effects for attrition bias

While effective attrition was low for both our 2- and 4-year samples, it is possible that the attrited may have experienced significantly different outcomes that those that we were able to find. We explore this possibility through a bounding exercise as conducted by Karlan and Valdivia (2011)[8], who use a range of assumptions for bounding originally from Horowitz and Manski (2000), Lee (2002), Kling and Liebman (2004).[9]

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[8] Dean Karlan and Martin Valdivia, "Teaching Entrepreneurship: Impact of Business Training on Microfinance Clients and Institutions," *Review of Economics and Statistics* 93 (2011): 510–552.

[9] Jeffrey R. Kling, Jeffrey B. Liebman, and Lawrence F. Katz, "Experimental Analysis of Neighborhood Effects," *Econometrica* 75, no. 1 (2007): 83–119; David Lee, *Trimming for Bounds on Treatment Effects with Missing Out-*

In Appendix Table 15 we construct new upper and lower effect bounds by imputing the outcomes for the missing individuals based on increasing (decreasing) the assumptions of outcomes on the missing individuals in treatment and control groups. This is done by imputing outcome means minus a predetermined standard deviation of the non-attrited treatment distribution, to the attrited in the treatment group. We then repeat the process for the attrited control group, but this time adding a predefined standard deviation from the found control distribution. The table thus contains the following constructed columns:

1. Imputed minimum value of each variable in the non-attrited treatment distribution to attrited in treatment group and maximum value of non-attrited control distribution to attrited in control group. We consider this an extreme bounding exercise.
2. Imputed mean minus 0.25 s.d. of the non-attrited treatment distribution to attrited in treatment group and mean plus 0.25 s.d. of the non-attrited control distribution to attrited in control group.
3. Imputed mean minus 0.10 s.d. of the non-attrited treatment distribution to attrited in treatment group and mean plus 0.10 s.d. of the non-attrited control distribution to attrited in control group.
4. Our non-bounded results.
5. Imputed mean plus 0.10 s.d. of the non-attrited treatment distribution to attrited in treatment group and mean minus 0.10 s.d. of the non-attrited control distribution to attrited in control group.
6. Imputed mean plus 0.25 s.d. of the non-attrited treatment distribution to attrited in treatment group and mean minus 0.25 s.d. of the non-attrited control distribution to attrited in control group.
7. Imputed maximum value of each variable in the non-attrited treatment distribution to attrited in treatment group and minimum value of non-attrited control distribution to attrited in control group. We consider this an extreme bounding exercise.

These columns are estimated for our skilled employment indicator, real net cash earnings and log of real net cash earnings for both 2- and 4-year follow-ups. The results for all variables across the two follow-ups are consistent with our (4) standard non-bounded results for (2) Lower 25, (3) Lower 10, (5) Upper 10 and (6) Upper 25. In fact, the only outcome that varies substantially is real net cash earnings for the 4-year endline, which now ranges between 24% and 67% effect.

The story is not the same for the extreme bounding of lower and upper minmax. The skilled employment indicator for the 2-year follow-up is the only dependent variable that does not vary dramatically. For all remaining outcomes, the ranges include negative outcomes for the lower minmax and extremely high positives for the upper minmax. This result is not particularly surprising as there is a wide variation in economic status among the treatment and control individu-

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comes (National Bureau of Economic Research Cambridge, Mass., USA, 2002); Joel L. Horowitz and Charles F. Manski, "Nonparametric Analysis of Randomized Experiments with Missing Covariate and Outcome Data," *Journal of the American Statistical Association* 95, no. 449 (2000): 77–84.

als. Imputing minimum (maximum) values for attriters imposes a very high assumption about their income.

These results suggest that, as regards to attrition, we would have to employ an extreme assumption in order to reject positive economic effects of the program; even normal pessimistic assumptions on attrition do not substantially change our results.

J Additional impact heterogeneity analysis

Appendix Table 16 examines impact heterogeneity on logged real vale of the business asset stock. We look at heterogeneity along the five main dimensions as in Table 6 of the main paper. We examine each form of heterogeneity individually and altogether. (Individually the test may be higher powered, while altogether is lower powered but less biased.) Column 1 considers the full sample, while the remaining columns look only at those without a skilled trade. In general the patterns are the same as described for earnings in the main paper.

K Social externalities: Additional analysis

K.1 Comparison to non-experimental income changes

Won-experimental changes in wealth and earnings are weakly but positively associated with increased social integration and community and electoral participation. Appendix Table 17 presents regression of the family indices on treatment (by gender); changes in the wealth index, log earnings, and log employment hours; and levels of wealth, earnings, and employment at baseline. We pool 2- and 4-year survey data where possible.

For the kin integration and community and electoral participation outcomes, the coefficients on wealth and income changes are typically positive and significant. Political scientists frequently observe a relationship between income, wealth and political participation in developed nations. Such correlations may be one reason for the belief that poverty alleviation will have social externalities. The absence of evidence for experimental treatment effects, however, suggests that these correlations may be endogenous, biasing us towards the belief that income shocks promote integration.

Unusually, however, we see the opposite correlation between aggression and protest behaviors and these earnings and wealth changes. The coefficients are still positive and significant, indicating that an increase in earnings and wealth is correlated with an increase in aggressive behaviors. Again, the absence of treatment effects on these outcomes suggest that these correlations are also endogenous and the coefficients are biased indications of any income-violence link.

K.2 Disaggregated components and treatment effects

Table 9 in the main paper displays ATEs for the family indices. displays descriptive statistics and and treatment effects (ATT, calculated the same as in the main paper) for each of the individual components of these indices, grouped by family.

Table D1 displays four kin integration variables at the two- and four-year endlines. Table D2 displays the four community participation variables collected at the two-year endline. We changed the measurement of community participation at the four-year endline, try to get more and slightly better quality data, and these six variables are displayed in Table D3. Table D4 displays contributions to 7 kinds of public goods. Table D5 displays 2-year and 4-year ATEs for the eight aggression variables measured at both endlines, while Table D6 lists 4-year ATEs on the added 14 aggression variables collected. Table D7 displays 6 electoral participation ATEs, Table D8 displays 4 partisan political participation ATEs, and Table D9 displays 7 protest attitude and participation ATEs.

K.3 Impact heterogeneity: Aggression and anti-social behavior

The ATE 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 non-aggressive 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 19, we recalculate average treatment effects, by gender, adding the baseline level of aggression and interaction term between treatment and aggression. In general, none of our main conclusions about the aggression ATEs 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, and 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 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 two and four-year 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 19. The basic two-year ATEs on men and women's aggression are unchanged, but now more aggressive males do not seem to respond disproportionately. Meanwhile, the most aggressive females appear to become weakly more aggressive as a result of treatment, though the significance is weak. We see no evidence of an average treatment effect on the other four-year dependent variables. The extended aggression and ASB index results, however, have a negative and significant coefficient on the interaction. At first this looks like evidence of a fall in aggression among the most aggressive treated people. 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, but the ATE 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.

L Other outcomes measured

A handful of other measures were collected at endline, but not discussed in the main paper as they were not central outcomes of interest. These are displayed in Appendix Table 20.

First, we have a measure of the *Number of groups you belong to* in total, which has a sizable and significant positive ATE, but this could arise mechanically from the NUSAF group, and so is of limited interest.

Second, at the 2-year survey we collected data on 8 self-reported forms of social support received in the past four weeks from friends and family. Each is measured on a 0-2 scale from "no support received" to "yes, often"). Examples include whether or not someone: looked after a family member or the possessions of the respondent while they were away, or sat with the respondent when they were feeling distressed or lonely. We can assemble these into a standardized *Index of social support*. It is significantly higher among treated males. We did not collect these data after 4 years in favor of increasing aggression and political and community participation.

We also have some data on mental health. Poverty and adverse shocks appear to be associated with common mental disorders such as depression, and positive shocks have the potential to relieve stress or hopelessness and thus improve mental health.[10] We adapt an additive *Index of psy-*

[10] A large epidemiological and psychological literature finds a correlation between poverty and common mental disorders, including Crick Lund et al., "Poverty and Common Mental Disorders in Low and Middle Income Countries: A Systematic Review," *Social Science and Medicine* 71 (2010): 517–528; Crick Lund et al., "Poverty and Mental Disorders: Breaking the Cycle in Low-income and Middle-income Countries," *Lancet* 378 (2011): 1502–1514. This literature suggests that economic shocks are more likely determinants of mental health. Poverty could also affect mental health through insecurity, hopelessness, rapid social change, and heightened risks of violence and physical ill-health (Vikram Patel and Arthur Kleinman, "Poverty and Common Mental Disorders in Developing Countries," *Bulletin of the World Health Organization* 81, no. 8 (2003): 609–615.).

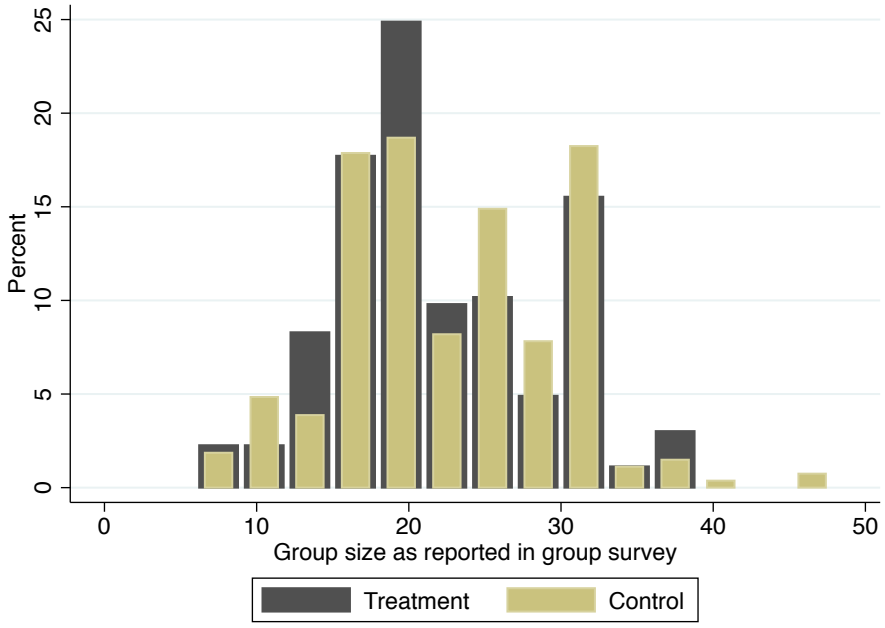
chological distress that runs from 0 to 21, using 7 self-reported symptoms of depression and anxiety, each rated 0 to 3 by frequency.[11] The *Index of Perseverance and Industriousness* is a standardized index generated from twelve measures of resilience in the face of difficulty, desire for success, and optimism.

