THE ECONOMIC AND SOCIAL RETURNS TO CASH TRANSFERS: EVIDENCE FROM A UGANDAN AID PROGRAM[1]

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Abstract:

A growing share of the world is young, poor, and underemployed. A widespread view is such people have high returns to investment but are credit constrained. Hence, aid in the form of capital or credit should expand occupational choice, self-employment and earnings. The evidence from transfers to established businesspeople is optimistic (at least for men), but little of this evidence concerns the young and unemployed. Meanwhile, the evidence from credit or highly conditional transfers of capital to the poorest is ambiguous or pessimistic. The ideal experiment—cash transfers for self-employment—is rare. To fill this gap, we randomize a large cash transfer program in northern Uganda. We follow thousands of largely unemployed youth two and four years after receiving grants worth twice their annual income. Most invest the transfers in vocations and earnings rise by at least 40%, especially among the more credit-constrained, patient, and risk-averse. Both male and female returns are high, yet while male controls catch up over time, female controls do not, suggesting the credit constraints facing Ugandan women are more severe. Finally, we consider the social returns to cash transfers. Poor, unemployed men are associated with social fragmentation and unrest and employment programs are predicated on their potential to reduce these negative social externalities. Despite impressive economic gains, however, we see almost no impact on cohesion, aggression, peaceful collective action, or violent protest. These results challenge a large body of theory and a common policy rationale for enhanced public spending on jobs and poverty relief.

1 Introduction

A third of the world's population is 15 to 34 years old and lives in a developing nation, and most of these youth are poor and underemployed (World Bank, 2012, 2007). A central question in development economics is how to expand non-agricultural employment for such youth and catalyze structural change (Acemoglu, 2008; Kuznets, 1966). The long run answer includes fostering large firms and formal employment. The transition, however, involves growth in small enterprise and informal employment, especially for immediate poverty relief. Academics and policymakers thus face a challenge: how to stimulate small enterprise growth and self-employment?

Most governments have a second motive for fostering jobs: social cohesion and stability. Large numbers of poor, unemployed young men are thought to weaken social bonds, reduce civic engagement, and heighten the risk of instability (e.g. Blattman and Miguel, 2010; Fuller, 1995; Goldstone, 2002; Kaplan, 1994; Kristof, 2010; World Bank, 2012). In this view, there are social externalities to job creation and poverty alleviation. Correlations between income and social measures often support this view—indeed, in our panel, levels of and changes in wealth, earnings and employment are associated with more social cohesion and peaceful collective action. Thus, in addition to testing theories and methods of income and employment growth, this paper experimentally tests for downstream effects on social cohesion and the risk of unrest.

Some of the most common microeconomic interventions—microcredit, cash transfers, and input distribution—are based on the premise that financial underdevelopment holds back enterprise growth. This idea is rooted in macroeconomic theory and evidence showing that economies with little financial depth have lower investment, innovation, and growth (Levine, 1997). It is supported by microeconomic theory and evidence arguing the poor have high returns to investment but are constrained from reaching them by market imperfections such as limited credit (Banerjee and Duflo, 2005; Banerjee and Newman, 1993; de Mel et al., 2008; Karlan and Zinman, 2009; King and Levine, 1993; Townsend, 2011).

Credit constraints are harmful to all, but are especially challenging for people entering informal labor markets and self-employment with little capital, such as the young. Credit constraints slow investment and will increase the time it takes new enterprises to reach their potential. If there are also increasing returns to production (such as start-up costs), then credit constraints could trap people in subsistence labor. In this case, interventions that give cash, credit, or inputs should increase occupational choice and spur investment and earnings, especially among the

young and poor. We illustrate this claim with a model of occupational choice, and test it with an experimental evaluation of a large cash transfer program to young men and women in Uganda.

Existing evidence for this theory is thin. A handful of 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).[1] They observe growth in the intensive margin, in existing businesses. Little of this evidence, however, concerns the poorest and unemployed, or growth on the extensive margin of employment.

The evidence from programs for the poorest, moreover, is ambiguous. One set of experiments study conditional cash transfer (CCT) programs, which tie transfers to child health and schooling obligations. Few of these, unfortunately, examine whether transfers are invested in enterprise (Fizbein et al., 2009). The few that do reach opposing conclusions.[2] Another set of experiments study the effects of giving "ultra-poor" rural dwellers livestock, skills training, and short-term income support. These programs increase assets and food security, but effects on production and earnings are mixed.[3] Finally, several unpublished microfinance experiments show, broadly speaking, increased borrowing and investment in existing household enterprises (such as livestock raising), but no apparent effect on new enterprise growth, profits, or consumption.[4]

How to interpret this evidence? One possibility is that other constraints bind the poor, such as social obligations to share or self-control problems. It is also possible that few of the poor and unemployed have high returns to investment, and so are not constrained by the lack of capital or credit. A third possibility, however, is that neither type of intervention offers a strong test of the theory, because of restrictions on the transfer. The framing and conditionality of CCT and ultra-poor programs may prevent investment in the highest-return new enterprises. With microcredit,

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[1] An experiment providing microcredit to small entrepreneurs on the margins of being rejected for loans shows an increase in borrowing but no increase in the size, number, or profitability of microenterprises, perhaps because of high annual interest rates of 200% (Karlan and Zinman, 2011).

[2] 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 experiment estimates about a quarter of a conditional transfer was invested in informal enterprise (Gertler et al., 2012).

[3] A four-year study of a BRAC program for Bangladesh women finds large increases in self-employment and earnings (Bandiera et al., 2013). However, two- to three-year studies of similar BRAC programs in Pakistan, Honduras and Ethiopia show little change in business activity or agricultural production (Goldberg, 2013).

[4] See Banerjee et al (2013) for an overview of the studies (Angelucci et al., 2012; Attanasio et al., 2011; Augsburg et al., 2012; Crépon et al., 2011).

meanwhile, interest rates exceeding 100%, abrupt repayment plans, and low tolerance for default may mean that the credit constraint on high return enterprises may not be relaxed.

Ideally, we would like experimental evidence on the effects of cash transfers on the poor, and employment growth on the extensive margin—similar to the small body of evidence on established entrepreneurs and intensive firm development.[5] To test the role of credit constraints, we can also go beyond average treatment effects, and use pre-intervention data to identify the heterogeneous responses to treatment that are consistent with alternative 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, the risk averse, and those without an enterprise.[6]

Focusing on economic returns alone, however, ignores two potentially large social externalities to employment creation, ones that could justify greater public investment. First, employment is commonly associated 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 strong correlation between employment and levels of trust, civic participation, and support for democracy (Altindag and Mocan, 2010; Wietzke and McLeod, 2012; World Bank, 2012). Is this causal?

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.[7] Finally, a psychology literature argues that under stress, including economic stress, people are more likely

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[5] For instance, one of the CCT studies evaluated the effects of an additional, unconditional business development grant, however, and finds significant increases in nonagricultural investment and earnings (Macours et al., 2012).

[6] 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 such heterogeneous treatment effects is fairly thin.

[7] Much of this evidence comes from historical and ethnographic studies of conflict (Goldstone, 2002; 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 (Cohen, 1965; Durkheim, 1893; Merton, 1938). This view is consistent with experimental economic evidence that people will generally 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 the action itself (e.g. Downs, 1957; Riker and Ordeshook, 1968).

to respond to bad stimuli with aggression (Berkowitz, 1993; Dollard et al., 1939). While these theories operate at different levels and articulate different mechanisms, all predict an inverse relationship between poverty and social cohesion, aggression, and stability.

Unfortunately we have almost 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. 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 the economic and social returns to anti-poverty programs. In 2008 the government distributed cash transfers worth roughly a year's income to thousands of poor and underemployed youth aged 16 to 35. The program explicitly aimed to reduce poverty and promote social cohesion and stability. The grants were unconditional, with two special features. 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. For administrative convenience, moreover, youth had to apply in small groups. Funds were distributed as a lump sum, which the group distributed among members.

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 worked fewer than 10 hours a week and earned less than a dollar a day. We randomly assigned 265 groups to the intervention and pursue a panel of 2,675 at three points in time: baseline, two years post-intervention, and again after four years.

This intervention offers several 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. Finally, we collect a wide array of data on socio-political behavior in a volatile region.

In line with our model's predictions, treated youth invest most of the grant in training and business assets. After four years, they are 65% more likely to have a skilled trade and have much higher business capital stocks. They also earn high returns; real earnings are 49% and 41% greater than the control group after two and four years. Moreover, real earnings appear to continue to grow over the full four years.

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 existing studies of existing entrepreneurs, females earn high returns. The impact of treatment on females is actually greater than that on males, 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 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.

Despite such large economic gains, however, we see little effect on our measures of social externalities. We collect a broad range of measures of family and community integration, community and national collective action, interpersonal aggression, disputes with authority, and attitudes toward and participation in violent protests. Several of these measures are positive correlated with (non-experimental) changes in earnings, wealth and employment in our panel. These correlations appear to be misleading. The experimental treatment effects from the cash grant are small, close to zero, and not statistically significant. We see a temporary fall in male aggression, and a temporary rise in female aggression, but both effects disappear by the four-year mark and could simply be a short-run anomaly.

We draw a number of conclusions. First, our evidence strengthens the economic case for cash transfers to the young, poor, and unemployed—including and perhaps especially women. This finding is important because, while most of the evidence on poverty relief is optimistic about women's ability to invest development aid in their children and other social spending (Duflo, 2012), the experimental literature has started to conclude that women do not invest development aid in new enterprises and higher lifetime incomes (and with it the ability to invest in children long term). We present a very different picture. 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 highest female impacts take four years to emerge, however, we believe our findings may also be a product of our unusually long panel.

Second, economic returns are high in spite of the intervention's emphasis on vocational training. The evidence on job training programs in the US, Europe and Latin America is famously pessimistic (e.g. Card et al., 2009), but there has never been a study of job training in a low-income country such as Uganda. A simple calibration model suggests the gains from this program arose largely from the availability of physical alongside human capital, which may account for the difference from other training interventions.

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. If so, the case for cash transfer-based aid programs will need to be made on the economic returns alone.

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 disbursement influenced these high levels of investment. We discuss related work in Uganda, however, that indicates monitoring and accountability by grant givers leads to little change in investment patterns (Blattman et al., 2013).

Overall, the results support a larger role for public financing for poor entrepreneurs and employment creation, and suggest that unconditional and externally unsupervised cash grants (which are significantly cheaper to implement than conditional or supervised transfers) can be an instrument of both poverty alleviation and growth, though unfortunately not social stability.

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 of the country. The north, home to a third of the population, lagged behind; beginning the 1980s, the north was plagued by insecurity. In the north-central region, an insurgency lasted from 1987 to 2006. Conflicts in south Sudan and Democratic Republic of Congo 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 a cash transfer program, the Northern Uganda Social Action Fund, or NUSAF (Government of Uganda, 2007). Starting in 2003, communities and groups could apply for government transfers for infrastructure construction or income support and livestock for the ultra-poor. Increasing the number, size and productivity of informal enterprises was also a priority. To do so, in 2006 the government announced a new NUSAF component: the Youth Opportunities Program (YOP), to raise youth incomes (and promote stability) through self-employment in a trade.

2.1 Intervention and participants

The YOP intervention provided cash grants to groups of 10 to 40 youth, with an average group size of 22. The average transfer was $7,497 per group or $382 per member at 2008 exchange rates ($763 at PPP rates).[8] The grant is roughly equal to baseline annual income. Per capita grants varied, however, with 80% of grants between $200 and $600. Variation is driven by differences in group size and requested amounts (see Appendix A1). The goal of the grant was to support skills training, tools and materials in a vocation of the youth's choosing, and enable them to practice their trade individually.

To be eligible, a group of youth had to submit a proposal to the central government via their local government. The proposal specified the amount requested, member names, a management committee of five members, the skills they proposed to train in, and a budget for how the transfer would be spent. "Facilitators"—usually a local government employee or civil society member—helped groups prepare proposals. Groups selected their own training organizations, typically a local artisan or a small local institute, of which there are many hundreds of varying formality and quality. In practice most groups proposed just one or two trades for their members.

If a group was selected, the central government made a lump transfer to a bank account in the names of the management committee. Group members were responsible for disbursement, and accountable only to one another. There was no central monitoring or enforcement after transfer.

[8] Throughout the paper we use a 2008 exchange rate of 1,721 (862 if quoting PPP prices) and state all Ugandan shilling and dollar amounts in real 2008 values.

As this is one of the largest, most well known aid programs in the country, in place for several years, applicants were probably aware of this absence of official accountability.

One reason the government had youth apply in groups is that it was easier to register, verify, and disburse to a few hundred groups rather than thousands of individuals. Another is that, in the absence of monitoring, group decision-making could increase compliance with the proposed budget. For instance, the group sometimes made lump sum payments to trainers and tool purchase on behalf of all members. We return to the theoretical effects of the group design below.

As this was a large, well-publicized and prized intervention, thousands of groups applied. Several hundred were funded across the north before commencing this study (all outside our current sample of communities). In 2007 the government determined that it had funds remaining for 265 groups in 13 of the 18 NUSAF districts. It asked the districts to nominate two to three times as many groups as there was funding. The central government audited nominated applications for completeness and to verify the group's existence. We observe a pool of 535 groups after they have been screened and selected from the population of youth and proposals.[9] See Figure 1 for a map of Uganda and groups per parish (an administrative unit that typically includes about a dozen villages).

Of the 535 groups, half existed prior to NUSAF, as sports or religious or community youth clubs. The main livelihood is agriculture. 35% of group members are female. Just 8% are engaged in a vocation at baseline (our "existing entrepreneurs") and 21% are engaged in either a vocation or a small business business, such as running a kiosk. As described above, applicants are underemployed and poor—incomes average roughly a dollar a day, a quarter reported no income in the past month, and a quarter did not finish primary school (full baseline statistics in Appendix A2). Yet however poor, applicants were above the average wealth and education level in the north.[10]

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[9] This selection occurred in a number of ways. First, the government asked that 22 groups of underserved populations (e.g. Muslims, orphans) be funded and excluded from the experiment. 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. Third, youth self-selected, and so applicants may be more motivated than average and have more entrepreneurial aptitude. Of course, since YOP was the only available transfer to young adults, even those with an affinity for other work would have incentives to apply.

[10] 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

2.2 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 by district. Despite the scale of the intervention, we judge spillovers to be 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 (Appendix A2).

We define treatment compliance narrowly: all individuals in the group are coded as compliers (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.

Given that non-compliance is small and unsystematic, our preferred ATE estimator is the complier average causal effect (CACE) estimate, which uses assignment to treatment, A_{ij}, as an instrument for being treated, T_{ij}. We have data at baseline ($t = 0$), and the 2- and 4-year endline ($t = 1,2$) for each individual i in group j and district (stratum) d. We estimate the 2-year ATE (θ_1) and the 4-year ATE ($\theta_1 + \theta_2$) jointly, as follows:

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

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

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 ν_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.

(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.

3 Conceptual framework: Economic impacts of cash transfers

For cash transfers to spur investment and earnings, there must be market imperfections. In well-functioning markets, entrepreneurs will 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 the interest rate. Rather, they will consume some and save the rest. A windfall of in-kind capital forces suboptimal investment. Earnings will rise temporarily until the entrepreneur divests.

Markets seldom function so smoothly, especially credit markets. In Uganda, at baseline, few formal lenders had a presence in the region. Village savings and loan groups were common, but loan terms—even today—seldom extend beyond two months with annual interest rates of 200%. Just 11% of the baseline sample had saved funds, $22 at the median. A third had outstanding loans, less than $6 at the median, mainly from friends and family. Less than 10% borrowed from an institution, with the median loan just $17.

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

Setup—To structure our thinking we develop a two-period model of occupational choice in imperfect markets: no borrowing and production non-convexities. 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, $K_0 \geq 0$.[11]

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. "Labor-

[11] The model could be considered a two-period version of the one-period investment model in de Mel et al. , or a "grants" version of the two-period microcredit model in Banerjee et al. (2013). The model was developed jointly with Julian Jamison and is shared by a related study of poverty alleviation in Uganda (Blattman et al., 2013).

ers" solve U s.t. $c_1 + s = y + w$, and $c_2 = y + s$. "Budding entrepreneurs" solve U s.t. $c_1 + s + F + K = y + w$, 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 2 to 4 illustrate a stylized solution. Figure 2 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 2 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 3 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 4 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 go into current consumption and savings. Overall, we

should expect to see some relationship between grant size and returns (especially to the extent that 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. Thus the impacts of cash transfers will decrease in risk aversion.[12]

3.2 Possible effects of group disbursement

Groups could play three possible roles. The first is negative: we may worry that group disbursement could have adverse effects if leaders can capture the 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 on behalf of members. Or individuals may feel peer pressure to invest rather than consume. In our model, this would tend to increase and reduce the variability of impacts in δ, increasing the likelihood they pay F to become entrepreneurs. It is not clear, however, to what extent long-term earnings are impacted by this initial commitment device. Over time, low-patience individuals will be less likely to reinvest earnings and more likely to divest or let assets depreciate. Eventually they should resemble the low-patience existing entrepreneurs in Figure 4—entrepreneurs with low K and high Period 1 income. 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 right framework for thinking about intervention impacts.[13] 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 direct-

[12] See de Mel et al. (2008) for an analogous one-period model that illustrates this point.

[13] 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.

ly 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, as noted before, returns to skills are thought to be lower than that of capital. Yet groups are expected to propose budgets that dedicate roughly a quarter to half of the funds to training. Second, it is not clear the standard 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. We might be worried that the program's encouragement of vocational training especially harms women. 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 2 to 4, 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 official 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 and March 2008. Enumerators were able to locate 522 of the 535 groups.[14] They mobilized group members—typically about 95% were available—to complete a group survey that col-

[14] 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 also did not know the treatment status of the groups they were mobilizing.

lected 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 track and interview all 5 members of the 522 groups found at baseline, plus the unfound groups. At least one (and often several) attempts were made to find each individual. We then selected a random sample of migrants and other unfound individuals for intensive tracking, often in another district. The effective response rate is 91% after two years and 84% after four.[15] Though 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 statistically significant). Attrition is slightly higher among males, but otherwise relatively uncorrelated with baseline data, suggesting that it is relatively unsystematic. Appendix A4 describes the levels and correlates of attrition.

4.1 Economic outcomes

Table 1 lists summary statistics for the main outcomes. To measure investment, respondents self-report the *Hours of training received* between baseline and the 2-year endline (2Y) and their estimate of the value of their *Stock of business capital* (raw materials, tools and machines) in real 2008 Ugandan Shillings (UGX), deflated by the national consumer price index. Unfortunately, we do not have a more precise distribution of the group transfer, as groups disbursed funds among members in diverse ways and seldom keep records. Our measures represent our best (albeit incomplete) investment estimates.

[15] 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 Appendix A1 for a study timeline and A4 for analysis of attrition rates and patterns.

Our main outcome measure is monthly *Net earnings,* in real 2008 UGX. We ask respondents to estimate their gross then net earnings for each business activity, plus wages or earnings from other activities in the previous four weeks by activity, and we take the sum of net earnings over all activities.

We complement earnings with three 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 an abbreviated measure of *Short-term expenditures* in UGX based on 58 forms of short-term non-durable expenditure. Finally, we measure *Subjective well-being* by asking respondents to place themselves (relative to the community) on a 9-step ladder of wealth.

To measure employment, at each survey respondents report total *Hours of employment* in the past four weeks, excluding household work and chores but including cash-earning and subsistence labor. We also look at hours of employment in these three subcategories or by activity. Appendix C1 provides more measurement details and C2 describes secondary economic outcomes.

4.2 Social outcomes

Finally, to measure psychological and social impacts, we construct a number of additive indices (standardized as z-scores) based on families of related survey questions.[16] Each index has zero mean and unit standard deviation, and Table 1 lists summary statistics for the components of each family index. These component variables were largely drawn from 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).

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. Low levels of integration at this kin level could reflect the sociological concept of anomie discussed in the introduction. It could have more direct economic origins as well. In Africa, young adults who cannot

[16] In addition to being useful summary measures of a large number of variables, these family aggregates guard against rejecting true null hypothesis when testing multiple outcomes (Duflo, Glennerster et al. 2007).

contribute to the household or kin may find themselves dislocated from these networks. In principle, this dislocation could reduce constraints on anti-social behavior.[17]

Another aspect of social integration is captured by an *Index of community participation* based on 10 measures of association 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.

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.[18]

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). Finally, 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).[19]

4.3 Measurement error and average treatment effect estimation

The most important potential source of measurement error comes from self-reported outcome data. We will overestimate the ATE if treated individuals over-report well-being (for instance, if they believe surveyors come from the government) or if control individuals under-report out-

[17] Social groups act as a mutual insurance system, and the kin system in particular works as a form of mutual assistance among members of an extended family, traditionally from the older to the younger (Hoff and Sen, 2005). In such societies, the transition from "youth" to "adult" is a transition from disregard to social esteem and support, and is partly determined by one's ability to give rather than receive gifts and transfers.

[18] 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.

[19] We were asked not to collect extensive aggression and political data at the two-year survey by the survey donors, the Government and the World Bank, who were concerned that such questions would be misinterpreted as seeking political leverage out of NUSAF. We returned for the four-year survey with private funding to tackle these topics.

comes (for instance, in the hope it will increase their chance of future transfers). We have no reason to believe, however, that respondents systematically misreported all survey measures.

The second comes from extreme values. All our UGX-denominated outcomes have a long upper tail to which any measure of central tendency is sensitive. Outliers are excessively influential (and may or may not be errors). We take three steps to minimize this problem: first, we top-code all UGX-denominated variables at the 99th percentile; second, we examine treatment effects at the median and other quantiles; and third, we examine the ATE of a natural logarithmic term. Since some respondents report zero UGX, this requires us to take the log plus 100 UGX (about five cents). We will show the results are nearly identical to other non-linear transformations that are defined at zero, such as the inverse hyperbolic sine. We use the logarithm in this paper for its ease of interpretation but test sensitivity to its use (among other specification changes).