Finally, we have several measures surrounding domestic violence and female empowerment. The *Female Empowerment Index* is an additive index from 0 to 6 indicating a wife's degree of financial independence from her husband. The *Average Support for Wife Abuse* index runs from 0 to 3 and is an average of four questions assessing attitudes toward wife beating, and *Does Not Admit to Hitting/Being Hit* is a dummy indicating admission of actual wife abuse.

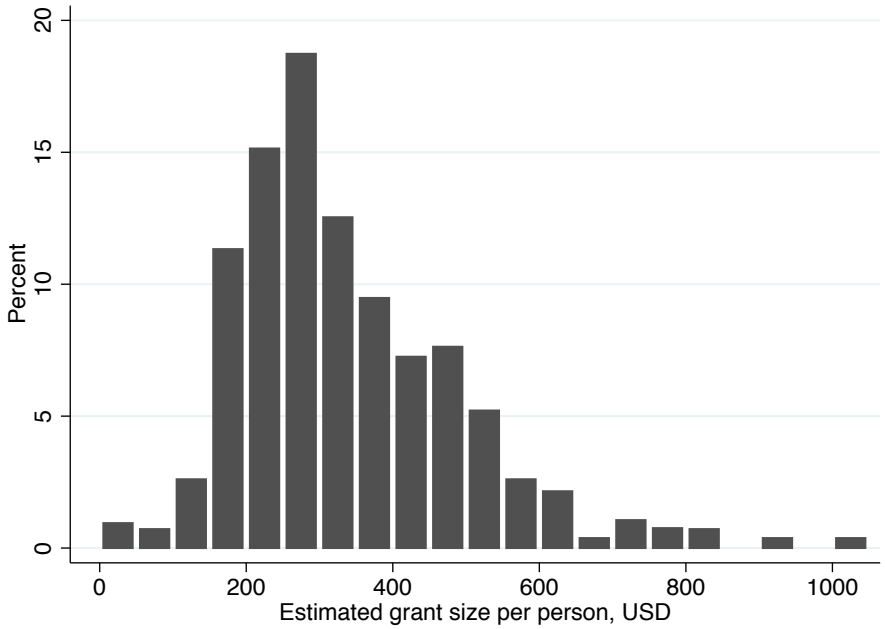
[11] Symptoms include feelings of isolation, nightmares, difficulty sleeping, hyper-arousal, etc. We adapt our 7-item scale from the 19-item distress scale used by the Survey of War Affected Youth (SWAY) in northern Uganda, by Christopher Blattman and Jeannie Annan, "The Consequences of Child Soldiering," *Review of Economics and Statistics* 92 (2010): 882–898.. All 19 symptoms were collected at baseline, and for the 7-item endline scale we took the 7 most influential items from the rotated first factor of all 19.

Appendix Figure 1: Group and grant size

i. Distribution of group size for 535 group (bin size = 3)

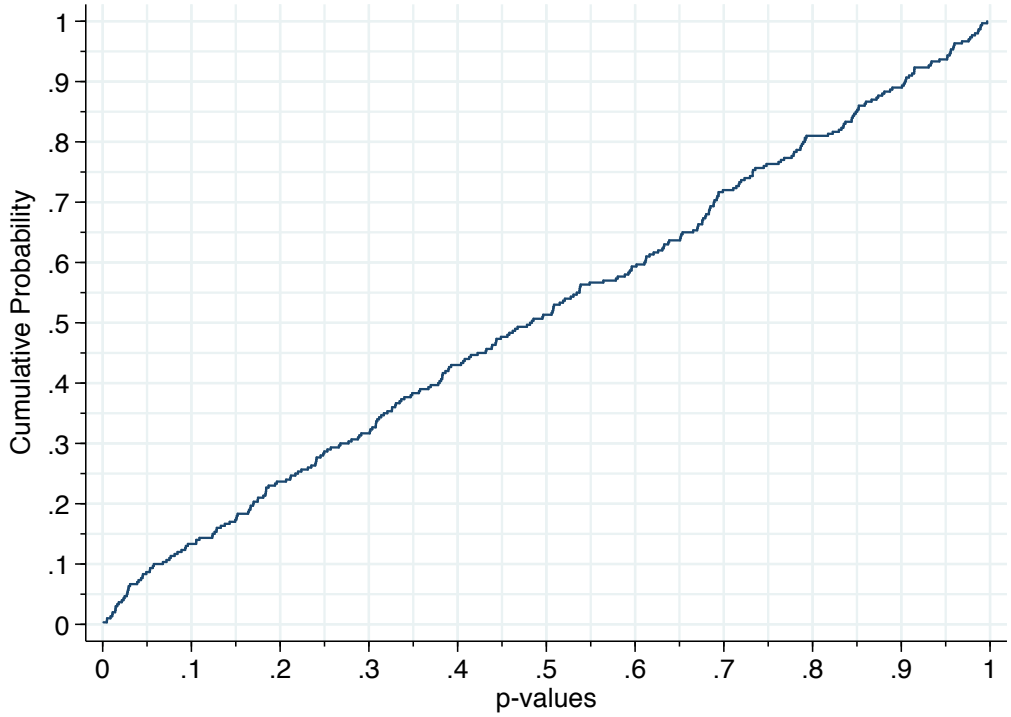


ii. Distribution of average grant size per person for 265 treatment groups



Notes: We exclude diverted or non-transferred funds from the total grant amount. UGX-denominated grants are converted to dollars at the 2008 market exchange rate of 1,915 UGX per USD. The bin width is $50.

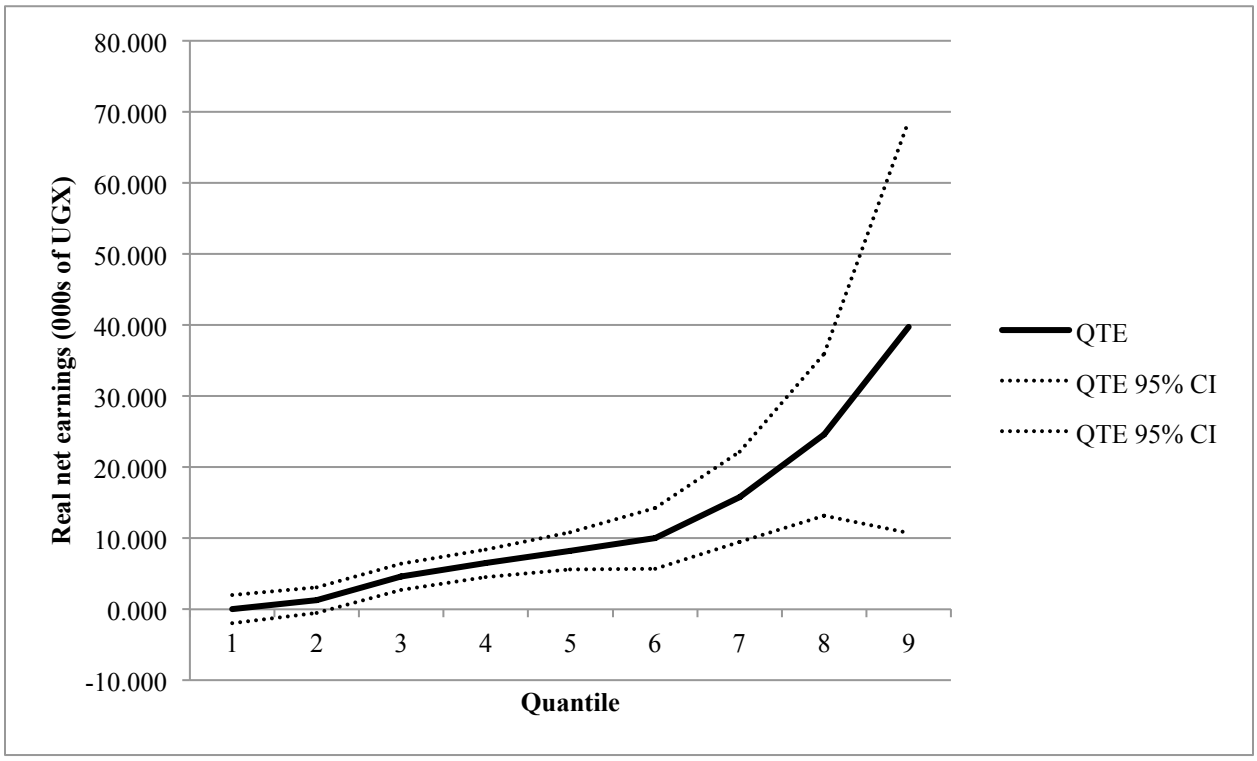
Appendix Figure 2: CDF of p-values from tests of balance of 300 baseline variables