5 Economic impacts

5.1 Investment

The vast majority of treatment group members make the investments they proposed: most enroll in training, and it appears a majority of the transfer is spent on fees and durable assets. Table 2 displays 2- and 4-year ATEs (and their difference) for investments in training and assets, estimated using the pooled Equation 1. It also displays estimate ATEs by gender. For each ATE we display the control group mean and the percentage change represented by the ATE.[20]

Skills training—Between baseline and the 2-year endline, 74% 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 2). 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. Generally they train with a local artisan or a small training institute run by local artisans. Meanwhile, 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

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

treated, though the trainings tend to be much shorter. About 40% pay their own way, and the rest receive training from a church, government extension office, or non-governmental organization (NGO). Thus, even though controls were motivated enough to apply for the intervention, just 6% can afford the vocational training without a transfer. Appendix B has a detailed analysis of training levels, choices, institutes, and correlates among the treated and controls.

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. Between years two and four the control group begins to catch up, especially the males.

Figure 5 illustrates cumulative distribution functions (CDF) for the natural log of the stock of business capital, including goods and tools. From Figure 5a, 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 2, 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.

While large at both points in time, the ATE shrinks between the two- and four-year surveys. This mainly reflects catch-up by control males. In levels, treated males have a capital stock 157% greater than control males after two years and 41% greater after four. Treated females have a stock 108% greater than control females after 2 years and again 108% greater after 4. Looking at stock levels, 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.[21]

[21] 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.

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—specification of a proposal, auditing prior to disbursal, and group organization and control over funds—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. The treatment group was no more or less likely to have received other transfers and what they did receive (in logs) is not significantly different from the control group (Table 2).

5.2 Earnings and employment

Earnings—These skills and capital investments translate into large earnings gains after two and four years. To see this, Figure 5b displays CDFs of log real earnings by gender and treatment status, Figure 6 displays the levels and trends over time for real earnings in levels and logs, and Table 3 calculates ATEs for all, including difference-in-difference estimates for earnings.[22]

As with capital, 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 5b). 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 3). 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.

[22] 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 6 and the baseline differences-in differences estimate with some caution.

We see these large earnings gains in spite of the potential program weaknesses: encouragement to skill investments in a narrow range of trades, and the large number of people in one community trained in one skill. Moreover, female-dominated trades such as tailoring provide returns comparable to other trades. 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 relatively high earnings whether a man or a woman is practicing (see Appendix C3). Moreover, a simple calibration exercise suggests that the bulk of the treatment effect is due to investments in physical capital (Appendix C4).

Perhaps the most striking result, however, is the difference in trend between male and female controls: while control males keep pace with treated ones, real earnings in the female control group are nearly stagnant over four years (Figure 6a). Average real earnings among treated females grow somewhat in the first two years, but the increase is greatest between the two- and four- year surveys. The control group shows almost no change. In contrast, treated males see sustained earnings growth, with the biggest increase in the first two years. The male control group keeps pace with the treatment group and may even be slowly closing the gap. The difference-in-difference ATE estimates in Table 3 estimate the difference in the slope of treatment and control lines in Figure 6, by gender. They show a steeper slope between baseline and the 2-year endline for both men and women, but higher and only significant for males. The negative coefficient for males between two and four years indicates catch up, though it is not significant.

We see the same pattern among males looking at log real earnings in Figure 6b. The coefficients on the difference-in-difference estimates now estimate the difference in treatment and control growth rates. The coefficient for males is positive between baseline and the two-year endline, but negative and significant between two and four years, arguing for some catch up. This is one difference between the log and level earnings analysis. A second is that female control earnings no longer look as 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: the difference-in-difference estimate for female log earnings is positive between two and four years. While not statistically significant, it is nearly a log point greater than the male coefficient, and this difference is significant.

Durable wealth and short-term consumption—Table 4 calculates ATEs on an index of wealth and short term expenditures, where the results echo our earnings results: after 4 years, the treatment group's wealth index is 0.2 standard deviations 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.[23]

Employment and occupational choice—The treatment group increases their labor supply in response to the increase in capital, especially women. Hours of employment in market and subsistence activities increase 21 hours per month after two years and 25 hours after four, in both cases a 17% increase over the control group (Table 4). The increase is entirely in market activities, with no change in subsistence production (Appendix C2). 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 presumably comes with increased earnings.

This employment increase also reflects a shift in occupational choice towards skilled and market work. If we define skill- and capital-intensive work broadly, to include all professional services, trades, and petty business, 34% of the control group engages in this work. Treatment doubles this proportion. Among men, the increase in employment is entirely in cash-earning activities without any fall in subsistence activity. Among women, cash-earning hours increase 50% after two years and 60% after four, relative to controls. Domestic work falls 18% after two years and 6% after four (Appendix C2).

Subjective well-being—Finally, consistent with these income and wealth gains, treated subjects perceive themselves as doing economically better than fellow community members. Asked where they stand in terms of wealth relative to other community members, on a ladder from 1 to 9, the control group responds 2.7 after two years and 3.3 after four (an increase of 21%). Male and female levels are similar. We see treatment effects of 0.37 and 0.53 after two and four years, which correspond to 14% and 16% increases relative to the control group.

[23] Appendix C2 illustrates additional economic outcomes, including savings, loans outstanding, and credit access, and generally finds significant increases.

5.3 Return on investment

The average treatment effect on net earnings in Table 3 represents a 40% annual return on the average transfer per group member.[24] 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.[25]

Are these returns "high"? One answer depends on the real interest rate used. In 2008-09, Uganda's real prime lending rate to banks was 5%. Short-term microfinance rates, on the other hand, are roughly 200% per annum. While detailed data are not available, real commercial lending rates of 10 to 20% appear to be common among larger firms. Thus the average returns to capital above also approach the "high" returns of 40 to 60% recorded for existing microenterprises in Sri Lanka, Mexico, and Ghana (de Mel et al., 2008; McKenzie and Woodruff, 2008; Udry and Anagol, 2006). The fact that the Ugandan vocational returns are on the low end of this range may reflect the particular sample and context, the emphasis on vocational training, or the clustering of skills training in each town.

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

Variation in cash grant amounts suggest that, relative to the discount rates and savings/lending rates faced by program applicants, the marginal returns to capital are so not high that the larger grants are fully invested. To see this, in Table 5 we look at the effect of grant size on four outcomes—the logs of real capital stock, earnings, short-term expenditures, and savings. Pooling both endlines, we regress each logged 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 21% in capital stocks, 17% in earn-

[24] 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%.

[25] 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 educational attainment at baseline. 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. Treatment effects are described in Appendix C2. The 2-year ATE for males and females is UGX 18,110 and the 4-year ATE is UGX 27,835.

ings, 5% in short-tem expenditures, and 37% in savings. The standard errors are wide, however, and these estimates are not statistically significant.

The high elasticity of savings could reflect the fact that the returns to investment, however high given the average grant size, have limits. Alternatively, the results could reflect risk aversion: precautionary savings or for investment in alternative future businesses should this one fail.

5.5 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 (Appendix C5). Generally the size and the significance of results do not change. An exception is the change in top-coding, where eliminating top-coding increases the ATE and reducing the threshold to 95% decreases the ATE, largely because the largest values of investment stocks and income are in the treatment group. Hence the estimates reported in Tables 2 and 3 are the more conservative ones.

The same qualitative conclusions and statistical significance also hold for treatment effects at the median and other major quantiles (an alternative approach to central tendency in the presence of extreme values). The median treatment effect on net earnings is UGX 8,200, approximately half the ATE (Appendix C6).

We also bound the treatment effects for bias due to attrition. While attrition is relatively unsystematic and uncorrelated with treatment, it is nonetheless possible for the ATE to be biased upwards if unfound treatment individuals are possess lower potential returns than unfound controls. To bound the ATE, we can 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 Appendix C. 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 standard deviations better outcomes). Manski bounds include zero, however.

6 Impact heterogeneity

6.1 Evidence on market imperfections

We can explore the source of high returns, and test our theory, through the analysis of treatment heterogeneity. To the extent credit constraints restrict our sample, we should 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 potential 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. Impact heterogeneity is not identified, however, and can only provide suggestive support for (or against) the model.[26] This experiment has four advantages in testing these predictions: ex-ante predictions generated by the existing literature, a large sample size, a long horizon, and rich baseline data.

Table 6 examines impact heterogeneity on the log real values of the asset stock and earnings. We look at heterogeneity along five main dimensions: (1) an indicator for whether they had an *Existing vocation* at baseline; (2) a *Working capital z-score* summarizing initial asset wealth, savings and lending, and perceived credit access; (3) an *Ability 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.[27] We examine each form of heterogeneity individually and altogether. (Individually the test may be higher powered, while altogether is lower powered but less biased.)

First, patterns among existing entrepreneurs are consistent with our predictions. Focusing on columns 5 and 12, those with a vocation at baseline have a larger capital stock (0.89 log points)

[26] 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 through experimental impact heterogeneity: among the treated, the returns to capital are decreasing in initial household assets and increasing in a measure of cognitive ability (a digit span test) though not in education or risk aversion.

[27] These indices are weighted averages of survey questions, described in Appendix A3. All were measured at baseline, with the exception of patience questions, which were asked at the two-year endline. These exhibit no treatment effects, however, and so we treat the endline patience measure as time-invariant. Looking at the control group, these indices of working capital, ability, patience and risk all have the expected relationship with endline economic success (Appendix A3).

and real earnings (0.71 logs). Treatment has less impact on existing entrepreneurs: the coefficient on the interaction is negative and (in the case of log earnings) large, significant and nearly as large as the ATE (-0.83 logs). These patterns are most pronounced among males (columns 6-7 and 13-14), in large part because the number of females with a vocation at baseline is very small.

Second, investment and earnings fit the pattern we expect from working capital: those with a one s.d. greater initial level have higher capital stocks (0.44 logs) and earnings (0.45 logs) at endline. Again, treatment has the most impact on the most constrained: the coefficient on the interaction is negative for capital (-0.48 logs) and earnings (-0.19 logs), though this interaction is generally significant when examined individually (columns 1 and 8) and loses significance in the full regression (columns 5 and 12). Male-female patterns are similar.

Third, the ability results only somewhat consistent with the model's predictions. Alone, ability is positively related to capital and earnings in the full sample (columns 2 and 9), but higher ability individuals are less responsive to treatment—the opposite of what the model predicts. In the full regressions (columns 5 and 12) the coefficients shrink and lose significance. One interpretation is that our measure of ability (essentially, human capital) is a poor proxy for true entrepreneurial ability. Moreover, our measure may be correlated with physical capital, since schooling was fee-based for most of this sample. Unfortunately we do not have a broader array of cognitive and non-cognitive measures at baseline.

Fourth, the patterns of patience are consistent with our predictions, though not significant in the full regressions. A one s.d. increase in the patience index is associated with 0.26 logs greater capital stock and 0.24 logs higher earnings. The more patient also respond more to treatment: the interaction coefficient is 0.26 logs for capital stock and 0.32 logs for earnings, though these are not statistically significant.

Finally, patterns of risk aversion are also consistent with predictions, though also not significant in the full regressions. A one s.d. increase in risk aversion is associated with a lower response to treatment (the interaction coefficient is -0.375 logs for capital stock and -.216 for earnings, significant at the 1% level when interacted alone, but the coefficients decrease and become insignificant with a full set of interactions). Heterogeneity analysis within subgroups—those with and without existing vocations, or those with high and low patience, for instance—the results are qualitatively the same (not shown).

6.2 The effect of groups

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 7 we regress the log of capital stock and earnings on treatment and baseline group characteristics: quality of the *Group dynamic* (a z-score based on group members' assessments of the group's level of trust, cooperation, loyalty, inclusiveness, and equity); an indicator for whether they *Existed prior to YOP*; Group size; *Proportion female*; and *Heterogeneity* (a z-score of the standard deviation of group member education, wealth, and age). 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 (pooling endlines). 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.

6.3 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 subsequent 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 Social impacts

Overall, we see limited and weak evidence of a positive social impact on males after two years, and none whatsoever after four years. Table 8 presents ATEs for seven outcome families. The point estimates are typically close to zero, and standard errors on these z-scores are typically small (roughly 0.05 s.d.) suggesting we can confidently rule out medium to large changes.

First, treatment is associated with little change in kin integration: a 0.012 s.d. decline after two years and a 0.048 s.d. increase after four, with neither change statistically significant. Average treatment effects on the individual components of each outcome are displayed in Appendix D2. Looking at these component measures, there is no significant change in family support or marriage, but a slight (0.05 s.d.) yet significant reduction in family disputes and a slight (0.07 s.d.) 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 s.d. increase after two years (significant at the 10% level) and an insignificant 0.01 s.d. 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 com-

munity meetings. None are apparent after four years.[28] 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 s.d.) 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 to moderate (Table 1). There is a substantial amount of variation, however, including roughly 5% of the sample who report frequent and high levels of aggression across a number of measures.

Looking at males and females together, aggression declines 0.073 s.d. after two years and rises 0.05 s.d. after four, but neither effect is significant. When we disaggregate by gender, however, we see an unusual pattern at two years: a 0.20 s.d. decline among males, and a 0.18 s.d. 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 (Appendix D2). 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 merely temporary. A second possibility is that the two-year finding is anomalous, especially because it was only observed in subgroup analysis, and because few of the other social measures showed any change at two years, especially of that 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. Observers described voting day itself as free, fair and peaceful, but the opposition complained of high levels of government intimidation during the campaign (European Union, 2011). District and national elections were held in subse-

[28] Omitted from this index is community group membership, which does increase significantly with treatment, most likely mechanically, due to active membership in the NUSAF group.

quent weeks. Electoral participation tends to be high in Uganda, with 91% of the sample 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 assemble these measures of participation into an index, but see little impact of treatment: overall, the treated have an index 0.04 s.d. higher than controls. This impact increases slightly to 0.10 s.d. among males, significant at the 10% level (Table 8). Looking at the individual components, this weak effect seems to be driven mainly by a slightly higher propensity to have voted in the district election.

While we see little change in levels of participation. In a separate paper, we explore the effect of the intervention 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 in north Africa, the main opposition leader called for nationwide protests, partly against steeply rising food and energy prices, but in general as an act of protest against the government and an election perceived as unfair. Marches were held in the capital and many towns, including several in the north. Many of these marches 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, and just 2-3% of our (largely non-urban) 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 (Table 1). We assemble these protest attitudes and participation into an index, and the treated have a mere 0.008 s.d. 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. 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.

7.1 Comparison to non-experimental income changes

In contrast to these experimental estimates, non-experimental changes in wealth, income, and employment are associated with increased social integration and community and electoral partic-

ipation. Table 9 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. 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.

8 Discussion and conclusions

The two major theoretical questions addressed by this experiment are first, whether, under what circumstances, and for whom cash windfalls and other economic shocks will have high returns; and second, whether any corresponding employment and income increases will result in greater social cohesion and stability, as is commonly assumed.

We find the least support for this latter social theory. The absence of individual-level impacts on integration and aggression do not exclude the possibility of social externalities to poverty reduction. Some of the sociological and political theories, for instance, could be interpreted as unresponsive to changes on the margin, but rather dependent on larger structural and societal changes. Nonetheless, the absence of change on the individual margin runs counter to many expectations, including opportunity cost theories of conflict. It suggests that the social externalities and non-pecuniary returns from large anti-poverty programs are small, and so the case for these public investments should be made on the economic returns alone.

Our results suggest this economic case is remarkably strong, however. The grants are typically invested and yield high returns. High returns have been noted elsewhere, albeit mainly among

existing entrepreneurs and mostly males. High returns among the young, poor and unemployed, male and female alike, are consistent with economic theory but nonetheless unexpected by many.

This is important evidence from both an academic and a policy perspective. Despite their increasing prevalence, state and aid organizations treat cash skeptically: 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". We believe this bias against cash is unwarranted.

Another notable finding is the high returns to cash even among poor, unemployed and relatively uneducated women. How can we reconcile our female results with the emerging literature that finds returns among female entrepreneurs to be low? Most of all, the context and population in this study are different. These women are poorer, thus perhaps more constrained, and hence cash grants would be expected to have a larger impact. In this sense these women are not all all unuusal. The application and selection process, however, may also have selected a particularly promising group, although (one might argue) no more so than the process that selects women into being existing entrepreneurs. Finally, horizon may be 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. 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.

Finally, our results present an optimistic picture of vocational training in low-income countries. Looking across the dozens of evaluations in high and middle-income countries, several studies conclude that job-training seldom passes a cost-benefit test (Heckman et al. 1999; Betcherman et al. 2007; Card et al. 2009). There has never been a study in a low-income country, however, especially Africa. 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 high returns we see in Uganda could be due to the relative scarcity of technical skills and the availability of capital for business start-up. Future research should pursue these questions, for technical and vocational training represented almost $3 billion in development assistance from 1990 to 2005, or 7.5% of all education aid (World Bank 2010).

We see the priority for future research as twofold. First is the question whether high investment levels and returns were a product of the particular 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 forthcoming).

A second important avenue 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 and more prevalent. Besides a growing number of government and World Bank Social Action Funds, there are examples such as GiveDirectly, an international NGO founded by economists, which distributes unconditional cash via mobile phone to impoverished households. It has been rated as the #2 "top charity" in the world by the online charity evaluator GiveWell, as one of 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). This paper's results suggest this extreme 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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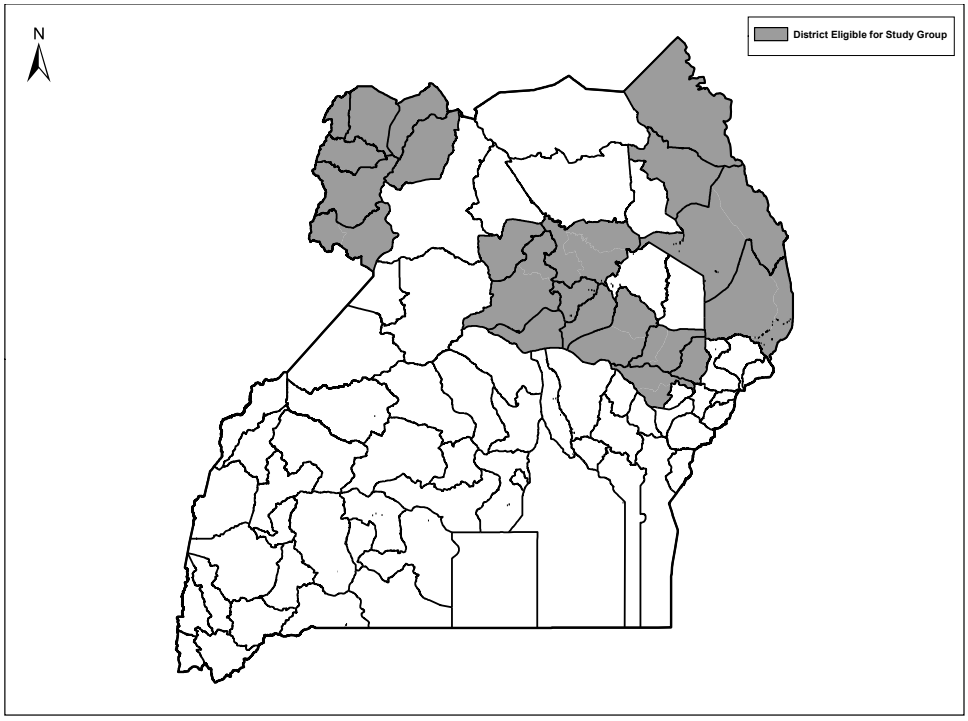
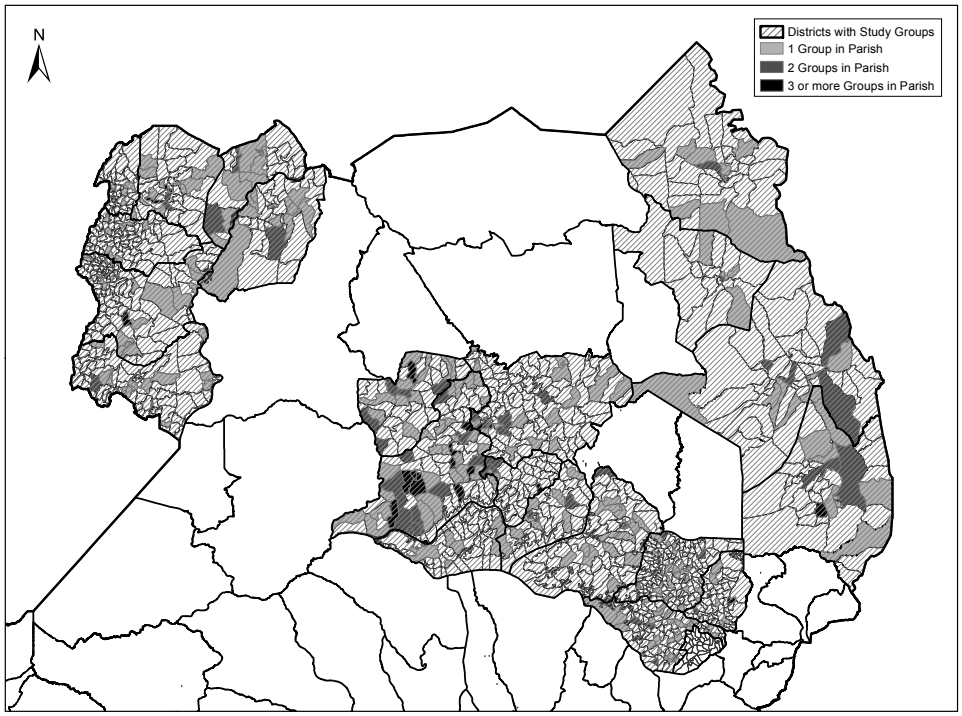
Figure 1: Location of study communities

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.