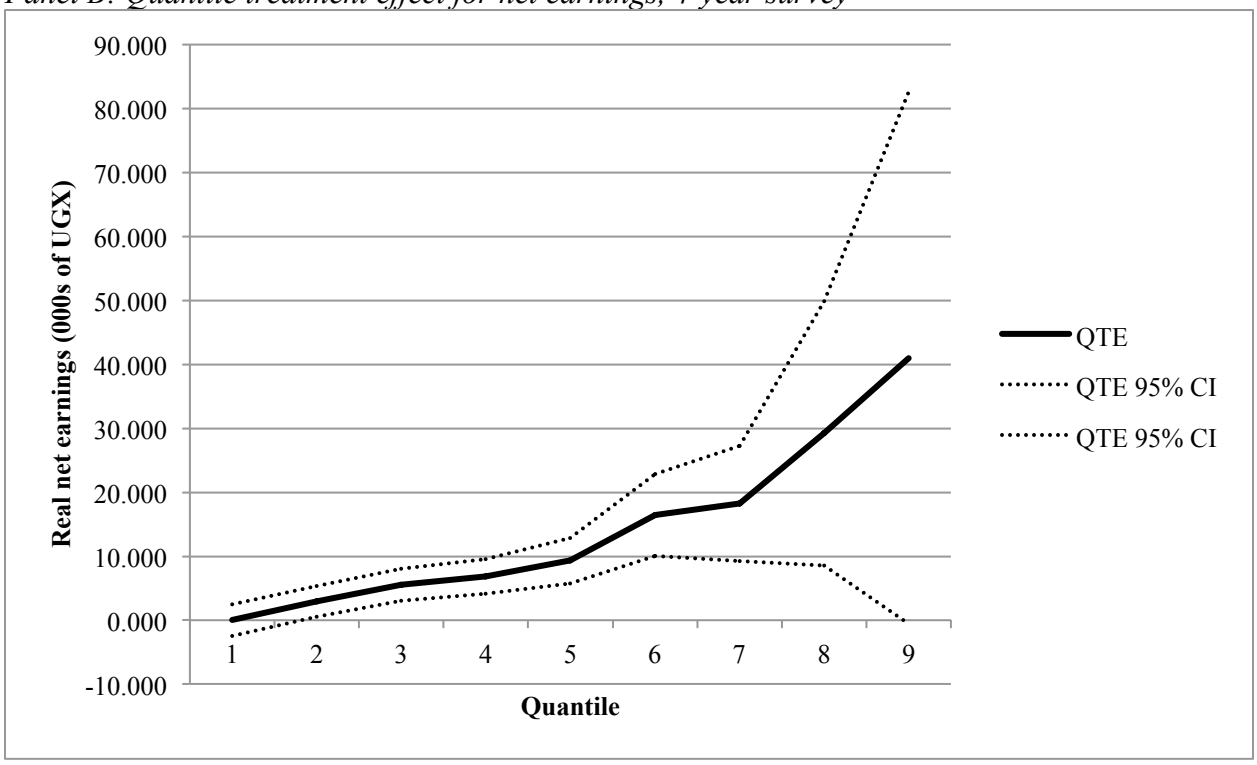
Notes: Each p-value is calculated based on the coefficient on assignment to treatment in a regression of the baseline variable on that treatment variable and district fixed effects, clustered by group.

Appendix Figure 3: Quantile Treatment Effects for Net Earnings

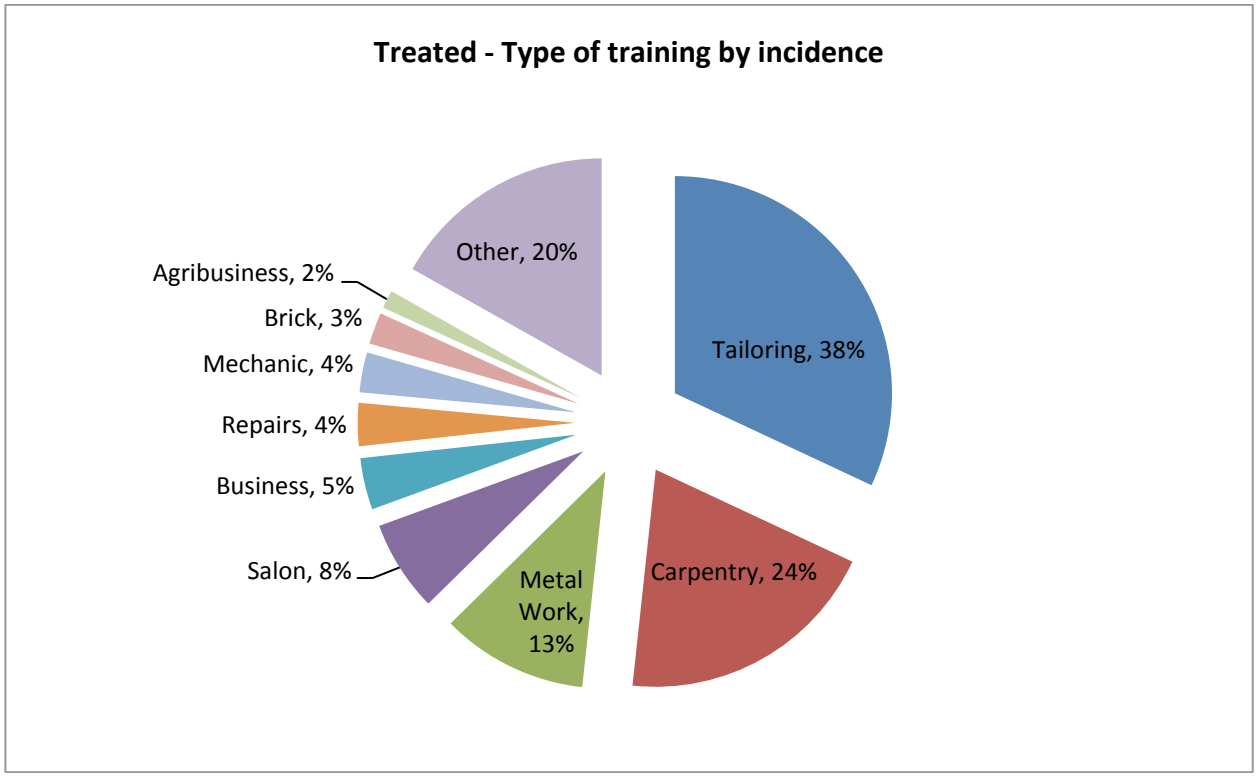
Panel A: Quantile treatment effect for net earnings, 2-year survey

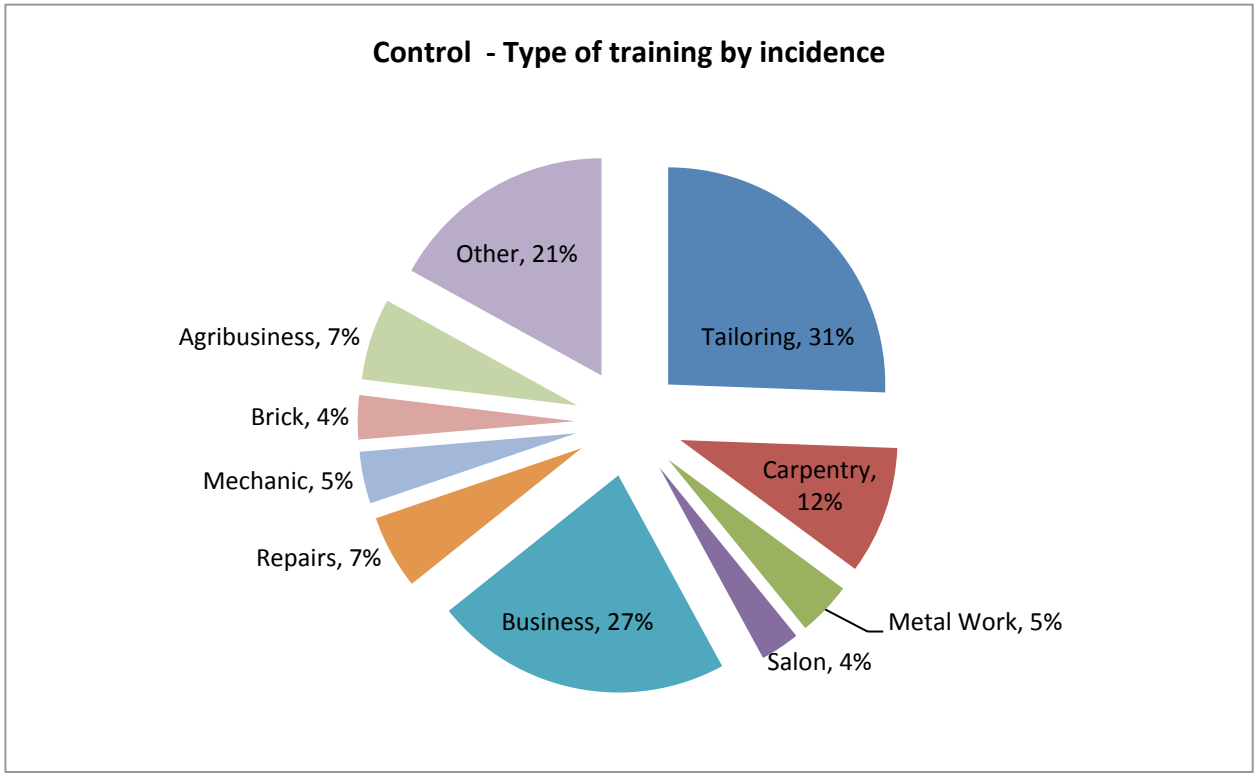


Panel B: Quantile treatment effect for net earnings, 4-year survey



Appendix Figure 4: Types of training (incidence), by treatment status, male and female





Appendix Table 1: Summary statistics for major variables, including tests of balance