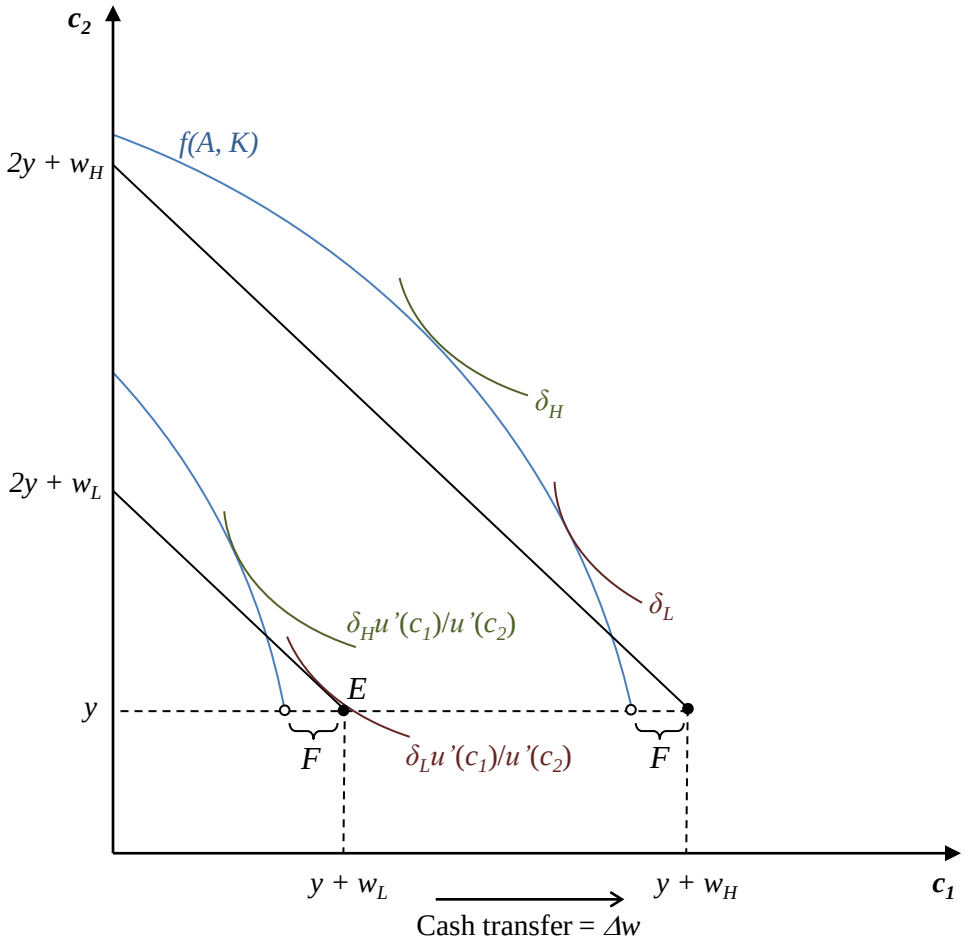


Figure 2: Impact of cash transfers on occupational choice (No existing entrepreneurs)

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.

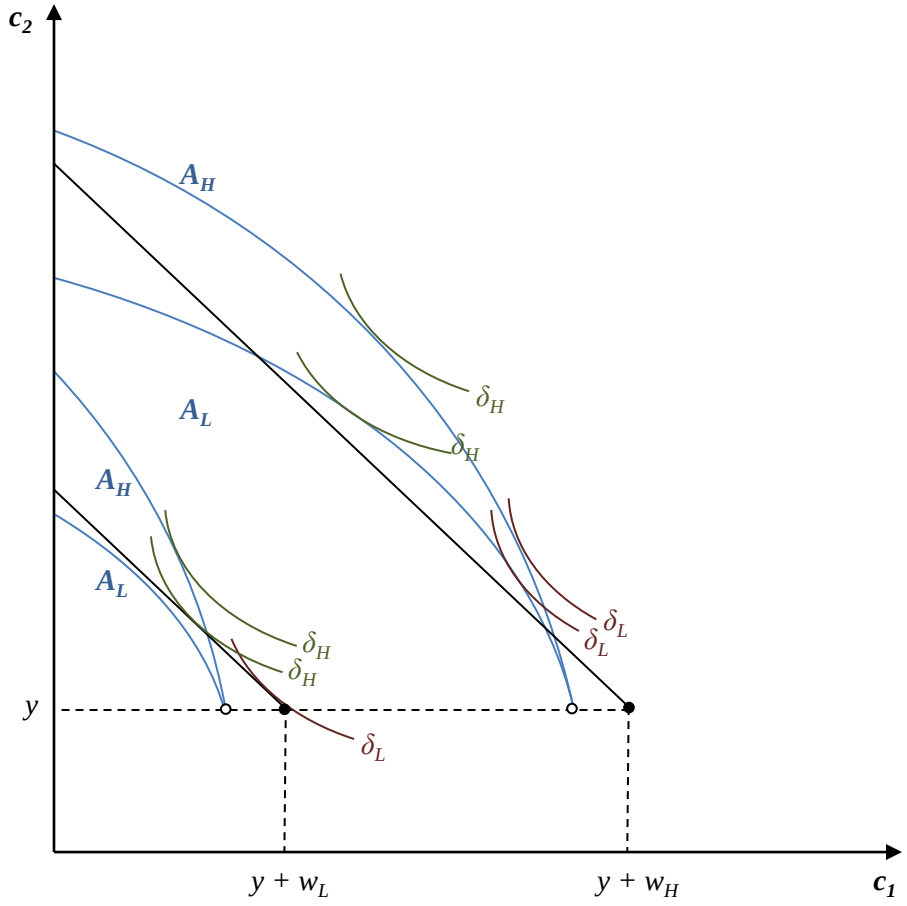


Figure 3: High versus low ability individuals (No existing entrepreneurs)

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.

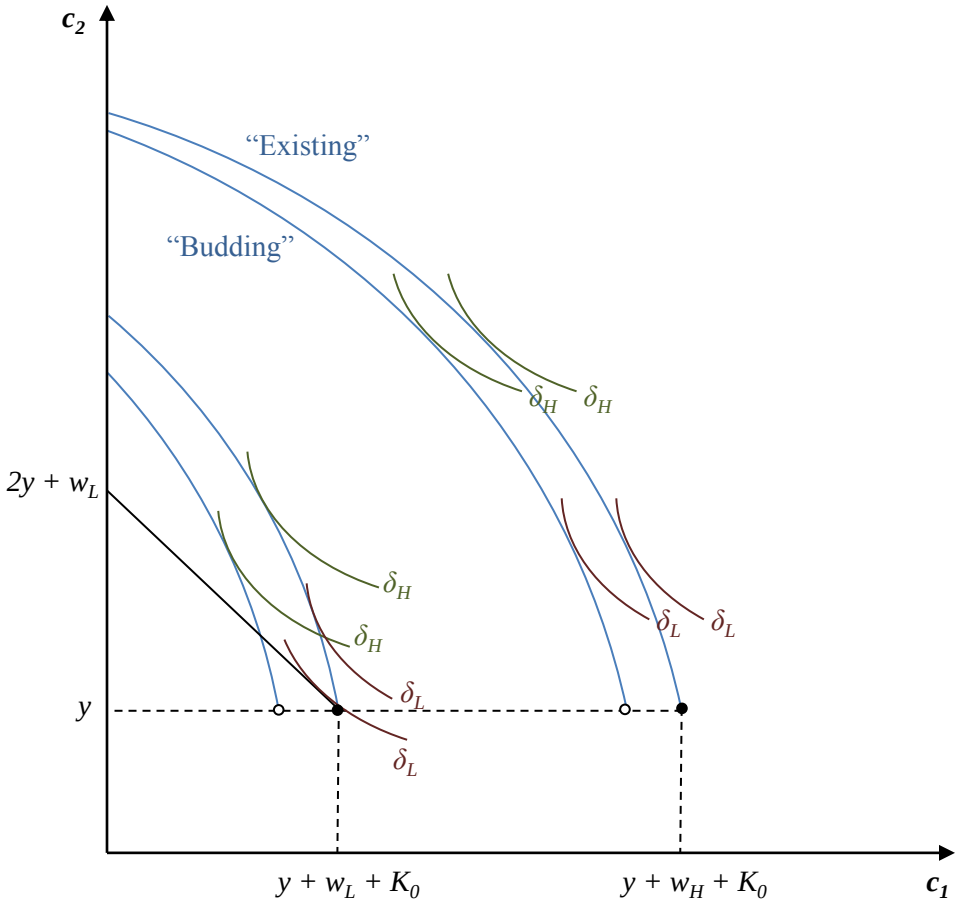


Figure 4: Existing versus budding entrepreneurs, with equal levels of starting capital

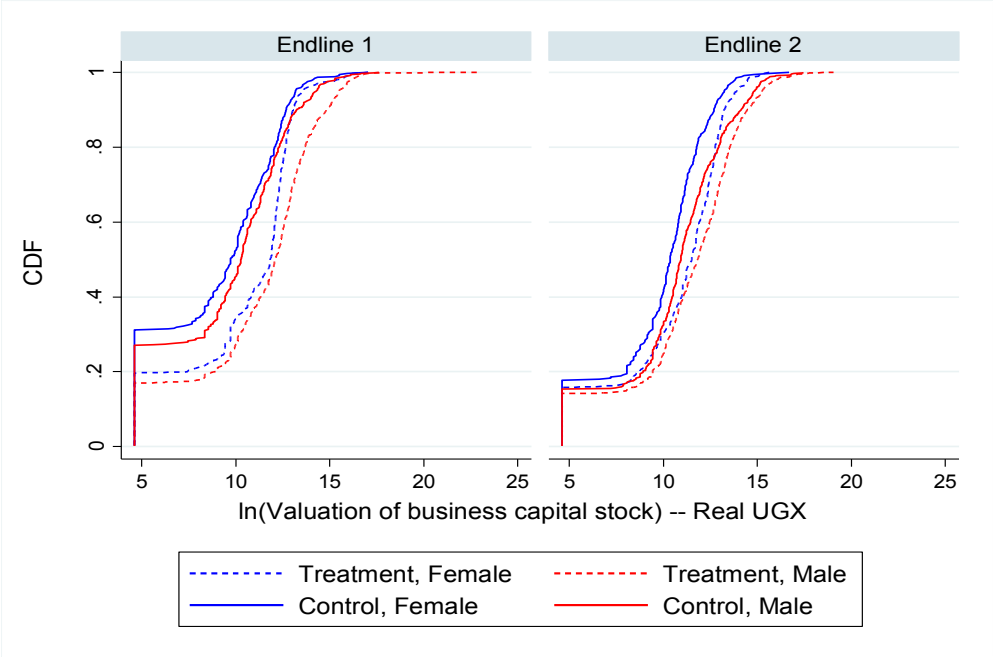
For illustrative simplicity we assume first period entrepreneur income is equal to labor income: $f(A,K_0) = y$.