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		**Treatment**		**Regression difference**	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Difference†**	**p-value**
Group-level characteristics (N=535)						
Group size as reported in group survey	22.2	7.1	21.4	6.8	0.09	0.86
1 if group existed before applying for NUSAF funding	0.45	0.50	0.49	0.50	0.03	0.42
How long the group has existed for	3.80	2.00	3.82	1.89	-0.01	0.98
Grant size in USD	7,497	2,220	7,275	2,025	18.3	0.89
Size of grant per person using group_size, USD	376	170	379	176	3.8	0.76
Estimated grant size per person, USD	363	160	382	171	14.0	0.24
Normalized measure of in-group dynamic	-0.02	1.02	0.02	0.98	0.05	0.52
Normalized measure of in-group heterogeneity	-0.03	0.93	0.03	1.06	-0.03	0.74
Individual-level characteristics						
Age at baseline	24.8	5.2	25.1	5.3	0.18	0.52
Female	0.35	0.48	0.32	0.47	-0.02	0.54
Currently engaged in skilled work (indicator)	0.21	0.41	0.22	0.41	0.01	0.76
Vocation	0.08	0.27	0.08	0.28	0.00	0.73
Total hours spent on market activities in past 4 weeks	39.0	79.5	38.9	73.5	0.05	0.99
Total hours spent on subsistence work in past 4 weeks	18.6	45.1	22.5	47.1	3.87	**0.08**
No cash earnings in past 4 weeks	0.25	0.43	0.24	0.43	-0.00	0.91
Did not complete primary school	0.25	0.44	0.29	0.45	0.02	0.24
Human capital index	0.01	0.95	-0.01	1.05	-0.01	0.68
Highest level reached at school	7.95	2.92	7.82	3.03	-0.06	0.68
1 if high or low literacy	0.75	0.43	0.71	0.45	-0.02	0.24
Prior vocational training dummy	0.07	0.26	0.08	0.28	0.02	**0.04**
ADL Index (additive bad)	8.68	2.52	8.63	2.21	-0.13	0.32
Mean: Number of digits correctly repeated in numeracy (endline)	4.2	2.0	4.0	2.0	-0.04	0.58
Working capital index	-0.04	0.90	0.04	1.09	0.08	**0.00**
Wealth index	-0.16	0.93	-0.08	1.03	0.08	**0.08**
Value of savings over past 6 months at baseline	19.2	98.2	32.9	137.1	11.59	**0.01**
Net cash earnings	62.2	129.0	67.7	135.1	7.94	0.24
Could you obtain a 100,000 USH loan?	0.33	0.47	0.40	0.49	0.06	**0.00**
Could you obtain a 1 million USH loan?	0.10	0.30	0.12	0.32	0.01	0.33

† Calculated by a regression of the outcome on assignment to treatment and randonimzation strata (district) fixed effects, with robust standard errors clustered at the group level

All UGX-denominated outcomes were censored at the 99th percentile to contain potential outliers

**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 2: Patience measures and treatment-control differences

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		**Treatment**		**Regression difference**	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Difference**[†]	**p-value**
Patience index (z-score)	0.00	0.95	-0.00	1.05	0.00	0.95
Good at resisting temptation	2.4	0.7	2.3	0.7	-0.03	0.31
Spend afternoon waiting for free med exam	1.6	0.9	1.6	0.9	0.01	0.88
Take warnings now for many years in advance	2.2	0.8	2.2	0.8	0.02	0.43
Sometimes not able to stop doing something that is wrong	1.8	1.0	1.8	0.9	-0.00	0.94
Keep postponing activities	1.8	0.9	1.7	0.9	-0.06	0.11
If you get money, you spend it too quickly	1.7	0.9	1.7	0.9	-0.01	0.86
Sometimes act quickly and not think about results of actions	1.9	0.9	1.8	0.9	-0.03	0.29
Regret many choices you have made in the past	1.1	0.8	1.0	0.8	0.00	0.89
Easy task first or hard task first	1.4	0.9	1.4	0.9	0.03	0.51
Medicine today vs. medicine in one week that will cure you	1.1	1.0	1.1	1.0	-0.00	0.97

† Calculated by a regression of the outcome on assignment to treatment and randonimzation strata (district) fixed effects, with robust standard errors clustered at the group level
All UGX-denominated outcomes were censored at the 99th percentile to contain potential outliers
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 3: Employment levels and occupational choice

	Baseline		4-year endline survey			
	Full sample		Control		Treatment	
	Average weekly hours	% of total hours	Average weekly hours	% of total hours	Average weekly hours	% of total hours
Agricultural activities						
Farming and animal raising	4.6	23%	16.7	44%	17.1	39%
Fuel collection	0.5	3%	0.8	2%	0.8	2%
Agricultural wage labor	0.5	3%	2.4	6%	1.8	4%
Total agricultural	*5.6*	*28%*	*19.8*	*52%*	*19.7*	*44%*
Non-agricultural activities						
Low skill wage labor	1.9	9%	3.3	9%	3.7	8%
Low-skill business						
Brewing	0.4	2%	0.4	1%	0.4	1%
Small repairs	0.8	4%	0.7	2%	0.6	1%
Petty trader/kiosk owner	2.2	11%	4.1	11%	4.4	10%
Brick-making	0.5	3%	0.7	2%	0.5	1%
Bicycle taxi	0.4	2%	1.0	3%	1.2	3%
Total low-skill non-agricultural	*6.2*	*31%*	*10.2*	*27%*	*10.8*	*24%*
Skilled trades						
Tailoring and weaving	2.5	12%	1.1	3%	2.5	6%
Carpentry	1.4	7%	0.7	2%	1.8	4%
Construction	0.9	4%	1.1	3%	1.6	4%
Metal working	0.9	5%	0.4	1%	0.9	2%
Hairdressing	0.6	3%	0.6	1%	0.7	1%
High-skill labor (e.g. teachers, health workers)	0.8	4%	2.8	8%	4.2	9%
Total skilled non-agricultural	*7.0*	*35%*	*6.7*	*18%*	*11.7*	*26%*
Total non-agricultural	*13.2*	*67%*	*16.8*	*44%*	*22.5*	*51%*
Other	1.0	5%	1.2	3%	2.2	5%
Total	**19.8**	**100%**	**37.8**	**100%**	**44.4**	**100%**

Appendix Table 4: Baseline correlates of endline economic success

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Success index [z-score][§]				**Log of real net monthly earnings**				**Wealth index**			
	Basic baseline characteristics		**Adding baseline indices**		**Basic baseline characteristics**		**Adding baseline indices**		**Basic baseline characteristics**		**Adding baseline indices**	
	Control	**Treatment**	**Control**	**Treatment**	**Control**	**Treatment**	**Control**	**Treatment**	**Control**	**Treatment**	**Control**	**Treatment**
Highest level reached at school	0.059	0.063			0.029	0.046			0.083	0.083		
	[0.009]***	[0.010]***			[0.029]	[0.026]*			[0.009]***	[0.011]***		
1 if high or low literacy	-0.005	0.010			0.182	0.113			-0.081	-0.029		
	[0.057]	[0.059]			[0.165]	[0.159]			[0.059]	[0.062]		
Prior vocational training dummy	0.065	-0.019			0.204	-0.085			0.047	-0.006		
	[0.101]	[0.091]			[0.280]	[0.247]			[0.092]	[0.089]		
Age at baseline	0.020	0.016	0.011	0.007	0.047	0.018	0.037	0.010	0.015	0.018	0.003	0.007
	[0.005]***	[0.005]***	[0.005]**	[0.005]	[0.014]***	[0.012]	[0.013]***	[0.013]	[0.005]***	[0.005]***	[0.006]	[0.006]
Female	-0.162	-0.072	-0.193	-0.098	-0.939	-0.467	-0.938	-0.542	0.083	0.047	0.036	0.033
	[0.058]***	[0.053]	[0.054]***	[0.056]*	[0.160]***	[0.150]***	[0.155]***	[0.149]***	[0.058]	[0.055]	[0.055]	[0.058]
Endline 2 indicator	0.270	0.332	0.273	0.308	0.587	0.587	0.599	0.530	0.222	0.312	0.222	0.295
	[0.056]***	[0.049]***	[0.051]***	[0.052]***	[0.152]***	[0.130]***	[0.148]***	[0.131]***	[0.054]***	[0.053]***	[0.052]***	[0.057]***
Human capital index			0.217	0.202			0.136	0.068			0.295	0.293
			[0.047]***	[0.042]***			[0.132]	[0.106]			[0.045]***	[0.046]***
Working capital index			0.222	0.211			0.352	0.243			0.224	0.245
			[0.048]***	[0.041]***			[0.135]***	[0.107]**			[0.052]***	[0.049]***
Patience index			0.071	0.130			0.067	0.342			0.093	0.081
			[0.059]	[0.055]**			[0.158]	[0.138]**			[0.063]	[0.062]
Risk aversion index			-0.087	-0.252			0.032	-0.272			-0.154	-0.296
			[0.060]	[0.058]***			[0.178]	[0.137]**			[0.055]***	[0.067]***
Observations	2,116	2,098	2,098	2,076	2,113	2,097	2,095	2,075	2,099	2,083	2,081	2,061
R-squared	0.142	0.175	0.175	0.142	0.070	0.066	0.080	0.049	0.172	0.194	0.179	0.150

Robust standard errors in brackets, clustered at the group level
**** p<0.01, ** p<0.05, * p<0.1*
§ The success index is an additive, standard normal composite measure of net earnings and the wealth index

Appendix Table 5: Survey response and attrition rates, all rounds

	(1)	(2)	(3)	(4)
	Phase 1			
	% selected	**# sought**	**# found**	**% found**
Baseline	100%	2,679	2,598	97%
2-year endline	100%	2,679	1,699	63%
4-year endline	100%	2,679	1,633	61%

	Phase 2			
	% selected	**# sought**	**# found**	**% found**
Baseline	--	--	--	--
2-year endline	53%	411	307	75%
4-year endline	39%	403	236	59%

	Overall	
	% unfound (all)	**Effective attrition***
Baseline	3%	3%
2-year endline	25%	9%
4-year endline	30%	16%

** Equal to (1-Phase 1 % found)*(1-Phase 2 % found)*

Appendix Table 6: Correlates of attrition

	(1)	(2)	(3)	(4)
	Unfound at 2-year endline		**Unfound at 4-year endline**	
Assigned to treatment	0.056 [3.052]***	0.075 [3.318]***	-0.034 [-1.468]	0.045 [1.704]*
Age at baseline		-0.006 [-3.222]***		-0.007 [-3.216]***
Female		-0.040 [-2.134]**		-0.053 [-2.568]**
Working capital index		0.034 [1.745]*		0.037 [1.667]*
Human capital index		-0.007 [-0.489]		0.022 [1.173]
Vocation		-0.046 [-1.595]		-0.021 [-0.522]
1 if treasurer, chairperson, vice-chair, or secretary at baseline		-0.022 [-1.270]		-0.035 [-1.492]
1 if chairperson or vice-chairperson at baseline		0.012 [0.436]		0.005 [0.162]
1 if group existed before applying for NUSAF funding		0.005 [0.225]		-0.026 [-1.043]
Normalized measure of in-group dynamic		-0.007 [-0.610]		-0.016 [-1.029]
Size of group according to baseline group roster		-0.002 [-1.232]		-0.005 [-2.405]**
Normalized measure of in-group heterogeneity		0.013 [1.248]		0.031 [2.524]**
Inequality of grant distribution		-0.020 [-1.900]*		-0.066 [-6.435]***
1 if respondent currently enrolled in school		-0.039 [-1.249]		-0.036 [-0.679]
Total hours spent on non-household activities in past week		0.000 [0.860]		0.001 [1.629]
Observations	2,244	2,244	2,032	2,006
R-squared	0.047	0.069	0.095	0.168
p-val	0.002	0.0140	0.143	0.000

Robust standard errors in brackets, clustered at the group level
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 7: ATEs on secondary investment outcomes

	Enrolled in vocational training since baseline	Returned to school since baseline	Real value of business assets acquired since baseline		Indicator of other state/NGO transfers	Real value of other state/NGO transfers
	2Y	2Y	2Y	4Y	2Y	2Y
ATE (All)	0.611	0.029	808.038	129.614	0.001	87.466
Std. Err.	[0.024]***	[0.017]*	[119.391]***	[68.074]*	[0.019]	[26.777]***
Control mean	0.152	0.103	247.7	317.1	0.137	28.04
ATE as % of mean	402%	28%	326%	41%	1%	312%
Male ATE	0.596	0.025	1,009.183	93.996	-0.003	98.354
Std. Err.	[0.030]***	[0.021]	[163.472]***	[92.601]	[0.022]	[32.055]***
Control mean	0.159	0.125	300.1	426.5	0.146	31.69
ATE as % of mean	375%	20%	336%	22%	-2%	310%
Female ATE	0.642	0.038	404.202	197.302	0.007	65.518
Std. Err.	[.038]***	[.027]	[128.609]***	[74.119]***	[.034]	[36.75]*
Control mean	0.138	0.0670	156.8	129.9	0.141	21.93
ATE as % of mean	465%	57%	258%	152%	5%	299%
Female - Male ATE	0.046	0.013	-604.981	103.306	0.010	-32.836
Std. Err.	[0.047]	[0.033]	[205.402]***	[113.719]	[0.039]	[43.452]
Observations	1997	2003	2003	1865	2598	2003

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capita
All UGX denominated variables censored at the 99th percentile.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 8: ATEs on secondary employment outcomes

	Total hours spent on chores in past 4 weeks		Keeps record of expenses and revenues?	Formally registered any businesses?	Pay any business taxes?	Indicator for has employees	Indicator for earns a wage		Average earnings per hour ('000s of UGX)	
	2Y	4Y	2Y	2Y	2Y	2Y	2Y	4Y	2Y	4Y
ATE (All)	-6.336	-2.083	0.120	0.051	0.084	-0.022	-0.030	0.047	-0.165	0.19
Std. Err.	[3.138]**	[3.469]	[0.027]***	[0.021]**	[0.027]***	[0.030]	[0.024]	[.026]*	[0.163]	[.081]**
Control mean	39.43	40.33	0.308	0.152	0.212	0.490	0.780	0.797	0.624	0.356
ATE as % of mean	-16%	-5%	39%	34%	40%	-4%	-4%	6%	-26%	53%
Male ATE	-1.419	0.395	0.132	0.071	0.104	0.025	-0.023	0.039	-0.243	0.255
Std. Err.	[2.272]	[2.443]	[0.033]***	[0.027]***	[0.032]***	[0.034]	[0.030]	[.032]	[0.234]	[.115]**
Control mean	10.93	10.16	0.350	0.174	0.221	0.497	0.820	0.791	0.737	0.407
ATE as % of mean	-13%	4%	38%	41%	47%	5%	-3%	5%	-33%	63%
Female ATE	-16.281	-6.929	0.095	0.009	0.042	-0.116	-0.044	0.062	-0.005	0.063
Std. Err.	[8.241]**	[9.045]	[.045]**	[.029]	[.044]	[.052]**	[.038]	[.042]	[.154]	[.067]
Control mean	88.48	90.69	0.235	0.116	0.194	0.478	0.835	0.807	0.425	0.263
ATE as % of mean	-18%	-8%	40%	8%	22%	-24%	-5%	8%	-1%	24%
Female - Male ATE	-14.862	-7.324	-0.037	-0.062	-0.062	-0.141	-0.021	0.023	0.238	-0.192
Std. Err.	[8.559]*	[9.395]	[0.055]	[0.037]*	[0.052]	[0.060]**	[0.046]	[.049]	[0.284]	[.13]
Observations	1996	1861	1971	1970	1975	2003	2598	2598	1857	1809

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 9: ATEs on other secondary economic outcomess

	Log of value of all current savings	Log of value of outstanding loans	Perceived access to credit index	Net household transfers (000s of UGX)	Adjusted Profits (000s of UGX)		Log of adjusted Profits	
	2Y	2Y	2Y	2Y	2Y	4Y	2Y	4Y
ATE (All)	0.572	0.210	0.075	-7.029	18.029	27.79	0.107	0.124
Std. Err.	[0.182]***	[0.177]	[0.042]*	[5.213]	[5.783]***	[8.636]***	[0.029]***	[.036]***
Control mean	7.704	10.92	0.839	6.792	22.13	50.39	4.670	4.796
ATE as % of mean	74%	2%	9%	-103%	81%	55%	9%	10%
Male ATE	0.782	0.117	0.114	-11.671	24.469	26.839	0.134	0.113
Std. Err.	[0.224]***	[0.224]	[0.049]**	[6.865]*	[7.719]***	[11.374]**	[0.039]***	[.048]**
Control mean	7.840	11.07	0.905	8.673	22.84	63.37	4.651	4.839
ATE as % of mean	113%	1%	13%	-135%	107%	42%	10%	1%
Female ATE	0.144	0.414	-0.003	2.328	5.03	29.569	0.052	0.147
Std. Err.	[.324]	[.313]	[.075]	[7.36]	[7.973]	[12.833]**	[.038]	[.049]***
Control mean	7.479	10.69	0.725	3.433	20.80	27.65	4.702	4.720
ATE as % of mean	10%	4%	0%	68%	24%	107%	1%	12%
Female - Male ATE	-0.638	0.297	-0.117	13.999	-19.439	2.73	-0.082	0.034
Std. Err.	[0.398]	[0.396]	[0.088]	[10.010]	[11.165]*	[17.232]	[0.055]	[.068]
Observations	1997	485	2003	2003	1996	1861	1990	1859

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
All UGX denominated variables censored at the 99th percentile.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 10: Correlates of training take-up

	Enrolled in vocational training since baseline					
	Basic baseline characteristics		Adding baseline indices		Adding group characteristics	
	Control	Treatment	Control	Treatment	Control	Treatment
Female	-0.010	0.015	-0.006	0.019	-0.020	0.031
	[0.025]	[0.033]	[0.025]	[0.033]	[0.026]	[0.035]
Age at baseline	-0.001	-0.004	-0.001	-0.004	-0.001	-0.005
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Highest level reached at school	0.007	-0.004				
	[0.004]	[0.006]				
Human capital index			0.028	-0.005	0.030	-0.001
			[0.021]	[0.028]	[0.020]	[0.030]
Working capital index			0.040	0.009	0.039	0.008
			[0.025]	[0.026]	[0.025]	[0.027]
Patience index			0.038	0.005	0.029	0.016
			[0.035]	[0.039]	[0.034]	[0.040]
1 if group existed before applying for NUSAF funding					0.052	0.104
					[0.030]*	[0.037]***
Normalized measure of in-group dynamic					-0.010	-0.011
					[0.018]	[0.018]
Size of group according to baseline group roster					-0.002	-0.003
					[0.002]	[0.003]
Proportion of group female (according to admin data)					-0.021	-0.054
					[0.055]	[0.072]
Normalized measure of in-group heterogeneity					-0.013	-0.023
					[0.016]	[0.016]
Observations	1,183	1,039	1,182	1,039	1,122	989
R-squared	0.056	0.154	0.063	0.154	0.086	0.160

Robust standard errors in brackets, clustered at the group level
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 11: Earnings patterns by trade (using earnings from that trade only)

Panel a: Earnings from each trade in the past 4 weeks among those who received the NUSAF cash grant ('000 UGX)

	All			Male				Female			
	Mean	Median	Std. Dev	Mean	Median	Std. Dev	N	Mean	Median	Std. Dev	N
Brick	62.25	0.00	173.78	64.50	0.00	178.54	95	26.67	0.00	56.10	6
Carpentry	46.74	20.00	69.01	47.43	20.00	69.39	230	6.50	2.50	9.95	4
Metalworking	42.04	20.00	65.00	43.07	20.00	65.65	103	6.67	0.00	11.55	3
Tailoring	19.71	10.00	33.62	21.25	10.00	30.10	139	18.65	8.00	35.87	203
Hair Salon	33.11	13.50	46.78	40.48	20.00	51.21	38	26.10	10.00	41.59	40

Panel b: Earnings from each trade in the past 4 weeks among those who received the grant ('000 UGX) -- Zero values excluded