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 5: Cumulative distribution functions of investment and earnings

a. Stock of business assets



b. Real net earnings

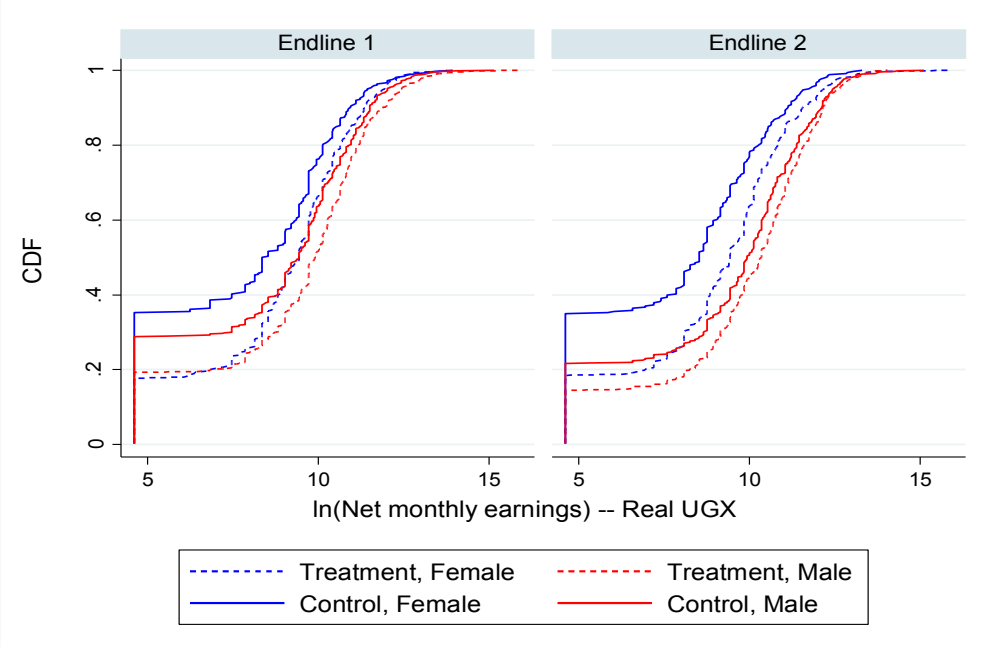
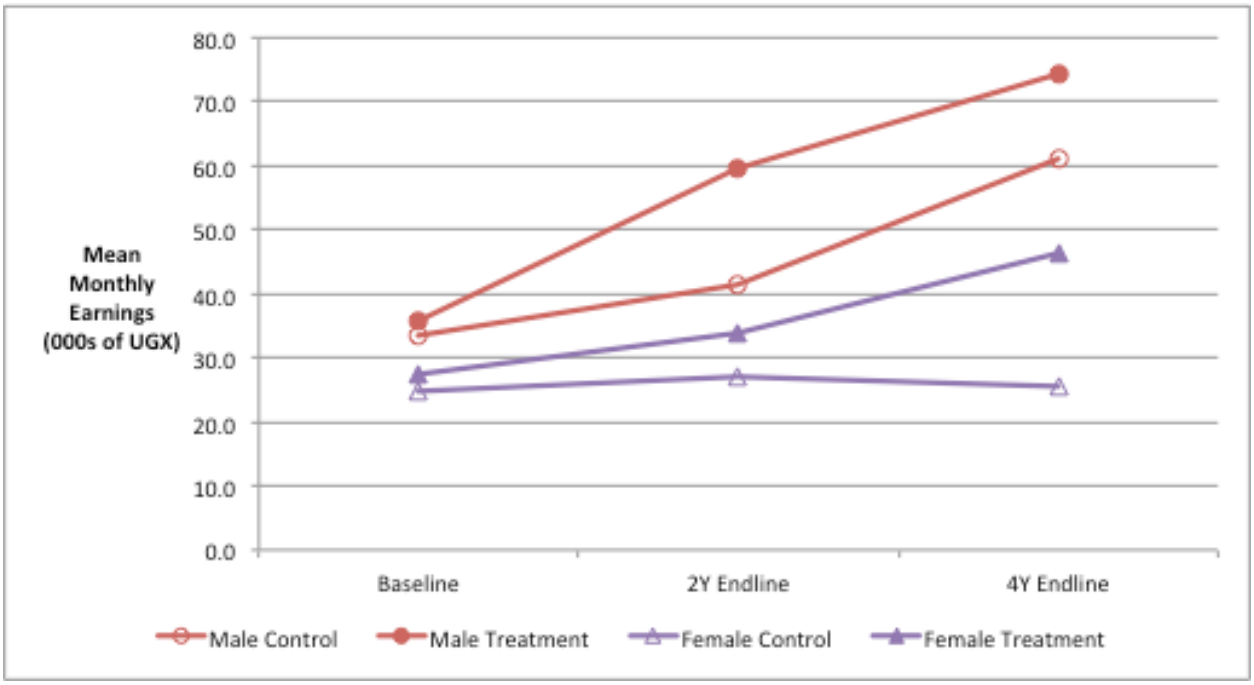


Figure 6: Real earnings trend

a. Levels



b. Logs

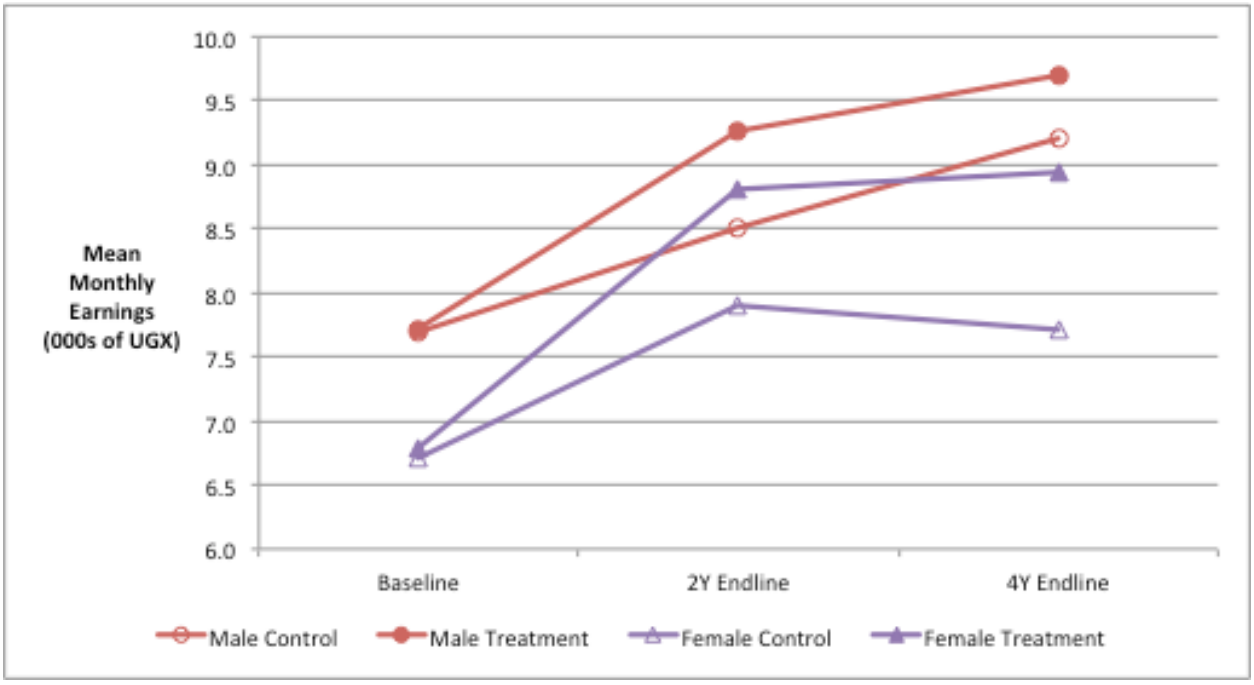


Table 1: Key outcomes and summary statistics

	(1)	(2)	(3)	(4)
	2 Year Endline		**4 Year Endline**	
	Control Mean	**Treatment Mean**	**Control Mean**	**Treatment Mean**
Investments in vocational skills and capital				
Hours of training received	48.94	378.32	--	--
Real value of business asset stock	299.39	737.14	392.45	607.82
Income, poverty and employment				
Real net cash earnings	36.10	51.40	47.90	65.43
Monthly employment hours, including chores	120.91	139.29	147.05	170.85
Index of wealth	-0.05	0.04	0.16	0.33
Short term expenditures	--	--	51.67	55.72
Current position on wealth ladder: 1 to 9	2.73	3.05	3.29	3.73
Social integration				
Family caring index	2.68	2.65	2.75	2.71
Family harmony index	3.71	3.73	3.89	3.92
Lives with a partner dummy	0.73	0.77	0.79	0.82
Elder relations index	2.57	2.56	2.54	2.50
Community participation and engagemennt				
Number of group memberships	3.31	3.35	1.62	1.73
Attends community meetings	0.67	0.70	--	--
Community mobilizer	0.51	0.60	0.60	0.62
Community leader	0.40	0.41	--	--
Speaks out at community meetings	0.61	0.66	--	--
Frequency that community meeting in past year	--	--	1.12	1.07
Are you a member of the LC1 committee?	--	--	0.13	0.15
Are you currently a member of any committee that makes decisions that affect a large portion of the community?	--	--	0.28	0.27
If nominated to become an LC1 by your community and you had the time, would you want to hold such a position?	--	--	0.69	0.67
Got together with others to raise an issue	--	--	0.55	0.58
Contributions to public goods				
In the past 12 months did you contribute to road maintenance in your community?	--	--	0.50	0.50
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
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
In the past 12 months did you contribute funds to school fundraising?	--	--	0.49	0.49
In the past 12 months did you contribute to construction, repair, or maintenance of community latrines?	--	--	0.36	0.37
In the past 12 months did you contribute to any other community fundraising or work?	--	--	0.45	0.46
In the past 12 months did you contribute to funeral expenses of someone outside of your family?	--	--	0.43	0.45
Aggressive and hostile behaviors				
Are you quarrelsome: 0 to 3	0.30	0.29	0.34	0.32
Do you take things from other places without permission: 0 to 3	0.14	0.13	0.07	0.06
Do you curse or use abusive language: 0 to 3	0.12	0.12	0.08	0.07
Do you threaten to hurt others: 0 to 3	0.15	0.13	0.08	0.11
Disputes with neighbors: 0 to 3	0.20	0.20	0.09	0.14
Disputes with community leaders: 0 to 3	0.08	0.06	0.01	0.01
Disputes with police: 0 to 3	0.05	0.02	0.01	0.01
Involved in physical fights: 0 to 3	0.05	0.05	0.03	0.04
Aggressive and hostile behaviors (extended)				
You yelled at others when they have annoyed you.	--	--	0.66	0.75
You reacted angrily when provoked by others.	--	--	0.69	0.79
You got angry when frustrated.	--	--	1.34	1.23
You damaged things because you felt mad.	--	--	0.15	0.13
You become angry or mad when you don't get to do things.	--	--	1.20	1.05
You got angry when others threatened you.	--	--	1.40	1.26
You felt better after hitting or yelling at someone.	--	--	0.24	0.25

	2 Year Endline		4 Year Endline	
	Control Mean	Treatment Mean	Control Mean	Treatment Mean
You hit others to defend yourself.	--	--	0.39	0.43
You damage things for fun.	--	--	0.14	0.12
You used physical force to get others to do what you want.	--	--	0.18	0.12
You used force to obtain money or things from others.	--	--	0.09	0.10
You have gotten others to gang up on someone else.	--	--	0.10	0.09
You carried a weapon to use in a fight.	--	--	0.05	0.03
You yelled at others so they would do things for you.	--	--	0.28	0.32
Election Action				
Attended a voter education meeting	--	--	0.49	0.55
Got together with other to discuss who to vote for	--	--	0.56	0.55
Reported a campaign malpractise or incident	--	--	0.11	0.15
Successfully registered in 2011	--	--	0.98	0.99
Voted in the presidential election	--	--	0.91	0.93
Voted in the LCV election	--	--	0.87	0.90
Index of partisan political action				
Attended an election rally	--	--	0.99	1.01
Participated in an political primary	--	--	0.95	1.00
Worked to get a candidate or party elected	--	--	0.97	1.04
Are you a member of any political party?	--	--	0.93	0.95
Protest Attitudes				
Protest attendance index (0-3)	--	--	0.02	0.03
In your mind, do you feel that these reasons justified an act of protest?	--	--	0.47	0.45
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
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
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
If there was a protest for similar reasons in your district now would you go?	--	--	0.09	0.09
If the protest turned violent, would you stay to participate in the violence, or would you leave?	--	--	0.07	0.09