	All			Male				Female			
	Mean	Median	Std. Dev	Mean	Median	Std. Dev	N	Mean	Median	Std. Dev	N
Brick	179.64	40.00	259.07	185.68	40.00	265.38	33	80.00	80.00	84.85	2
Carpentry	54.41	26.00	71.61	54.82	28.00	71.85	199	13.00	13.00	11.31	2
Metalworking	48.44	30.00	67.54	48.75	30.00	67.85	91	20.00	20.00	.	1
Tailoring	22.24	10.00	34.93	24.02	12.00	30.95	123	21.03	10.00	37.44	180
Hair Salon	36.37	20.00	47.82	42.73	20.00	51.70	36	29.83	12.00	43.24	35

Panel c: Average wage from each trade in the past 4 weeks among those who received the NUSAF cash grant ('000 UGX)

	All			Male				Female			
	Mean	Median	Std. Dev	Mean	Median	Std. Dev	N	Mean	Median	Std. Dev	N
Brick	2.96	0.00	15.03	1.62	0.00	5.85	93	23.70	0.00	56.98	6
Carpentry	1.24	0.63	1.89	1.24	0.63	1.90	221	0.80	0.80	0.35	2
Metalworking	1.14	0.42	2.48	1.17	0.44	2.50	101	0.07	0.00	0.13	3
Tailoring	0.54	0.25	1.01	0.56	0.30	0.78	137	0.53	0.21	1.14	200
Hair Salon	0.95	0.38	1.38	0.62	0.25	1.16	37	1.27	0.66	1.50	40

Panel d: Avg wage from each trade in the past 4 weeks among those who received the grant ('000 UGX) -- Zero values excluded

	All			Male				Female			
	Mean	Median	Std. Dev	Mean	Median	Std. Dev	N	Mean	Median	Std. Dev	N
Brick	8.36	1.67	24.59	4.56	1.67	9.20	33	71.11	71.11	97.42	2
Carpentry	1.40	0.75	1.96	1.40	0.75	1.96	196	0.80	0.80	0.35	2
Metalworking	1.29	0.50	2.60	1.30	0.50	2.61	91	0.22	0.22	.	1
Tailoring	0.61	0.31	1.05	0.63	0.36	0.80	121	0.59	0.25	1.19	179
Hair Salon	1.05	0.50	1.41	0.65	0.27	1.18	35	1.45	0.89	1.52	35

Appendix Table 12: Average log earnings by trade

Regressions on log of net earnings (all income sources), by incidence of main economic activity

	Brick-making	Bee-keeping	Bike repair	Shoe repair	Carpentry	Borehole repair	Metal-working	Weaving		Tailoring		Hair Salon	
Indicator for performed economic activity	0.796	0.850	0.887	0.829	1.224	0.719	1.625	0.820	1.123	1.083	0.987	1.370	1.391
	[0.183]***	[0.307]***	[0.176]***	[0.307]***	[0.130]***	[0.376]*	[0.182]***	[0.256]***	[0.197]***	[0.136]***	[0.177]***	[0.223]***	[0.246]***
Indicator for performed economic activity x Female									-0.580		0.169		-0.040
									[0.493]		[0.239]		[0.391]
Female	0.024	0.024	0.023	0.023	0.022	0.024	0.024	0.024	0.024	0.024	0.024	0.025	0.025
	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**	[0.010]**
Age	-0.784	-0.809	-0.779	-0.812	-0.685	-0.819	-0.755	-0.845	-0.830	-0.979	-1.002	-0.870	-0.868
	[0.120]***	[0.119]***	[0.120]***	[0.119]***	[0.121]***	[0.119]***	[0.119]***	[0.119]***	[0.120]***	[0.121]***	[0.134]***	[0.118]***	[0.121]***
Human Capital Index	0.212	0.209	0.221	0.210	0.217	0.215	0.212	0.214	0.213	0.207	0.207	0.198	0.198
	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**	[0.091]**
Physical Capital Index	0.405	0.412	0.408	0.412	0.413	0.406	0.394	0.407	0.407	0.420	0.418	0.392	0.392
	[0.093]***	[0.093]***	[0.093]***	[0.093]***	[0.093]***	[0.093]***	[0.092]***	[0.092]***	[0.092]***	[0.092]***	[0.092]***	[0.092]***	[0.092]***
Endline 2 indicator	0.578	0.580	0.585	0.588	0.584	0.589	0.596	0.587	0.589	0.596	0.595	0.585	0.585
	[0.088]***	[0.088]***	[0.088]***	[0.088]***	[0.088]***	[0.088]***	[0.088]***	[0.088]***	[0.088]***	[0.087]***	[0.087]***	[0.088]***	[0.088]***
Constant	8.416	8.412	8.436	8.433	8.335	8.474	8.432	8.470	8.466	8.396	8.406	8.411	8.410
	[0.406]***	[0.414]***	[0.407]***	[0.403]***	[0.406]***	[0.408]***	[0.403]***	[0.404]***	[0.404]***	[0.391]***	[0.392]***	[0.404]***	[0.404]***
Observations	4,218	4,218	4,218	4,218	4,218	4,215	4,215	4,215	4,215	4,215	4,215	4,215	4,215
R-squared	0.077	0.074	0.076	0.074	0.086	0.074	0.083	0.075	0.076	0.088	0.088	0.081	0.081

Robust standard errors in brackets, clustered by group and stratified by district.

The dependent variable is log earnings (from all activities), regressed on an indicator for whether the person practices that trade as their main trade.

Interactions with female indicator only created for those professions with sufficient number of female participants

**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 13: Calibration exercise

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Full sample	**ln(Real net earnings)**											
	Pooled	**2Y only**	**4Y only**	**Pooled**	**Pooled**	**2Y only**	**4Y only**	**Pooled**	**Pooled**	**2Y only**	**4Y only**	**Pooled**
ln(Business assets acquired)	0.128	0.137	0.112	0.125	0.128	0.137	0.112	0.125	0.128	0.137	0.112	0.125
	[0.009]***	[0.013]***	[0.013]***	[0.009]***	[0.009]***	[0.013]***	[0.013]***	[0.009]***	[0.009]***	[0.013]***	[0.013]***	[0.009]***
ln(Estimated cost of training), medium	0.030	0.030	0.035	0.033								
	[0.007]***	[0.010]***	[0.010]***	[0.007]***								
ln(Estimated cost of training), low					0.031	0.031	0.037	0.035				
					[0.008]***	[0.011]***	[0.011]***	[0.008]***				
ln(Estimated cost of training), high									0.029	0.029	0.034	0.032
									[0.007]***	[0.010]***	[0.010]***	[0.007]***
Observations	4,211	2,350	1,861	4,211	4,211	2,350	1,861	4,211	4,211	2,350	1,861	4,211
Indicator for 4Y survey round	N	N	N	Y	N	N	N	Y	N	N	N	Y
R-squared	0.122	0.138	0.127	0.132	0.122	0.138	0.127	0.132	0.122	0.138	0.127	0.132

Panel B: Control group only	**ln(Real net earnings)**		
	2Y only	**4Y only**	**Pooled**
ln(Business assets acquired)	0.138	0.125	0.137
	[0.017]***	[0.019]***	[0.013]***
ln(Estimated cost of training), medium	0.041	0.001	0.020
	[0.016]**	[0.021]	[0.013]
Observations	1,311	1,042	2,353
Indicator for 4Y survey round	N	N	N
R-squared	0.132	0.143	0.128

Panel C: Performance of model			
	2Y only	**4Y only**	**Pooled**
Diff. between real and calibrated ln(Earn	-0.11	-0.73	-0.41
Estimated α	0.14	0.13	0.14
Estimated β	0.04	0.00	0.02

Robust standard errors in brackets, clustered at the group level
Demographic controls omitted from all tables
The low, medium, and high estimates of the cost of training are 2000, 4000 and 6000 UGX per hour, based on low, medium and high survey-reported estimates.
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 14: Robustness of ATEs to alternative specifications

	ATT	ITT	ATT	ITT	ATT
	With controls	With controls	No controls*	No controls*	With controls
	Standard weights	Standard weights	Standard weights	Standard weights	Adjusted weights for group size
i. Real value of business asset stock					
Treatment	339.506	297.043	368.028	322.135	330.515
	[59.581]***	[52.290]***	[62.935]***	[55.355]***	[58.034]***
Observations	4,215	4,215	4,215	4,215	4,215
R-squared	0.075	0.073	0.047	0.044	0.074
ii. Log of real value of business asset stock					
Treatment	1.447	1.266	1.495	1.309	1.422
	[0.139]***	[0.123]***	[0.141]***	[0.125]***	[0.149]***
Observations	4,215	4,215	4,215	4,215	4,215
R-squared	0.154	0.150	0.131	0.128	0.145
iii. Real net cash earnings					
Treatment	18.799	16.447	20.011	17.515	19.112
	[3.806]***	[3.319]***	[4.050]***	[3.538]***	[4.001]***
Observations	4,211	4,211	4,211	4,211	4,211
R-squared	0.115	0.116	0.043	0.043	0.114
iv. Log of real net cash earnings					
Treatment	0.886	0.775	0.928	0.813	0.913
	[0.111]***	[0.098]***	[0.115]***	[0.101]***	[0.119]***
Observations	4,211	4,211	4,211	4,211	4,211
R-squared	0.090	0.090	0.049	0.049	0.088
v. Anti-social Behavior Index (z-score)					
Treatment	-0.007	-0.006	-0.000	-0.000	0.001
	[0.040]	[0.035]	[0.040]	[0.035]	[0.044]
Observations	4,222	4,222	4,222	4,222	4,222
R-squared	0.066	0.066	0.059	0.059	0.069

Robust standard errors in brackets, clus
*** p<0.01, ** p<0.05, * p<0.1*
** District dummies are always included because of different probability of selection in treatment by strata*