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

Table 2: Average treatment effects on investments in vocational skills and capital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Hours of training received	**Real value of business asset stock**			**Log of real value of business asset stock**			**Difference in real value of business asset stock**			**Real value of other state/NGO transfers**
	2Y	**2Y**	**4Y**	**4Y - 2Y**	**2Y**	**4Y**	**4Y - 2Y**	**Change from E1 to E2**			**2Y**
ATE (All)	389.163	470.950	200.641	-270.309	1.842	1.033	-0.809	-0.814			0.149
Std. Err.	[23.798]***	[91.784]***	[74.421]***	[117.823]**	[0.197]***	[.181]***	[0.264]***	[0.275]***			[0.135]
Control mean	48.94	299.4	392.4	93	9.236	10.17	0.934	0.910			5.564
ATE as % of mean	795%	157%	51%		519%	176%		-57%			15%
Male ATE	384.296	601.276	217.919	-383.357	1.920	0.889	-1.031	-0.913			0.182
Std. Err.	[24.325]***	[125.665]***	[104.847]**	[163.655]**	[0.241]***	[.223]***	[0.327]***	[0.331]***			[0.160]
Control mean	40.72	360.7	532.3	171.6	9.396	10.50	1.104	1.058			5.588
ATE as % of mean	944%	167%	41%		563%	137%		-62%			18%
Female ATE	399.02	207.775	165.81	-41.965	1.685	1.31	-0.375	-0.625			0.083
Std. Err.	[44.226]***	[102.086]**	[65.275]**	[116.029]	[.342]***	[.291]***	[.443]	[.464]			[.231]
Control mean	62.38	192.6	153.2	-39.4	8.937	9.610	0.673	0.691			5.523
ATE as % of mean	640%	108%	108%		409%	255%		-52%			6%
Female - Male ATE	14.724	-393.501	-52.109	341.392	-0.235	0.421	0.656	0.288			-0.099
Std. Err.	[46.473]	[160.109]**	[118.927]	[195.623]*	[0.418]	[.358]	[0.550]	[.559]			[0.273]
Observations	1997	1996	1865	3861	1996	1865	3861	1532			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 capital.
**** p<0.01, ** p<0.05, * p<0.1*

Table 3: Average treatment effects on income, poverty and employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Real net cash earnings			**Log of real net cash earnings**			**Difference in real net cash earnings**	**Difference in real net cash earnings**	**Log difference in real net cash earnings**	**Log difference in real net cash earnings**
	2Y	**4Y**	**4Y - 2Y**	**2Y**	**4Y**	**4Y - 2Y**	**Baseline to E1**	**E1 to E2**	**Baseline to E1**	**E1 to E2**
ATE (All)	17.785	19.878	2.093	0.916	0.846	-0.070	17.446	0.921	1.064	-0.245
Std. Err.	[4.790]***	[5.551]***	[7.143]	[0.151]***	[.154]***	[0.211]	[4.701]***	[6.932]	[0.185]***	[0.202]
Control mean	36.10	47.90	11.8	8.285	8.653	0.368	6.095	17.57	0.976	0.689
ATE as % of mean	49%	41%		147%	130%		286%	5%	185%	-23%
Male ATE	23.183	19.059	-4.124	0.847	0.598	-0.249	23.044	-4.175	0.980	-0.559
Std. Err.	[6.512]***	[7.318]***	[9.659]	[0.178]***	[.18]***	[0.251]	[6.477]***	[9.050]	[0.222]***	[0.246]**
Control mean	41.31	61.04	19.73	8.509	9.205	0.696	9.315	24.53	0.889	1.047
ATE as % of mean	56%	31%		130%	79%		247%	-17%	160%	-45%
Female ATE	6.892	21.411	14.519	1.058	1.329	0.271	6.095	10.72	1.234	0.36
Std. Err.	[6.643]	[8.224]***	[10.177]	[.271]***	[.261]***	[.37]	[6.603]	[9.975]	[.329]***	[.345]
Control mean	27.12	25.46	-1.66	7.898	7.715	-0.183	0.501	6.053	1.127	0.0827
ATE as % of mean	25%	84%		178%	265%		1217%	177%	225%	35%
Female - Male ATE	-16.291	2.352	18.643	0.211	0.731	0.520	-16.949	14.90	0.254	0.919
Std. Err.	[9.531]*	[11.058]	[14.317]	[0.321]	[.308]**	[0.441]	[9.625]*	[13.256]	[0.395]	[.423]**
Observations	1996	1861	3857	1996	1861	3857	1996	1529	1992	1529

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.
*** $p<0.01$, ** $p<0.05$,
* $p<0.1$

Table 4: Average treatment effects on income, poverty and employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Monthly employment hours			**Index of wealth**			**Short term expenditures**	**Current position on wealth ladder: 1 to 9**		
	2Y	**4Y**	**4Y - 2Y**	**2Y**	**4Y**	**4Y - 2Y**	**4Y**	**2Y**	**4Y**	**4Y - 2Y**
ATE (All)	21.035	25.355	4.320	0.100	0.202	0.102	7.277	0.369	0.528	0.159
Std. Err.	[6.007]***	[6.764]***	[8.889]	[0.055]*	[.064]***	[0.082]	[2.033]***	[0.087]***	[.1]***	[0.130]
Control mean	120.9	147.0	26.1	-0.0516	0.160	0.2116	51.67	2.727	3.292	0.565
ATE as % of mean	17%	17%					14%	14%	16%	
Male ATE	20.128	18.76	-1.368	0.133	0.179	0.046	6.943	0.474	0.611	0.137
Std. Err.	[7.319]***	[8.262]**	[10.941]	[0.070]*	[.071]**	[0.097]	[2.543]***	[0.106]***	[.116]***	[0.155]
Control mean	133.0	169.9	36.9	-0.0267	0.188	0.2147	53.76	2.750	3.298	0.548
ATE as % of mean	15%	11%					13%	17%	19%	
Female ATE	22.901	38.131	15.23	0.035	0.248	0.213	7.923	0.157	0.368	0.211
Std. Err.	[9.98]**	[11.542]***	[14.905]	[.086]	[.119]**	[.144]	[3.209]**	[.149]	[.187]**	[.238]
Control mean	100.2	109.1	8.9	-0.0986	0.110	0.2086	48.24	2.688	3.287	0.599
ATE as % of mean	23%	35%					16%	6%	11%	
Female - Male ATE	2.773	19.371	16.598	-0.098	0.069	0.167	0.980	-0.317	-0.243	0.074
Std. Err.	[12.165]	[14.081]	[18.334]	[0.109]	[.136]	[0.172]	[4.023]	[0.181]*	[.221]	[0.285]
Observations	1996	1861	3857	1983	1848	3831	1859	1994	1858	3852

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.
*** $p<0.01$, ** $p<0.05$,
* $p<0.1$

Table 5: Grant size per person as treatment

	(1)	(2)	(3)	(4)
	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**
log Grant size pp: adjusted estimate grant size and adjusted estimate group size	0.202	0.137	0.043	0.412
	[0.240]	[0.167]	[0.061]	[0.352]
Female	-0.522	-0.547	-0.070	-0.441
	[0.198]***	[0.143]***	[0.054]	[0.269]
e2	0.137	0.380		
	[0.170]	[0.123]***		
Human capital index	-0.025	0.165	0.172	1.007
	[0.153]	[0.108]	[0.038]***	[0.186]***
Working capital index	0.142	0.315	0.156	0.470
	[0.145]	[0.116]***	[0.038]***	[0.191]**
R-squared	0.141	0.080	0.193	0.197
Control Mean	9.698	8.466	10.65	7.611
Treatment Effect Percentage	0.0208	0.0162	0.00399	0.0541
Obs	1684	1682	811	871

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

Table 6: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Log of real value of business asset stock							Log of real net cash earnings						
	All					Males	Females	All					Males	Females
Treated	1.848	1.878	1.882	1.874	1.870	1.949	1.640	0.940	0.969	0.973	0.980	0.978	0.969	1.053
	[0.200]***	[0.200]***	[0.200]***	[0.200]***	[0.199]***	[0.241]***	[0.337]***	[0.154]***	[0.155]***	[0.154]***	[0.154]***	[0.152]***	[0.178]***	[0.277]***
Treated X Endline 2	-0.770	-0.773	-0.775	-0.789	-0.817	-1.020	-0.486	-0.037	-0.033	-0.054	-0.087	-0.122	-0.331	0.105
	[0.262]***	[0.262]***	[0.262]***	[0.263]***	[0.262]***	[0.320]***	[0.426]	[0.211]	[0.212]	[0.212]	[0.212]	[0.210]	[0.248]	[0.359]
Endline 2	0.885	0.884	0.883	0.875	0.882	1.042	0.663	0.383	0.382	0.376	0.390	0.400	0.714	-0.096
	[0.166]***	[0.166]***	[0.166]***	[0.166]***	[0.166]***	[0.204]***	[0.256]***	[0.140]***	[0.140]***	[0.140]***	[0.140]***	[0.140]***	[0.167]***	[0.220]
Female	-0.442	-0.412	-0.406	-0.401	-0.363			-0.731	-0.671	-0.694	-0.736	-0.660		
	[0.123]***	[0.126]***	[0.124]***	[0.123]***	[0.127]***			[0.102]***	[0.105]***	[0.105]***	[0.103]***	[0.106]***		
Treated X Vocation	-0.309	-0.373	-0.367	-0.149	-0.076	-0.038	-0.340	-0.858	-0.806	-0.820	-0.738	-0.717	-0.830	-0.386
	[0.405]	[0.401]	[0.403]	[0.387]	[0.393]	[0.436]	[1.189]	[0.368]**	[0.364]**	[0.366]**	[0.360]**	[0.357]**	[0.382]**	[1.274]
Vocation	0.921	0.962	0.953	0.944	0.886	1.012	0.521	0.755	0.778	0.777	0.770	0.708	0.934	0.147
	[0.253]***	[0.250]***	[0.252]***	[0.250]***	[0.254]***	[0.295]***	[0.686]	[0.225]***	[0.220]***	[0.221]***	[0.222]***	[0.222]***	[0.249]***	[0.500]
Treated X Working capital index	-0.434				-0.469	-0.486	-0.406	-0.195				-0.180	-0.180	-0.116
	[0.146]***				[0.248]*	[0.267]*	[0.455]	[0.101]*				[0.176]	[0.216]	[0.313]
Working capital index	0.428				0.435	0.455	0.479	0.517				0.449	0.437	0.482
	[0.136]***				[0.169]**	[0.168]***	[0.317]	[0.097]***				[0.126]***	[0.159]***	[0.195]**
Treated X Human capital index		-0.315			-0.034	0.185	-0.459		-0.179			-0.140	-0.111	-0.142
		[0.130]**			[0.213]	[0.267]	[0.380]		[0.100]*			[0.173]	[0.218]	[0.327]
Human capital index		0.270			0.035	-0.104	0.268		0.334			0.181	0.261	0.060
		[0.116]**			[0.141]	[0.192]	[0.209]		[0.095]***			[0.123]	[0.165]	[0.192]
Treated X Patience index			-0.261		0.283	0.205	0.229			-0.099		0.321	0.372	0.288
			[0.132]**		[0.262]	[0.316]	[0.469]			[0.102]		[0.217]	[0.258]	[0.403]
Patience index			0.514		0.260	0.345	0.197			0.486		0.245	0.150	0.346
			[0.149]***		[0.187]	[0.238]	[0.286]			[0.122]***		[0.155]	[0.192]	[0.249]
Treated X Risk Aversion Index				-0.375	-0.205	-0.213	0.102				-0.216	-0.215	-0.189	-0.321
				[0.133]***	[0.280]	[0.354]	[0.457]				[0.104]**	[0.236]	[0.281]	[0.417]
Risk Aversion Index				0.570	0.512	0.478	0.372				0.406	0.431	0.356	0.467
				[0.192]***	[0.234]**	[0.289]*	[0.400]				[0.176]**	[0.210]**	[0.248]	[0.362]
R-squared	0.168	0.165	0.168	0.167	0.173	0.192	0.147	0.095	0.091	0.091	0.089	0.101	0.078	0.119
Obs	3850	3850	3848	3816	3815	2541	1274	3846	3846	3845	3812	3812	2538	1274
Control Mean	9.698	9.698	9.698	9.698	9.698	9.698	9.698	8.466	8.466	8.466	8.466	8.466	8.466	8.466

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

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

Table 7: Investments and performance by group characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	2 Year Endline						4 Year Endline				Pooled Endlines			
	Hours of training received		Log of real value of business asset stock		Log of real net cash earnings		Log of real value of business asset stock		Log of real net cash earnings		Log of real value of business asset stock		Log of real net cash earnings	
	Treated	All	Treated	All	Treated	All	Treated	All	Treated	All	Treated	All	Treated	All
Treated		217.230		2.923		0.769		1.453		0.817		2.556		0.803
		[91.128]**		[0.758]***		[0.598]		[0.637]**		[0.608]		[0.537]***		[0.448]*
Treated × Group existed prior to YOP		84.454		-0.535		0.238		-0.128		0.045		-0.309		0.156
		[47.908]*		[0.389]		[0.305]		[0.345]		[0.308]		[0.273]		[0.222]
Group existed prior to YOP (indicator)	77.426	1.613	-0.077	0.323	0.075	-0.151	0.131	0.279	0.291	0.137	0.031	0.284	0.206	-0.001
	[36.270]**	[22.926]	[0.282]	[0.238]	[0.193]	[0.202]	[0.215]	[0.208]	[0.180]	[0.197]	[0.189]	[0.166]*	[0.135]	[0.145]
Treated × Group dynamic index		23.484		0.550		0.271		0.209		0.298		0.408		0.297
		[17.883]		[0.168]***		[0.132]**		[0.172]		[0.147]**		[0.123]***		[0.101]***
Group dynamic index	-9.013	-16.491	0.246	-0.208	0.003	-0.210	0.021	-0.057	-0.003	-0.248	0.154	-0.145	-0.000	-0.236
	[16.257]	[8.954]*	[0.133]*	[0.123]*	[0.093]	[0.106]**	[0.117]	[0.131]	[0.096]	[0.117]**	[0.093]*	[0.093]	[0.070]	[0.080]***
Treated × Group size		2.340		-0.029		0.015		-0.010		0.011		-0.019		0.011
		[3.223]		[0.028]		[0.022]		[0.026]		[0.022]		[0.020]		[0.016]
Group size	-3.047	-1.949	-0.026	-0.001	0.007	0.000	-0.006	0.010	-0.010	-0.008	-0.016	0.001	-0.002	-0.004
	[2.664]	[1.482]	[0.020]	[0.019]	[0.014]	[0.015]	[0.020]	[0.018]	[0.015]	[0.015]	[0.015]	[0.014]	[0.011]	[0.011]
Treated × % of group female		205.646		-0.745		-0.827		-0.081		-0.453		-0.474		-0.603
		[85.771]**		[0.758]		[0.563]		[0.637]		[0.595]		[0.519]		[0.421]
% of group female	92.267	-92.868	-0.359	0.557	-0.606	0.248	-0.889	-0.679	-0.260	0.352	-0.543	-0.014	-0.457	0.270
	[87.134]	[44.611]**	[0.597]	[0.493]	[0.396]	[0.413]	[0.445]**	[0.414]	[0.404]	[0.388]	[0.391]	[0.342]	[0.292]	[0.288]
Treated × Group heterogeneity index		-36.984		-0.236		-0.209		-0.238		0.037		-0.253		-0.093
		[23.535]		[0.189]		[0.151]		[0.166]		[0.139]		[0.128]**		[0.103]
Group heterogeneity index	-28.215	-3.994	-0.006	0.142	-0.095	0.077	-0.237	-0.136	0.002	-0.075	-0.127	0.013	-0.041	0.012
	[19.557]	[11.104]	[0.126]	[0.139]	[0.097]	[0.117]	[0.119]**	[0.117]	[0.093]	[0.113]	[0.089]	[0.092]	[0.071]	[0.081]
E2											0.212	0.896	0.359	0.384
											[0.159]	[0.165]***	[0.119]***	[0.139]***
Treated x E2												-0.772		-0.016
												[0.261]***		[0.210]
Inequality of grant distribution	-12.565		0.130		-0.100		-0.278		-0.100		-0.062		-0.115	
	[16.958]		[0.130]		[0.094]		[0.115]**		[0.075]		[0.093]		[0.062]*	
Level of leader capture	-0.947		0.401		0.069		-0.159		-0.257		0.119		-0.143	
	[49.886]		[0.445]		[0.334]		[0.353]		[0.268]		[0.310]		[0.215]	
R-squared	0.141	0.277	0.158	0.180	0.116	0.097	0.189	0.192	0.123	0.128	0.136	0.168	0.095	0.100
Obs	968	1979	968	1978	968	1978	915	1847	913	1843	1883	3825	1881	3821
Control Mean	48.94	48.94	9.236	9.236	8.285	8.285	10.17	10.17	8.653	8.653	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 baseline measures of employment and human and working capital.
**** p<0.01, ** p<0.05, * p<0.1*

Table 8: 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.
**** p<0.01, ** p<0.05, * p<0.1*

Table 9: A comparison of experimental and non-experimental associations between income and social outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Family cohesion outcomes (Z score), t1	**Family cohesion outcomes (Z score), t2**	**Community participation outcomes (Z score), t1**	**Community participation outcomes (Z score), t2**	**Community public good contributions (Z score), t2**	**Electoral participation outcomes (Z score), t2**	**Anti-social behavior and dispute outcomes (Z score), t1**	**Anti-social behavior and dispute outcomes (Z score), t2**	**Anti-social behavior and dispute outcomes extended (Z score), t2**	**Protest attitudes and participation (Z score), t2**
Treated	0.023	0.002	0.064	0.092	0.013	0.095	-0.220	0.024	-0.019	-0.022
	[0.066]	[0.060]	[0.060]	[0.069]	[0.070]	[0.062]	[0.065]***	[0.061]	[0.064]	[0.063]
Treated X Female	-0.161	0.124	0.008	-0.257	-0.136	-0.222	0.376	0.010	-0.002	-0.034
	[0.124]	[0.120]	[0.110]	[0.109]**	[0.112]	[0.121]*	[0.104]***	[0.105]	[0.114]	[0.124]
Wealth index change (t1 - t0)	0.054		0.108				-0.004			
	[0.029]*		[0.027]***				[0.024]			
Wealth index change (t2 - t0)		0.050		0.100	0.059	0.039		-0.014	-0.031	0.022
		[0.028]*		[0.024]***	[0.026]**	[0.026]		[0.022]	[0.023]	[0.025]
Log net earnings change (t1 - t0)	0.008		0.037				0.028			
	[0.011]		[0.009]***				[0.009]***			
Log net earnings change (t2 - t0)		-0.002		0.023	0.036	0.017		0.028	0.037	0.016
		[0.009]		[0.009]**	[0.009]***	[0.009]*		[0.007]***	[0.008]***	[0.009]*
Log employment hours change (t1 - t(	-0.009		-0.004				0.008			
	[0.010]		[0.009]				[0.010]			
Log employment hours change (t2 - t(		-0.004		-0.035	-0.024	-0.016		0.004	0.008	-0.015
		[0.010]	`	[0.009]***	[0.009]**	[0.010]		[0.009]	[0.009]	[0.010]
Wealth index, t0	0.005	-0.031	0.082	0.029	0.006	-0.012	0.029	0.004	0.015	0.068
	[0.036]	[0.038]	[0.033]**	[0.031]	[0.032]	[0.038]	[0.029]	[0.029]	[0.031]	[0.036]*
Log net earnings, t0	0.015	-0.001	0.037	0.033	0.029	0.034	0.022	0.045	0.040	0.013
	[0.015]	[0.015]	[0.013]***	[0.013]**	[0.014]**	[0.013]***	[0.013]*	[0.014]***	[0.013]***	[0.015]
Log employment hours, t0	-0.005	-0.020	0.009	-0.016	-0.004	-0.034	0.016	0.005	0.015	-0.019
	[0.015]	[0.015]	[0.015]	[0.014]	[0.015]	[0.016]**	[0.015]	[0.013]	[0.013]	[0.015]
Female	-0.163	-0.348	-0.386	-0.263	-0.102	-0.108	-0.094	0.090	0.136	0.030
	[0.073]**	[0.079]***	[0.071]***	[0.073]***	[0.071]	[0.080]	[0.071]	[0.067]	[0.077]*	[0.077]
Constant	-1.532	-1.349	-1.241	-0.631	-1.916	-5.359	-1.520	-1.567	-0.880	-2.108
	[0.933]	[1.030]	[0.894]	[1.042]	[0.969]**	[1.143]***	[0.914]*	[0.955]	[0.936]	[1.036]**
Observations	2,329	1,838	2,329	1,838	1,838	1,838	2,329	1,838	1,838	1,838
R-squared	0.136	0.164	0.230	0.199	0.146	0.157	0.155	0.068	0.079	0.076
e2 Female TE Std Error Star		[.107]		[.09]*	[.093]	[.108]		[.089]	[.095]	[.1]
e2 Female TE coeff		0.126		-0.164	-0.123	-0.126		0.0342	-0.0207	-0.0559
e2 Female TE Pval		0.240		0.0677	0.187	0.242		0.700	0.827	0.576
e2 Female TE Std Error		0.107		0.0896	0.0929	0.108		0.0889	0.0947	0.0999
e1 Female TE Std Error Star	[.11]		[.096]				[.088]*			
e1 Female TE coeff	-0.138		0.0723				0.156			
e1 Female TE Pval	0.213		0.453				0.0770			
e1 Female TE Std Error	0.110		0.0963				0.0878			

Robust standard errors in brackets, cl

Omitted regressors include an age quartic, district indicators, and baselin

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