Appendix Table 15: Attrition bounds on the main ATEs

Mean standardized treatment effects under varying missing data assumptions

	(1) Lower minmax	(2) Lower 25	(3) Lower 10	(4) Unadjusted ATE estimate	(5) Upper 10	(6) Upper 25	(7) Upper minmax
2-year endline							
Skilled employment indicator	0.158	0.297	0.316	0.341	0.346	0.361	0.427
SE	[0.030]	[0.026]	[0.026]	[0.029]	[0.026]	[0.026]	[0.026]
% Change	36%	82%	90%	99%	102%	110%	145%
p-value	0%	0%	0%	0%	0%	0%	0%
Real net cash earnings	-51.29	10.15	13.78	17.42	19.75	22.26	114.40
SE	[8.95]	[4.15]	[4.11]	[4.70]	[4.08]	[4.09]	[10.20]
% Change	-43%	26%	37%	48%	57%	67%	370%
p-value	0%	1%	0%	0%	0%	0%	0%
Log of real net cash earnings	-0.58	0.69	0.80	0.88	0.97	1.04	2.39
SE	[0.19]	[0.13]	[0.13]	[0.15]	[0.13]	[0.13]	[0.18]
% Change	-6%	8%	10%	11%	12%	13%	31%
p-value	0%	0%	0%	0%	0%	0%	0%
Obs	2,591	2,591	2,591	1,996	2,591	2,591	2,591
4-year endline							
Skilled employment indicator	0.061	0.235	0.260	0.271	0.290	0.319	0.410
SE	[0.030]	[0.025]	[0.024]	[0.029]	[0.025]	[0.025]	[0.027]
% Change	12%	53%	60%	64%	70%	79%	116%
p-value	4%	0%	0%	0%	0%	0%	0%
Real net cash earnings	-93.76	12.56	17.62	20.51	23.28	29.45	112.09
SE	[12.68]	[5.00]	[4.95]	[5.70]	[4.95]	[4.98]	[10.34]
% Change	-68%	24%	36%	43%	50%	67%	281%
p-value	0%	1%	0%	0%	0%	0%	0%
Log of real net cash earnings	-1.08	0.67	0.81	0.90	0.97	1.13	2.69
SE	[0.22]	[0.13]	[0.13]	[0.16]	[0.13]	[0.13]	[0.20]
% Change	-11%	8%	9%	10%	11%	13%	34%
p-value	0%	0%	0%	0%	0%	0%	0%
Obs	2,594	2,594	2,594	1,861	2,594	2,594	2,594

(1) imputes minimum value of each variable in the non-attrited treatment distribution to attrited in treatment group maximum value of non-attrited control distribution to attrited in control group
(2) imputes mean minus 0.25 s.d. of the non-attrited treatment distribution to attrited in treatment group mean plus 0.25 s.d. of the non-attrited control distribution to attrited in control group
(3) imputes mean minus 0.10 s.d. of the non-attrited treatment distribution to attrited in treatment group mean plus 0.10 s.d. of the non-attrited control distribution to attrited in control group
(4) mean standardized treatment effect on the non-attrited
(5) imputes mean plus 0.10 s.d. of the non-attrited treatment distribution to attrited in treatment group mean minus 0.10 s.d. of the non-attrited control distribution to attrited in control group
(6) imputes mean plus 0.25 s.d. of the non-attrited treatment distribution to attrited in treatment group mean minus 0.25 s.d. of the non-attrited control distribution to attrited in control group
(7) imputes maximum value of each variable in the non-attrited treatment distribution to attrited in treatment group minimum value of non-attrited control distribution to attrited in control group

Appendix Table 16: Treatment heterogeneity in business asset stocks

	(1)	(2)	(3)	(4)	(5)	(6)
	Log of real value of business asset stock					
	Full sample	**Those without an existing skilled trade**				
Treated	1.848	1.909	1.935	1.941	1.914	1.913
	[0.200]***	[0.203]***	[0.203]***	[0.203]***	[0.203]***	[0.202]***
Treated X 4Y endline	-0.770	-0.855	-0.854	-0.861	-0.844	-0.878
	[0.262]***	[0.274]***	[0.274]***	[0.273]***	[0.274]***	[0.272]***
4Y endline	0.885	0.969	0.965	0.967	0.956	0.966
	[0.166]***	[0.174]***	[0.174]***	[0.174]***	[0.174]***	[0.173]***
Treated X Skilled trade	-0.309					
	[0.405]					
Existing skilled trade	0.921					
	[0.253]***					
Treated X Working capital index	-0.434	-0.433				-0.454
	[0.146]***	[0.150]***				[0.263]*
Working capital index	0.428	0.403				0.405
	[0.136]***	[0.144]***				[0.178]**
Treated X Human capital index			-0.327			-0.035
			[0.134]**			[0.225]
Human capital index			0.259			0.027
			[0.121]**			[0.147]
Treated X Patience index				-0.265		0.260
				[0.137]*		[0.283]
Patience index				0.540		0.282
				[0.158]***		[0.199]
Treated X Risk Aversion Index					-0.364	-0.186
					[0.137]***	[0.294]
Risk Aversion Index					0.550	0.485
					[0.202]***	[0.244]**
Treated X Female						
Female	-0.442	-0.383	-0.356	-0.343	-0.363	-0.326
	[0.123]***	[0.125]***	[0.128]***	[0.126]***	[0.126]***	[0.129]**
R-squared	0.168	0.160	0.158	0.161	0.159	0.164
Obs	3850	3535	3535	3533	3505	3504
Control Mean	9.698	9.595	9.595	9.595	9.595	9.595

Robust standard errors in brackets, clustered by group and stratified by district Omitted regressors include an age quartic, district indicators, and baseline measures of employmnet and human and working capital
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 17: Non-experimental associations between poverty and social outcomes

	Family cohesion outcomes (z-score)	Community participation outcomes (z-score)	Community public good contributions (z-score)	Electoral participation outcomes (z-score)	Anti-social behavior and dispute outcomes (z-score)	Anti-social behavior and dispute outcomes extended (z-score)	Protest attitudes and participation (z-score)
Wealth index change	0.034	0.033	0.040	0.088	0.005	-0.030	-0.053
	[0.028]	[0.021]	[0.030]	[0.038]**	[0.022]	[0.032]	[0.029]*
Wealth index, baseline	-0.034	-0.011	0.028	0.071	-0.004	0.009	0.040
	[0.038]	[0.029]	[0.046]	[0.051]	[0.027]	[0.047]	[0.044]
Log net earnings change	0.008	0.013	0.030	0.013	0.017	0.047	0.031
	[0.009]	[0.007]*	[0.012]**	[0.013]	[0.008]**	[0.012]***	[0.011]***
Log net earnings, baseline	-0.002	0.010	0.014	0.003	0.027	0.057	0.024
	[0.014]	[0.012]	[0.019]	[0.020]	[0.011]**	[0.019]***	[0.021]
Log employment hours change	-0.002	-0.002	-0.012	-0.015	-0.009	0.007	-0.005
	[0.008]	[0.006]	[0.012]	[0.014]	[0.008]	[0.012]	[0.012]
Log employment hours, baseline	-0.007	0.019	-0.002	-0.036	-0.019	-0.007	-0.022
	[0.013]	[0.012]	[0.020]	[0.024]	[0.013]	[0.020]	[0.019]
Female	-0.230	-0.399	-0.140	-0.164	-0.023	0.170	0.033
	[0.059]***	[0.054]***	[0.072]*	[0.082]**	[0.058]	[0.083]**	[0.077]
4-year endline indicator	-0.009	0.079			-0.072		
	[0.052]	[0.050]			[0.048]		
Observations	1,936	1,936	923	923	1,936	923	923
R-squared	0.141	0.161	0.136	0.147	0.068	0.104	0.102

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
**** p<0.01, ** p<0.05, * p<0.1*

Table A18: Other outcomes and summary statistics	(1)	(2)	(3)	(4)	(5)	(6)
	2 Year Endline		4 Year Endline		Pooled Endlines	
	Control Mean	Treatment Mean	Control Mean	Treatment Mean	ATE	Standard Error
Investments in vocational skills and capital						
Hours of training received	48.94	378.32	--	--	389.061	[23.813]***
Real value of business asset stock	299.39	737.14	392.45	607.82	470.811	[91.756]***
Income, poverty and employment						
Real net cash earnings	36.10	51.40	47.90	65.43	17.827	[4.795]***
Monthly employment hours	120.91	139.29	147.05	170.85	21.032	[6.003]***
Index of wealth	-0.05	0.04	0.16	0.33	0.100	[0.055]*
Short term expenditures	--	--	51.67	55.72	7.268	[2.032]***
Current position on wealth ladder: 1 to 9	2.73	3.05	3.29	3.73	0.369	[0.087]***
Social integration						
Family caring index	2.68	2.65	2.75	2.71	-0.011	[0.037]
Family harmony index	3.71	3.73	3.89	3.92	0.021	[0.034]
Lives with a partner dummy	0.73	0.77	0.79	0.82	0.021	[0.025]
Elder relations index	2.57	2.56	2.54	2.50	-0.029	[0.039]
Community participation and engagemennt						
Number of group memberships	3.31	3.35	1.62	1.73	0.185	[0.110]*
Attends community meetings	0.67	0.70	--	--	0.033	[0.027]
Community mobilizer	0.51	0.60	0.60	0.62	0.046	[0.026]*
Community leader	0.40	0.41	--	--	0.014	[0.027]
Speaks out at community meetings	0.61	0.66	--	--	0.051	[0.026]*
Frequency that community meeting in past year	--	--	1.12	1.07	0.035	[0.052]
Are you a member of the LC1 committee?	--	--	0.13	0.15	0.024	[0.020]
Are you currently a member of any committee that makes decisions that affect a large portion of the	--	--	0.28	0.27	-0.012	[0.027]
If nominated to become an LC1 by your community and you had the time, would you want to hold such a	--	--	0.69	0.67	-0.008	[0.027]
Got together with others to raise an issue	--	--	0.55	0.58	0.030	[0.030]
Contributions to public goods						
In the past 12 months did you contribute to road maintenance in your community?	--	--	0.50	0.50	-0.019	[0.030]
In the past 12 months did you contribute to construction or maintenance of community wells, hand-pumps, and other water sources?	--	--	0.50	0.50	-0.010	[0.030]
In the past 12 months did you contribute to construction, repair or maintenance of community buildings, such as grain stores or community halls?	--	--	0.29	0.33	0.017	[0.016]
In the past 12 months did you contribute funds to school fundraising?	--	--	0.49	0.49	0.018	[0.029]
In the past 12 months did you contribute to construction, repair, or maintenance of community latrines?	--	--	0.36	0.37	0.007	[0.020]
In the past 12 months did you contribute to any other community fundraising or work?	--	--	0.45	0.46	0.022	[0.025]
In the past 12 months did you contribute to funeral expenses of someone outside of your family?	--	--	0.43	0.45	-0.009	[0.023]
Aggressive and hostile behaviors						
Are you quarrelsome: 0 to 3	0.30	0.29	0.34	0.32	0.026	[0.034]
Do you take things from other places without permission: 0 to 3	0.14	0.13	0.07	0.06	-0.004	[0.028]
Do you curse or use abusive language: 0 to 3	0.12	0.12	0.08	0.07	0.006	[0.025]
Do you threaten to hurt others: 0 to 3	0.15	0.13	0.08	0.11	-0.028	[0.028]
Disputes with neighbors: 0 to 3	0.20	0.20	0.09	0.14	-0.013	[0.032]
Disputes with community leaders: 0 to 3	0.08	0.06	0.01	0.01	-0.030	[0.018]*
Disputes with police: 0 to 3	0.05	0.02	0.01	0.01	-0.040	[0.016]**

	2 Year Endline		4 Year Endline		Pooled Endlines	
	Control Mean	Treatment Mean	Control Mean	Treatment Mean	ATE	Standard Error
Involved in physical fights: 0 to 3	0.05	0.05	0.03	0.04	0.003	[0.014]
Aggressive and hostile behaviors (extended)						
You yelled at others when they have annoyed you.	--	--	0.66	0.75	0.037	[0.051]
You reacted angrily when provoked by others.	--	--	0.69	0.79	-0.030	[0.048]
You got angry when frustrated.	--	--	1.34	1.23	0.063	[0.063]
You damaged things because you felt mad.	--	--	0.15	0.13	-0.016	[0.027]
You become angry or mad when you don't get to do things.	--	--	1.20	1.05	0.001	[0.061]
You got angry when others threatened you.	--	--	1.40	1.26	-0.015	[0.059]
You felt better after hitting or yelling at someone.	--	--	0.24	0.25	-0.019	[0.035]
You hit others to defend yourself.	--	--	0.39	0.43	0.048	[0.043]
You damage things for fun.	--	--	0.14	0.12	-0.006	[0.024]
You used physical force to get others to do what you want.	--	--	0.18	0.12	-0.031	[0.027]
You used force to obtain money or things from others.	--	--	0.09	0.10	-0.006	[0.022]
You have gotten others to gang up on someone else.	--	--	0.10	0.09	-0.020	[0.023]
You carried a weapon to use in a fight.	--	--	0.05	0.03	-0.011	[0.014]
You yelled at others so they would do things for you.	--	--	0.28	0.32	0.021	[0.035]
Election Action						
Attended a voter education meeting	--	--	0.49	0.55	0.028	[0.029]
Got together with other to discuss who to vote for	--	--	0.56	0.55	-0.024	[0.029]
Reported a campaign malpractise or incident	--	--	0.11	0.15	0.030	[0.020]
Successfully registered in 2011	--	--	0.98	0.99	0.005	[0.008]
Voted in the presidential election	--	--	0.91	0.93	-0.001	[0.016]
Voted in the LCV election	--	--	0.87	0.90	0.013	[0.018]
Index of partisan political action						
Attended an election rally	--	--	0.99	1.01	0.058	[0.056]
Participated in an political primary	--	--	0.95	1.00	0.054	[0.055]
Worked to get a candidate or party elected	--	--	0.97	1.04	0.119	[0.059]**
Are you a member of any political party?	--	--	0.93	0.95	0.040	[0.056]
Protest Attitudes						
Protest attendance index (0-3)	--	--	0.02	0.03	-0.004	[0.013]
In your mind, do you feel that these reasons justified an act of protest?	--	--	0.47	0.45	0.006	[0.021]
Do you feel that the protesters were justified in committing violent acts, such as destroying property, and attacking police and military, in the Kampala protests?	--	--	0.22	0.23	0.001	[0.016]
Do you feel that the police and the military were justified in having a violent response to the gatherings of protesters in Kampala?	--	--	0.37	0.38	-0.020	[0.021]
Do you wish there would have been a protest in your district during the period of the major protests in Kampala?	--	--	0.09	0.08	-0.008	[0.015]
If there was a protest for similar reasons in your district now would you go?	--	--	0.09	0.09	0.003	[0.016]
If the protest turned violent, would you stay to participate in the violence, or would you leave?	--	--	0.07	0.09	0.014	[0.014]

All treatment effects are treatment on the treated (TOT)
**** p<0.01, ** p<0.05, * p<0.1*
All UGX-denominated outcomes were censored at the 99th percentile to contain potential outliers

Appendix Table 19: Aggression heterogeneity analysis

Panel A: Using baseline aggression	Aggression index				Extended aggression index		Protests		Human capital index	
	2Y Male	**2Y Female**	**4Y Male**	**4Y Female**	**4Y Male**	**4Y Female**	**4Y Male**	**4Y Female**	**BL Male**	**BL Female**
Treated	-0.208	0.227	0.044	0.063	0.019	0.012	-0.003	-0.056	0.094	0.034
	[0.065]***	[0.091]**	[0.058]	[0.097]	[0.060]	[0.097]	[0.063]	[0.095]	[0.040]**	[0.060]
Treated X Aggression index	-0.173	-0.047	-0.054	0.065	-0.020	0.040	-0.001	0.001	0.046	-0.058
	[0.073]**	[0.088]	[0.065]	[0.098]	[0.067]	[0.105]	[0.071]	[0.094]	[0.036]	[0.056]
Aggression index	0.114	0.090	0.030	0.039	0.066	0.033	-0.055	-0.016	-0.046	-0.008
	[0.056]**	[0.049]*	[0.044]	[0.051]	[0.052]	[0.062]	[0.051]	[0.061]	[0.022]**	[0.030]
Observations	1,328	667	1,228	627	1,228	627	1,228	627	1,496	738
R-squared	0.124	0.236	0.056	0.124	0.067	0.122	0.068	0.111	0.684	0.662

Panel B: Using aggression p-score	Aggression index				Extended aggression index		Protests		Human capital index	
	2Y Male	**2Y Female**	**4Y Male**	**4Y Female**	**4Y Male**	**4Y Female**	**4Y Male**	**4Y Female**	**BL Male**	**BL Female**
Treated	-0.202	0.212	0.033	0.093	-0.002	0.045	-0.003	-0.05	0.071	0.08
	[0.067]***	[0.088]**	[0.061]	[0.089]	[0.061]	[0.092]	[0.063]	[0.094]	[0.038]*	[0.050]
Treated X Aggression p-score	-0.075	0.129	-0.098	-0.159	-0.169	-0.218	-0.047	0.023	0.078	-0.089
	[0.063]	[0.077]*	[0.069]	[0.136]	[0.068]**	[0.120]*	[0.067]	[0.115]	[0.039]**	[0.090]
Aggression p-score	0.098	0.022	0.193	0.235	0.296	0.245	-0.025	0.07	0.423	0.446
	[0.073]	[0.071]	[0.078]**	[0.097]**	[0.079]***	[0.094]***	[0.075]	[0.091]	[0.048]***	[0.126]***
Observations	1318	665	1217	626	1217	626	1217	626	1483	736
R-squared	0.122	0.236	0.061	0.133	0.078	0.13	0.067	0.113	0.736	0.703

Robust standard errors in brackets, clustered by group
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table A20: Other family outcomes

	Number of groups belongs to (capped at 6), possibly including NUSAF group		Social support outcomes (Z score)	Depression and distress index (Z score)		Perseverence and industriousness (z-score)	Female Empowerment index: 0 to 6	Average support for wife abuse: 0 to 3	Does not admit to hitting/being hit
	2Y	4Y	2Y	2Y	4Y	4Y	2Y	2Y	2Y
ATE (All)	0.297	0.175	0.133	-0.056	-0.018	0.294	0.048	0.026	0.013
Std. Err.	[0.104]***	[0.065]***	[0.049]***	[0.050]	[0.058]	[0.202]	[0.100]	[0.034]	[0.017]
Control mean	3.314	1.619							
ATE as % of mean	9%	11%							
Male ATE	0.312	0.212	0.151	-0.118	-0.034	0.377	0.057	0.043	-0.013
Std. Err.	[0.123]**	[0.081]***	[0.058]***	[0.058]**	[0.067]	[0.251]	[0.122]	[0.039]	[0.021]
Control mean	3.473	1.690							
ATE as % of mean	9%	13%							
Female ATE	0.266	0.103	0.096	0.069	0.011	0.133	0.028	-0.01	0.065
Std. Err.	0.181	0.107	0.0876	0.0977	0.104	0.329	0.177	0.0622	0.0293
Control mean	3.030	1.501							
ATE as % of mean	9%	7%							
Female - Male ATE	-0.046	-0.109	-0.055	0.187	0.045	-0.244	-0.029	-0.053	0.078
Std. Err.	[0.214]	[0.134]	[0.103]	[0.114]*	[0.121]	[0.410]	[0.216]	[0.071]	[0.036]**
Observations	1995	1856	2003	2003	1865	1859	1972	1992	1997

Robust standard errors in brackets, clustered by group and stratified by district.
Omitted regressors include an age quartic, district indicators, and baseline measures of employment and human and working capital.
**** p<0.01, ** p<0.05, * p<0.1*